THE GENESIS OF LACHMANN'S METHOD
The Genesis of Lachmann’s Method

SEBASTIANO TIMPANARO

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Sebastiano Timpanaro (1923–2000) was one of the most important Italian intellectuals of his generation and a major figure in twentieth-century Classics, history, and Marxist and Freudian theory. He is not as well known in England and America as he deserves to be, however, in part because of the seeming disparity of his interests, in part because of the very unconventionality and originality of his views, and in part because traditionally it has been from other European countries, especially France and Germany, that thinkers have more often been translated into English (if only because traditional academic Italian, a language in which Timpanaro fortunately did not write, is singularly resistant to attempts at rendering into English). I hope the present edition will serve not only to facilitate study of those crucial aspects of the history of Classics and of the theory and practice of textual editions that his book presents so clearly, but also to introduce to a somewhat wider audience the thought of a quietly brilliant, soberly passionate, and profoundly serious European intellectual.

Sebastiano Timpanaro was born into a family that combined a deep dedication to intellectual pursuits, especially to the history of science, with a no less deep commitment to leftist politics. His father, Sebastiano Timpanaro Sr. (1888–1949), was born in the small town of Tortorici, near Messina in Sicily, but then went north to study physics at the universities of Naples and then Bologna. His promising career as an assistant in experimental physics at the University of Parma was cut short in 1939 by his categorical refusal to take the oath of allegiance that the Fascist government required of all Italian state employees; he found work first in a private school in Florence, where he taught mathematics and physics for a number of years, and then—in the meantime he had been obliged to swear the loyalty oath after all—at the Domus Galilaeana, a research institute in the history of science in Pisa, which
he directed from 1942 until his death. Sebastiano Timpanaro's mother, Maria Cardini (1890–1978), was born in Arezzo and took a degree in Greek literature at the University of Naples; in 1914 she studied briefly in Berlin with two of the greatest Hellenists of the past centuries, Ulrich von Wilamowitz-Moellendorff and Hermann Diels. After a brief period as a Dadaist poet (for several years she corresponded frequently with Tristan Tzara), around 1920 she abandoned poetry forever and returned to her Greek studies. She acquired considerable recognition, not only in Italy, for her editions, translations, and studies of ancient Greek philosophy and science, especially on the topic of the Pythagoreans, but she remained for her whole career a teacher in junior high schools, first in Parma (where she met Sebastiano Timpanaro Sr.), then at the same private school in Florence as her husband, finally in Pisa. After the end of the Second World War, she became actively involved in local politics in Pisa for the Italian Socialist Party, campaigning especially for the establishment of non-Church nursery schools in the city.

Sebastiano Timpanaro was much attached to his parents, even by Italian standards—he published a selection of his father's papers with a preface by himself in 1952 (Timpanaro Sr. 1952), and the very last essay he wrote was the lengthy introduction to a collection of his mother's works that he edited, which was published after her death (Cardini 2001). From them he inherited interests and characteristics that marked all his work and his whole life: an uncompromising commitment to intellectual honesty and moral rigor; an unwavering dedication to an ideal of rationality, as it is expressed for example in the progress of the natural sciences, with the same time a painful recognition of the brutal irrationality of much of human history, especially in national and international politics; an exceptionally broad multidisciplinary and multilingual culture, with a particular fascination for French and German literature and intellectual history; a systematic preference for discussing current intellectual issues in terms not of the contemporary epigones who set the passing fashions but of the seminal thinkers who first set the fundamental questions; a constitutional inability to compromise; an almost morbid tendency to exacerbate disagreement to the point of irreparable rupture; an unmistakable tone in all his writings, above all strictly clear and precise, often austere, occasionally severe, but with a sprinkling of colloquialisms and, rarely, metaphors, unashamedly and sometimes rather ponderously didactic, free of any false rhetorical pathos and of even the slightest trace of wit or humor for their own sake.

Timpanaro was born in Parma and moved with his parents to Florence. In the university of that city he had the good fortune to be able to study Classics with Giorgio Pasqui (1885–1952), the greatest Italian Classicist of the twentieth century and a crucial figure in the sometimes difficult mediation between the German and the Italian national traditions of scholar-ship during that period. In Pisa Pasqui also taught at the elite Scuola Normale Superiore, and once Timpanaro's family moved there he was introduced into the intense intellectual atmosphere of that unique institution of scholarly research and teaching and into the diminutive, provincial, but culturally and politically very animated town in which it is located. It was also at Pisa that Timpanaro studied with Eduard Fraenkel, one of the greatest German Classicists of the century, who came to teach at the Scuola Normale regularly for a number of years after Pasqui's death. Under their guidance Timpanaro laid the scholarly foundations for his later work on Classical literature, especially on Latin poetry (above all in the highly technical disciplines of textual criticism and of microexegesis and lexical studies) and on the history of scholarship on Latin poetry during antiquity. In both of these fields, Timpanaro made numerous significant and lasting contributions (collected in Timpanaro 1978, 1986, 1994a, and 2001a), though he always preferred the form of the small, astonishingly erudite, highly condensed philological note to that of the expansive literary monograph and though he never himself undertook the full-scale editions of such authors as Ennius and Virgil that his teachers had hoped he would do.

On the basis of these studies, Timpanaro was widely regarded, at home and abroad, as one of the very few most highly esteemed Italian Classical scholars of his time. Yet he never became a regular professor at any Italian university (toward the very end of his life, he taught a few times at the University of Florence as a visiting professor); after some years of teaching in secondary schools near Pisa, he worked from 1960 until his retirement in 1983 as a proofreader at a Florentine publishing house, La Nuova Italia. What exactly the reason (or, likelier, the complex set of partial reasons) was for this seeming anomaly—an anomaly that was perhaps less extraordinary in Italy, where intellectual life has never been confined to the universities, than it would have seemed in some other countries—was always far from clear, even to those who knew him best. Some have suggested that the imperfect Italian university system was not capable of recognizing, or of accepting, his merits, or alternatively that he himself was not willing to compromise himself by entering into its institutional ambiguities. It should also be borne in mind that, in remaining aloof from direct participation in the university (though many of his closest friends were university professors), he was able to continue a family tradition set (though for different reasons) by his parents, to devote his free time entirely to scholarly research and publication without becoming embroiled in university administration and examinations, and to direct his pedagogical activity to friends and to anonymous readers rather than to the physically present, always unpredictable, sometimes rather unruly students who often seemed to inspire in him a degree of diffidence bordering on dread.
Timpanaro's focus on the most technical aspects of Classical scholarship and his confidence that Classics was a scientific discipline comparable, at least in certain regards, to the natural sciences did not mislead him into supposing that we might be capable of having a direct, unmediated relation to antiquity or into neglecting the history of Classical scholarship as though it were nothing more than an accumulation of superseded errors. On the contrary, from the beginning of his studies he paired the scrupulous investigation of ancient literary texts with the no less scrupulous investigation of those scholars who had investigated them before him, both in antiquity and in modern times; the interpretative variations of a single text along the line of the history of its reception helped enrich it with unexpected and not always absurd new meanings; the relative stability of that text provided a fixed point that permitted the historical variety of its attested understandings to be ordered and rendered meaningful. Timpanaro's first book, dedicated to his parents, was a study of Giacomo Leopardi's contributions to Classical studies (Timpanaro 1954, 1972a, 1972b); in it he demonstrated once and for all that Leopardi, so far from being nothing more than a self-absorbed Romantic poet obsessed only with his personal tribulations, was a Classical scholar of European rank who was not only deeply inspired by his reading of Greek and Latin texts but also capable of interpreting and extending them along with the very best of his contemporaries. This first book, which has remained the standard work on the subject, was followed by an edition of Leopardi's philological writings (Leopardi 1969) and by a number of other studies in modern literary and intellectual history, mostly devoted to nineteenth-century Italian writers (collected in Timpanaro 1965, 1969, 1980a, 1982, 1984a, 1994b). Although Timpanaro modestly defined himself once as nothing more than "a scholar of nineteenth-century cultural history who comes from Classical philology" (Brascaglì and Tellini 1996: 15), in fact his contribution to the way in which Italians understand their own intellectual history has been extremely influential. In particular, Timpanaro's insistence on the Classical component of much early nineteenth-century Italian writing has brought to prominence the continuities between a liberal strain of eighteenth-century Enlightenment thinking and a number of nineteenth-century writers who had previously been superficially pigeonholed as Romantic and were often discounted as conservatives or anti-nationalists. In so doing, he has helped reestablish an important link of continuity in European cultural history between the eighteenth and nineteenth centuries.

Timpanaro's concentration on nineteenth-century figures should not mislead: it was rather with certain aspects of the eighteenth century, especially with those that seemed to him to point to positive future developments or that had been unjustly neglected, that he himself evidently felt his deepest affinity. In particular, Timpanaro developed a sophisticated yet highly personal philosophical position that reached back to such ancient precursors as Epicurus and, above all, Lucretius but that would have been most at home in the eighteenth century. It is no accident that he chose to embody his views not only in the form of a collection of essays titled On Materialism (Timpanaro 1970, 1972a, 1997b; English translation, Timpanaro 1972b, 1980b, 1996) but also in that of a translation of F. Thiry d'Hollbach's 1772 treatise Le bon sens (Good sense; d'Hollbach 1853), to which he appended Voltaire's observations on the text and also provided an extensive introduction of his own, which explored the text in both historical and systematic perspectives. At the cost of a certain degree of oversimplification, we may summarize Timpanaro's philosophy under the headings of materialism, hedonism, atheism, and pessimism. As a materialist, Timpanaro was firmly opposed to any Idealistic idolatry of the supposed autonomy and freedom of the human agent. Instead he insisted on the total physical and biological determination of all human phenomena, severely criticizing on this account, among many others, the later Freud (Timpanaro 1974, 1975c, 1992; English translation, Timpanaro 1976a), almost all twentieth-century Marxists, and even Marx himself, whom he accused of lapsing frequently into Idealistic humanism and to whom he preferred Engels. Against the various forms of Idealism that, in such figures as Benedetto Croce (1866–1952), dominated Italian philosophy through most of the twentieth century, Timpanaro looked back to Leopardi as the model for an enlightened materialism—and also for an enlightened pessimism with which it was intimately linked. For, in this view, an essential part of the biological apparatus that makes up human beings as well as all other organisms is the unrelenting search for pleasure. But given that there is no God (a conclusion he derived from the failure of all attempted theodicies), it is certain that nature has not been constructed with a view toward us, so as to fulfill our ends, and hence the hedonistic desire for the satisfaction of our desires must inevitably be often frustrated—systematically, painfully, irremediably. Those who knew Timpanaro well—the oscillations in his moods between irony and despair; his intermittent bursts of intense joy at the pleasures of friendship or of scholarship; his unfailing, angular courtesy—never doubted that his pessimism was not only a well considered and carefully argued philosophical position but also a deeply ingrained way of life.

Nonetheless, Timpanaro was also for his whole life a passionately engaged militant on the far left wing of Italian politics. He began, soon after the end of the Second World War, as a member of the PSI (Partito Socialista Italiano, or the Italian Socialist Party), like both his parents; he shared with them a lifelong hostility to both the Christian Democrats, who, overtly Catholic and with the support of the Catholic Church, governed Italy uninte-
ruptedly for the first two decades after the war, and the Italian Communist Party, which he regarded as covertly theological in structure, ideology, and appeal—opium for the masses. But it has never been easy, in Italy or elsewhere, to be both left wing and principled, and at the same time to wish to attain actual political power. When the Italian Socialists decided in 1964 to abandon the opposition and to join the government in a coalition with the Christian Democrats, Timpanaro and his mother, like many other Socialists who reacted against what they considered a betrayal of their founding principles, became active members in the newly founded PSIUP (Partito Socialista Italiano di Unità Proletaria), the original name of the PSI, which splintered off from the left wing of the PSI. The PSIUP dissolved when it failed dismally in the general elections of 1972, and Timpanaro joined a successor party, the PDUP (Partito di Unità Proletaria); but the PDUP, despite coalescing with other small left-wing groups, was crushed in its turn in the elections of 1976. Timpanaro resigned from the PDUP in that year and never again joined any other political party on offer in Italy’s alphabet soup of left-wing acronyms. But until the end of his life he continued to write frequently in newspapers, journals, and private letters about specific political issues and about the tensions and contradictions in the Italian left—an ample, indeed inexhaustible subject for reflection, which, in his last years, he seasoned with a cautious rapprochement with environmentalist positions (Timpanaro 2001b). Timpanaro’s gauzy, well-dressed figure, with his high forehead and piercing eyes, was a familiar sight not only at scholarly conferences and seminars, where he was received by colleagues and students with a respect bordering on deference, but also at political demonstrations, where a vague sense of his scholarly renown and institutional marginality surrounded him with an aura of ascetic purity, indeed almost of saintliness. He must have been a formidable political opponent, erudite, eloquent, drastically and unrelentingly polemical, limited only by an intransigent incapacity to compromise and perhaps a certain remoteness from the lived realities (as opposed to the theoretical study) of politics and economics.

Oddly, the only books of Timpanaro’s that have been translated into English before now are his collection of essays On Materialism, in which he attacks European Marxism for having betrayed the legacy of genuine materialism bequeathed by Engels, and French Structuralism for having misunderstood and misapplied the linguistic theories of Saussure (Timpanaro 1975b, 1980b, 1996), and The Freudian Slip, his sustained polemic against the later Freud and Freudian psychoanalysis in general for having abandoned the materialist insights of Freud’s early writings, and against the psychoanalytic explanation of so-called Freudian slips in particular, which in most cases, based on his conception of materialism and his experience of textual criticism (and proofreading), he attributes instead to exactly the same kinds of mechanical processes as result so often in transcribing manuscripts (Timpanaro 1976a). A few articles, mostly on related themes, have also been translated (Timpanaro 1976b, 1977b, 1979, 1984b, 1988). The result is that Timpanaro is known in the English-speaking world above all as a materialist philosopher, Leftist theoretician, and critic of psychoanalysis. These are, of course, important aspects of his vast and diverse production—but it can be argued that they are the ones of least permanent value, in part because they were tied closely to what appear now to have been largely ephemeral modes of Leftist politics and thought (already in the 1990s Timpanaro ruefully confessed that On Materialism had come to seem to him like “a fossil”; Timpanaro 1994b: xi), in part because philosophy was in fact not Timpanaro’s greatest strength. Instead it is his historical and philosophical studies that seem to me to represent his most lasting contribution to Western culture. The present translation of The Genesis of Lachmann’s Method will give a wider circle of readers the opportunity to enrich their understanding of this profound and enigmatic figure.

Timpanaro’s book examines the historical development and the systematic limits of a particularly significant moment—the attempt during the nineteenth century to increase the degree of rationalization, standardization, and professionalization, within the evolution of a process that for millennia has been central to human culture—the transmission of written texts. It is only if his argument is placed within this larger horizon that its full import can be understood.

A written record has this advantage over an oral utterance—that it lasts in time beyond the moment of expression, in a physical form independent of the speaker's and listeners' memories. Yet this physical form too has its limitations, for it is restricted to a single spatial location and must be entrusted to an ultimately perishable medium to bear it. For the one reason or the other—either because the existing copy no longer suffices for the new, spatially dispersed uses to which it is now to be put (usually, new readers), or because it has become damaged over time (by overuse, inadequate materials, or simple old age)—it may become desirable to produce new copies of written texts. Before the age of photographs, photocopies, and scanners, which copy texts by purely mechanical processes simply on the basis of the contrast between lighter areas and darker ones, the only way to produce new copies was to transcribe them from old ones, element for element, most often semantic unit for semantic unit. If greater accuracy of transmission was required, this could be done visually, by a scribe copying onto one new medium the text he saw before his eyes (but the disadvantage was the smaller
number of copies that could thereby be produced at the same time from a single exemplar). If, on the other hand, a large number of copies was sought after, an acoustic procedure could be preferred whereby the exemplar was read out before a group of scribes, who listened to it and copied down, each onto his own medium, what they thought they had heard (at the cost of greater inaccuracy, due to homonyms, distraction, noise, and other forms of interference). It is only a guess, but probably a good one, that for most of the history of human culture the normal situation was one that began with a single exemplar to be copied (the source text) and ended up, as result and usually as purpose, with more than one copy of the text (the source text plus the target text, or multiple target texts); transmission normally entailed multiplication. And given that the procedure was performed neither by machines nor by gods but by humans, and that human transmission always entailed error, and multiplication of copies usually entailed proliferation of errors.

To have only a single source greatly simplified the copyist's task: he (it was of course usually a he) could attempt to transcribe it as faithfully as he or his supervisors wished, intervening into the text as he saw fit, so as to correct obvious errors or to effect what he considered to be improvements of various sorts. But what was he to do when he had available two source texts? Given the proliferation of errors, these were bound to differ from one another in their readings, at least occasionally, if they were of any considerable length. On what basis was he to choose which reading to put into the target text? However rarely such a situation occurred—and presumably for many centuries it did not occur frequently except in the largest scriptoria, monasteries, and libraries—it must have happened regularly enough for a certain set of rule-of-thumb criteria of choice to develop: whichever seemed to be the grammatically or semantically or logically better reading would be preferred from case to case, or both readings could be imported into the target text with or without an expression of greater authorization for one of them. The next step methodologically will have been to give a general preference to the one source text over the other available one whenever possible, either suppressing apparently equipollent readings in the latter or indicating them as inferior alternatives. This will have simplified the copyist's work, freeing him from the obligation to use his brain to choose among variants from case to case and, in effect, reducing once again the number of source texts. But at this point a new question arose: on what basis was the copyist to choose which one of the available sources he was to prefer? Over the centuries, various contradictory criteria were developed, each with its own partial and spurious justification: the oldest manuscript, the most legible manuscript, the one that appeared to have the most good readings, the one that had the fewest corrections, the one that had the most corrections, the one that derived from an authoritative provenance, the one that was closest to hand; and so forth. And of course even then the copyist (or his corrector) was still free as he saw fit to make whatever he thought were corrections and other improvements.

The advent of printing in the fifteenth century altered various parameters of the process of textual transmission but at first had no effect whatsoever on these methodological issues. Printing vastly multiplied the number of target texts that could be made on the basis of a single source text and created a greater degree of textual identity (though, especially at first, by no means complete identity) among those target texts. But printing did not in itself require people to change fundamentally the methods of textual transmission, and indeed for several centuries printers, correctors, and editors continued to use most of the same rule-of-thumb criteria that their predecessors, the ancient and medieval copyists, had developed. What changed matters most was instead the concatenation of three factors during the period from the fifteenth to the nineteenth centuries: the vast increase in the number of manuscripts, Greek and Latin, that became available throughout Europe during and after the Renaissance; the gradual concentration of the holdings of libraries no longer in a large number of small collections (each of which might have one manuscript of Cicero) but, more and more, in a small number of large collections (each of which might have dozens); and the general increase in the ease of communication and travel over the course of the early modern period. The result was that eventually there was no longer a scarcity of potentially available source texts from which further copies could be derived but an impressive, indeed intimidating, overabundance—Montfaucon's Palaeographia graeca (1708) already lists more than 13,000 Greek manuscripts. Since the fifteenth century, printers and editors had tended by force of inertia to copy their own texts from those printed by their predecessors, correcting them by the lights of their ingenuity where they seemed in error or adding for comparison some one manuscript (or a very few manuscripts), almost always one (or ones) that had the advantage of proximity or the appearance of old age. As the number of available manuscripts proliferated, further criteria for preferring one reading over another were developed—the better reading was the one to be found in the most manuscripts, or in the oldest ones, or in the oldest one of all—but a rational justification for such criteria was neither provided nor available. At least from the lofty perspective of a nineteenth-century Classical philology anxious to establish its credentials as a serious science, this was obviously an unsatisfactory state of affairs.

The first attempt to provide a thoroughly mechanical and systematic procedure for rationalizing and standardizing the choice among manuscripts, and hence among readings, was developed during the nineteenth century
and since the beginning of the twentieth century has been known as Lachmann's method because of its association with Karl Lachmann (1793–1851), a German Classicist who produced celebrated editions of texts in Latin, Greek, and medieval and modern German. Lachmann’s method is genealogical and mechanical in nature and aims at providing a standardized, rational procedure for editing texts on the basis of multiple manuscripts without requiring that the editor use his personal judgment in order to choose among variant readings. Its goal is to determine the filiation of manuscripts, that is, to ascertain which ones have been copied from which other ones; given that every act of transcription is likely to introduce new errors, a manuscript B, if it has been copied mechanically from a manuscript A, will have all the errors that A had (if it does not have all of them, then it has probably corrected some of them during the transcription and hence is likely not to have been copied mechanically after all), and it is also likely to have at least one new error of its own. If this can be shown to be the case, then B can be discarded for the purposes of the constitution of the text it shares with A, since B, compared with A, brings no new information that is not erroneous. Lachmann's method is mechanical, both in the sense that it must presuppose the unthinking transcription of manuscripts if it is to be applied to them and in the sense that the determination of relations of filiation is achieved on the basis of simple rules and calculations of probability. Ideally, choices of manuscripts and of readings based on this method will be rational in that they will depend not on the taste of the individual scholar but on objective evidence that can be mathematized and evaluated; hence they will be capable of becoming standardized, because any scholar, young or old, inexperienced or expert, should on principle come up with exactly the same results if he is given the same evidence. Lachmann information. We may interpret Lachmann's method as a defensive reaction to the proliferation of possible source texts, intended to reduce them to a more manageable number, and can identify it as one important element in the professionalization of Classics during the nineteenth century, since it established rules that all who wished to be recognized as full members of the discipline could be expected to follow so as to produce uniform and hence generally acceptable results.

Timpanaro’s book examines Lachmann’s method in three regards: (1) What was its origin, and, in particular, did Lachmann invent it? (2) What was its subsequent development and outcome? and (3) How valid is it? These three questions determine the structure of his book.

(1) In chapters 1 to 7 and Appendix A, Timpanaro considers the development of the methods of textual edition from the fifteenth century through the middle of the nineteenth century and inquires into the origin of the various features that go to make up Lachmann's method. Part of the genius of Timpanaro’s study is that he applies the very same technique of genealogi-

cal investigation to Lachmann’s method that characterizes the method itself. The aims of Timpanaro's analysis are set out with exemplary clarity in the second paragraph of his introduction, and its results are summarized tersely and lucidly in chapter 7. These conclusions are the following: that many of the techniques and presuppositions associated with the method were well established either among the Renaissance Humanists or during the following centuries, especially among the New Testament scholars of the eighteenth century; that the particular formulation of the method associated with Lachmann’s celebrated edition of Lucretius (Lachmann 1856) was anticipated in most of its essentials by such other scholars as Orelli, Zumpt, Ritschl, and Madvig in the decades before the publication of this work, and in particular almost in its entirety in Bernays’s work on Lucretius of 1847 (and to a lesser extent in Purmann’s of 1846); and that Lachmann’s own application of the method, not only in his earlier editions but even in his great Lucretius edition, was generally inconsistent and was marked by fundamental errors. In short, Timpanaro demonstrated, once and for all, that “Lachmann’s method” was not in fact Lachmann’s method (for he did not invent it) and that Lachmann’s method was not in fact “Lachmann’s method” (for he did not apply it consistently). Since the first publication of Timpanaro’s study, scholars who use the term “Lachmann’s method” without quotation marks have done so at their peril.

(2) In chapter 8, Timpanaro sketches a brief history of the decline of Lachmann’s method between the second half of the nineteenth and the beginning of the twentieth century. His focus is on the analogies that were perceived during that period between the developments in Classical textual editing on the one hand and those in comparative historical linguistics on the other: a first period in the middle of the nineteenth century in which a version of Lachmann’s method was applied, with euphoric hopes for success, to the search for genealogical relations within the Indo-European family of languages was followed in the last decades of that century by a wave of disillusionment and skepticism in both disciplines. The discovery that the method was fully applicable only in a relatively small number of cases led some textual critics to misapply it by artificially reducing the number of witnesses they took into consideration and hypothesizing filiations on the basis of inadequate evidence or none at all, and others to emphasize instead the importance of those many situations in which the method could not be applied safely because target texts had not been copied mechanically and exclusively from single sources. By the beginning of the twentieth century, the general reaction against Positivism in all the human sciences had led to a widespread distrust of Lachmann’s method in such Classicists as Pasqui, to whom the closing section of the chapter is dedicated.

(3) What then is the lasting value of Lachmann’s method, and what are
the limits of its validity? It is to these questions that chapter 8 is partly and
Appendices B and C are fully addressed (as is also the posthumous essay,
which I have titled “Final Remarks on Bipartite Stemmata” and which ap-
pears here for the first time). They largely take the form of a long drawn-out
argument with Paul Maas (1880–1964), who provided the most authorita-
tive statement of the principles of Lachmann’s method in his lapidary Text-
kritik (Maas 1958 [1927]). Against Maas’s almost mathematical forma-
tion of the principles of mechanical textual edition, Timpanaro insists on
the infinite diversity of human error and in particular on three well-attested
and universally recognized ways in which manuscripts can differ from
one another without the differences being explicable in terms of me-
chanical copying: contamination (when scribes make use of more than one
source text and on the basis of their own judgment mix readings from the
one with readings from the other); scribal conjecture (when copyists deliber-
ately make corrections of their own in the text they are copying because, for
one reason or another, they are dissatisfied with the source text’s readings);
and polygeny (when the same errors are produced entirely independently
in different transmission processes, either by chance or because under cer-
tain circumstances certain kinds of errors occur with greater proba-
bility).

As Timpanaro’s investigation of the limits of Lachmann’s method pro-
ceeds, it becomes more and more focused on one highly technical but very
important issue: why is it that so many textual traditions seem to take a bi-
partite form, dividing nearly into two and only two branches or families of
witnesses? The problem was first posed by the French medievalist Joseph
Bédier (1913, 1928); his suggestion that the explanation for this striking
prevalence was to be sought not in the intrinsic nature of textual transmis-
sion itself but in the faulty methods of textual editors provoked an intense
discussion among many scholars, including Pasqualli and Maas. Where Pas-
qualli denied the truth of Bédier’s observation, arguing that bipartite stem-
mas were far less frequent than Bédier thought, Timpanaro agreed with
Bédier about the fact of their overwhelming dominance; and where Maas
accepted that apparent fact but attempted to provide a statistical explana-
tion for it in terms of the possible outcomes of various scenarios involving
the copying of manuscripts, Timpanaro attacked both Maas’s statistics and
his whole methodology. Timpanaro himself seeks a cautious middle way,
accepting the predominance of bipartite stemmata as an objective fact due to
the modes of textual propagation in the Middle Ages, but suggesting that
certain erroneous editorial techniques increase the number of apparent cases
of bipartism beyond what ought to be the case. His own arguments (in-
cluding most recently another posthumous text, Timpanaro 2002c) did not
conclude the discussion once and for all, but gave rise to a series of contrib-
utions by other scholars, particularly Michael Reeve (especially Reeve
1986). As is indicated by Timpanaro’s posthumous paper published here, he
himself believed that Reeve had definitively proven him wrong in several im-
portant points (though not in all), and this judgment of his, though perhaps
too harsh, seems substantially correct.

But the issues at stake are of great enough interest from a methodologi-
cal point of view that the value of Timpanaro’s arguments is not thereby
simply vitiated: anyone interested in questions of textual transmission can
learn from the debate as a whole. And beyond the specific question of bi-
partite stemmata, the disagreement between Maas and Timpanaro retains a
permanent significance. For Maas’s concluding aphorism, “No specific has
yet been discovered against contamination” (Maas 1958 [1927]: 49), in set-
ting narrow limits to the scope of rationality in textual edition, had seemed to
assign everything outside them to the sway of caprice and blind chance:
Timpanaro’s sustained effort was directed not only to extending those lim-
its as far as he could, but also to demonstrating that even inside of them mat-
ters were not at all so geometric and predictable as Maas seemed to suggest.
Ultimately, what was at stake in the debate between them were different
views of what was to count as rationality in textual edition—for Maas, only
pure algorithms; for Timpanaro, statistical probabilities and stochastic pro-
cedures as well. If only for this reason, the disagreement between them,
which reflected not only their personalities but also larger differences in
the conditions of the sciences when the two men were being educated, could
never have known a final winner. This makes their debate not less interest-
ing and significant but all the more so.

Timpanaro’s demonstration in the present study is compact, pointed,
stringent, and, especially for the first of its three goals, entirely convincing.
Yet it is remarkable not only for these strengths, which it shares with other
great works of scholarship, but also in three other regards, in which it is un-
usual, indeed anomalous.

First, when it was first published, in the form of two articles that ap-
peared in 1959 and 1960 (Timpanaro 1959, 1960), it was the work of an
astonishingly young man: Timpanaro was only thirty-six years old at the
time, yet he was already prodigiously learned, assured in his judgments,
mature in his formulations, authoritative in his tone.

Second, even more strikingly, at the time he wrote these articles Timpa-
naro had never himself performed a critical edition of any text, ancient or
modern. He was best known in this period for his work on Leopardi’s philo-
sological writings, yet he had not edited these and did not do so until a decade
later, and then in collaboration with Giuseppe Pacella (Leopardi 1969). To
be sure, Timpanaro had already published a number of articles that demon-
strated his interest and competence in questions of textual criticism of Latin
poetry, especially a series of three articles presented as preparatory toward
not be more evident. The histories of Classical scholarship tend to be diachronic in sequence, biographical in format, and sometimes unmistakably hagiographic in tone; along the line of a more or less simple temporal axis they string together the biographies of the great scholars of the past, indicating their personal vicissitudes, strengths, and foibles, listing their great works, and inviting the student to admire their contributions to scholarship, to try to understand them, and to imitate them as best he can. The manuals of textual criticism are usually systematic in orientation, synchronic in structure, entirely unbiographical in format, and usually cool and reasoned in tone; they attempt to construct a series of rules to explain how errors come about and to indicate to the future editor what techniques and lines of reasoning he should apply in order to construct the best possible edition out of the materials at his disposal. The biographies of individual scholars are usually closely focused on the life and works of the particular person who forms their object, often employ archival material, and are usually more interested in synchronic and local, rather than in diachronic and large-scale contextualization. Finally the introductory manuals are most often encyclopedic in character within the terms of the specific discipline that is being presented; only rarely do they succeed in combining perceptive analysis of individual figures with the larger development of the field and of cultural history as a whole. What unites all these kinds of studies is that with very few exceptions indeed, their author is legitimate in the eyes of his readers by the first-class work he does in separate publications as an editor of Classical texts—an apparent generic requirement of such studies that makes the fact that Timpnaro never did edit a Classical text himself seem even more anomalous.

The Genesis of Lachmann’s Method, on the contrary, is an attempt to historicize the techniques of textual criticism and edition such as had not really ever been ventured before. It fits into none of the categories of scholarship on the history of Classical scholarship we have just distinguished. (1) Timpnaro is indeed writing a history and for that purpose does make use of biography as one of the important strategies with which he organizes his material, but he employs the accounts of individual lives only tactically, instrumentally, in order to explain and make these historical developments concrete, never as an end in itself. Besides, he makes no claim to present a survey of the whole of Classical scholarship in the period he considers, but only a single technique, Lachmann’s method. (2) Moreover, he considers editorial procedures not as a timeless set of universally valid rules that were always waiting to be discovered and, once revealed, could now be formalized and for all in a permanent and perfectly systematic arrangement, but rather as the product of specific human needs as these developed over the course of centuries in frequent contact with other cultural domains.

a new edition of the fragments of Ennius (Timpnaro 1946, 1947, 1948–49). But that edition of Ennius had not yet appeared at the time, ten years later, and in fact it never did appear. Indeed, by the end of his life the only editions that Timpnaro published were of writings by modern Italian authors which he prepared, almost always in collaboration with friends, on the basis of autograph manuscripts or early editions (e.g., Ascoli 1959; Bortolotti-Bornmann-Manfredi-Timpnaro 1963, 1970; Leopardi 1969; Pasquali 1986a, 1986b), and two popular editions of foreign texts with Italian translation (d’Holbach 1883; Cicero 1888, 1998, the latter indeed quite remarkable, but for its extraordinarily rich commentary, not for its text). Thus Timpnaro never published during his entire lifetime a single critical edition of an ancient author on the basis of the very procedures of collation, recension, and emendation of manuscripts that he had analyzed with such expertise and penetration in this book. He also planned for many years to write a handbook of textual criticism, but nothing ever came of this project. We should bear this in mind when we read what is almost the closing words of the body of this book: “And the practical exigency remains, that certain critical editions not be postponed forever for the sake of studying the history of the tradition in all its smallest details, that scholars not bury themselves so deeply in the study of medieval and Humanist culture that they forget to return to textual criticism” (below, p. 138).

Third, The Genesis of Lachmann’s Method is in a certain sense a book without a genre. Those who work on the modern study of ancient Greek and Latin literature tend in general to produce one or another of various kinds of sharply differentiated studies of its techniques. There have been (1) numerous histories of Classical scholarship, such as Bursian 1885, Sandys 1908–21 (1903–8) and 1915, Gudemann 1909 (1907), Kroll 1919 (1920), Wilamowitz 1882 (1921), or, more recently, Pfeiffer 1968 and 1976, Reynolds-Wilson 1990 (1968), Kenney 1974, and Briggs and Calder 1990; (2) many manuals of textual criticism, of which the best-known ones of the twentieth century are Havet 1911, Kantorowicz 1921, Maas 1928 (1927), Dain 1934 (1949), van Groningen 1963, and more recently West 1973; (3) various biographical studies of individual scholars, which have attempted to set their work in the context of their personal lives and historical periods, such as Bernays 1855 on Scaliger, Mably 1864 on Politian, Jebb 1882 on Bentley, Pattison 1892 on Casaubon, Clarke 1957 on Porson, and Grafton 1983–93 on Scaliger; and finally (4) a few introductory textbooks discussing methods in an individual scholarly discipline as a whole, which have included important technical histories of that discipline, most notably Wachsmuth 1869 on ancient history and Traube 1965 (1909–20): 1.1–80 on paleography. The differences between these various kinds of works could
(3) His book does bear Lachmann's name in its title, and it provides a penetrating analysis of many of Lachmann's works, yet it does not really fit into the category of biographical studies: its historical focus is far too wide (for it considers the history of a method from ancient times to the present), while its thematic focus is far too narrow (for it ignores many aspects of Lachmann's life and works and concentrates only on the question of Lachmann's method). (4) And finally, Timpanaro's work displays similarities in certain respects to Traube's history of paleography, which Pasquali and Timpanaro much admired, and Traube's analysis of Jean Mabillon's De re diplomatica libri VI (Traube 1965 [1909–20]: i.20–30) is perhaps the closest analogue within Classical studies to Timpanaro's account of Lachmann; yet Timpanaro's monograph is certainly not a study of the history of any discipline in its entirety, and Timpanaro himself is careful to emphasize that its object is not even textual criticism as a whole (below, p. 37), but only a single and very specific editorial procedure.

How are we to explain the oddities of this book? No doubt some are due to the individual peculiarities of its author, and doubtless no set of explanations will ever suffice to clear up altogether, or perhaps even to reduce significantly, its mysteries. Nonetheless it would be unfair to Timpanaro not to attempt to subject his own book on Lachmann's method to the very same kind of genealogical analysis that he applied with such success to that very method. We may therefore rephrase the above question as follows: What are the personal influences and scholarly traditions within which it makes most sense to situate The Genesis of Lachmann's Method?

The most obvious answer is of course Giorgio Pasquali, Timpanaro's teacher at Florence and Pisa. Pasquali had responded to the eighteen highly condensed and abstract pages of Maas's Textkritik (Maas 1958 [1927]) with a detailed and highly critical review that filled more than twice as many pages in the most prestigious German disciplinary organ (Pasquali 1959; five years later appeared the first edition of a lengthy monograph, ten times longer, which presented a full version of his views, Storia della tradizione e critica del testo (History of the tradition and textual criticism; Pasquali 1952 [1934]). Where Maas had striven to formulate in its most essential form an extreme version of Lachmann's method, formalizing a mechanical set of rules for determining relations of filiation among manuscripts in order to permit mere copies to be discarded for the sake of textual constitution, Pasquali insisted on all the factors that set limits to the validity of any such method, reminding scholars that copyists could be not only fallible machines but also creative and intelligent readers and writers, that medieval variants might well go back in certain cases to a plurality of ancient editions, and above all that medieval and Renaissance manuscript copies were not only dispensable witnesses to ancient texts, for the reconstitution of which they could be sacrificed without loss, but also a precious testimony to later understandings of earlier writings, documents of reception that had not only a hermeneutic value but also a cultural and historical dignity of their own.

It is evident that Pasquali's book provided the stimulus and the model for Timpanaro's—indeed, Timpanaro himself acknowledges as much when he writes in the preface to the first edition that his book is "an investigation born, one might say, in the margins of Pasquali 1952 (1934), of which it presupposes the reader's familiarity and to which it constantly refers" (below, p. 37). Pasquali posed many of the basic questions for which Timpanaro's study went on to try to provide more detailed and satisfactory answers. Moreover, the very structure of Pasquali's book bears a striking affinity to Timpanaro's; both books begin with a historical examination of the origins of Lachmann's method and of Lachmann's own use of it, and then go on to assess the limitations of its validity, though of course the proportions between the two parts are completely inverted in the two cases. Indeed, Pasquali's text has even influenced a number of the verbal formulations in Timpanaro's—for example, compare the following text of Timpanaro:

Later—in the nineteenth century, as we shall see, and unfortunately even today—this procedure, which has received the technical name of eliminatio codicium descriptorum [elimination of derivative manuscripts], has often become a convenient expedient for saving the Classical philologist time and trouble: insufficient evidence, or even the simple observation that there is a mass of recentiores [more recent witnesses] alongside a manuscript of considerable antiquity, has too easily suggested that the more recent ones derived from the older one. (Below, p. 47)

with that of Pasquali:

And one must answer that every time there was an ancient manuscript on the one side and a certain number of recent ones with the same contents on the other, that generation tended to derive these latter ones from that former one, and did not hesitate unscrupulously to abuse technical means of proof, or, to speak more clearly, was satisfied with demonstrations devoid of any value. (Pasquali 1952 [1934]: 26)

It is only if this relation of openly professed dependence is borne in mind that we can fully understand the justice of Timpanaro's decision to conclude the final chapter of his book with a lengthy evaluation of Pasquali's scholarship, which, first published less than a decade after his master's death,
moves well beyond the limits that considerations of mere argumentative relevance might have imposed in order to assume the dimensions and character of a formal eulogy.

Yet the evident similarities and filiation between the two books should not mislead us into mistaking the fundamental difference in their orientations. It is not accidental that Pasquili’s investigation into the origins and development of Lachmann’s method occupies only the first, brief chapter of his book (Pasquili 1952 [1934]: 3–12), for his principal object is not so much the historical evolution of a set of procedures as rather what he considers to be the facts of the transmission of a large number of mostly ancient texts, which he takes to be such that those procedures cannot be applied to them appropriately and successfully. Ultimately, Pasquili’s is a manual of editorial technique, addressed above all to this question: given the true nature of textual transmission, how best are we to edit texts? Its principal difference from other such manuals is that, so far from setting itself the task of promulgating rules that are to be applied more or less mechanically, it insists on the many factors that set narrow limits to the validity of all such rules; the extraordinary richness of its documentation ends up performing a deconstruction of traditional manuals of textual criticism, but from within the genre. Even Pasquili’s introductory chapter on Lachmann’s method is intended not so much to provide a dispassionate investigation of its history as rather to deprive it of the authority that, for so many scholars, derived from the prestige of his name. Timpanaro’s book, by contrast, is the history of the gradual discovery of a particular scholarly method over the course of centuries; the considerations of that method’s degree of validity, which occupy the final sections of his book, are appended to that historical investigation and are not at all indispensable to its argument. Ultimately, Timpanaro’s is a history of Classical scholarship, differing most strikingly from traditional examples of that genre in being organized not as a series of biographies but rather in terms of a single and highly specific scholarly method, to whose discovery a large number of individuals contributed in varying ways and degrees over the course of hundreds of years.

In a larger sense, the closest parallels to Timpanaro’s study of Lachmann’s method are to be found not in the field of Classical scholarship after all, not in histories of the discipline nor in manuals of textual criticism nor in biographies of scholars nor in introductory textbooks on method, but instead in the history of science: for Timpanaro’s investigation into the development of a single method, the most striking analogies are the sorts of studies of the development of an individual scientific technique or concept—the integral calculus, the heliocentric theory, the theory of relativity, and the like—which have been standard fare in the history of science since the nineteenth century. These were the kinds of studies that filled a considerable part of the libraries and minds of Timpanaro’s parents during his childhood and youth, and they have left an evident trace on all of his own work. Sebastiano Timpanaro Sr. had already been passionately engaged in the history of science even before he was compelled to abandon his active participation in research in experimental physics, and afterward he devoted his considerable energies and intelligence to this field above all: he always regarded the history of science as a crucial link between science and civil society, a link that had to be vigorously nourished and strengthened in order to prevent a potentially quite dangerous cultural fragmentation, particularly in a country with a strong humanistic tradition like Italy. And Timpanaro’s mother devoted her work on ancient Greek philosophy above all to the connections between philosophical and scientific thought in the period from the pre-Socrates through Aristotle, for example, in the case of the Pythagoreans, insisting against many of her colleagues on those elements in their doctrines that were more mathematical and naturalistic, less mystical and shamanistic.

Timpanaro himself cultivated an active interest in the discipline of the history of science, for example, maintaining a fairly close personal relation with the prominent Italian Communist philosopher of science Ludovico Geymonat (despite the absolute incompatibility of their political views), collecting, reading, and annotating his books and publishing two important reviews of his works (Timpanaro 1973, 1977c). And his quasi-professional familiarity with the scientific discipline of historical linguistics is evident on many pages of the present book. For this presence of a strong scientific component in Timpanaro’s scholarship, it seems evident that his parents are an important explanatory factor. To those who have studied their works (edited expertly by Timpanaro himself), much in the present study will have a familiar ring. Thus Timpanaro takes care here to link the historiography of science with that of society as a whole—but already his mother had written, “the historian of science is a scientist with a critic’s and a historian’s mentality, who feels the need to illuminate scientific creation and to insert it into the process of scientific thought and of the general vision of the world of its period, with which it stands in a relation of such intimate necessity that if it is abstracted from it that vision ends up not only being distorted, but also falsified” (Cardini 1951). We can see throughout these pages how fruitful a cautious use of biography can be for the history of science, given that there is no science without scientists—but already his father and mother had written the history of science in precisely the same way, in terms of individuals like Leonardo and Gallileo, Alcmaeon and Melissaus, and his father had declared programmatically, “the history of science . . . must present to us the physicists, the chemists, the naturalists, the mathematicians, the astronomers, living and working, in such a way that they become as familiar to us as we are ourselves. We must live their triumphs, their investigations,
their hypotheses in all their details, in all their nuances, in all their energy” (Timpanaro St. 1952: 13).

Between this very narrow context provided by Pasquali and Timpanaro’s parents, on the one hand, and the very broad context supplied by the history of science on the other, it is very difficult indeed to identify a specific, middle-range institutional context within Classical studies proper that can adequately explain the genesis of The Genesis of Lachmann’s Method. Indeed, Timpanaro’s book is not really very much at all like any of the standard forms of the history of Classical scholarship that preceded it. Yet there is one field of scholarship that flourished in Italy in the 1940s and 1950s, and with which Timpanaro was demonstrably familiar, in which the kinds of questions he raised here and the kinds of strategies he deployed in trying to answer them did have an important place: the study of the scholarly practices of Renaissance Humanism. Of course, the Italian Humanists have long been a favorite object for study by their modern comparitors, but such study has usually not been focused on the kinds of precise technical issues that are so characteristic of Timpanaro’s book but on other kinds of questions: biographical, political, literary, archival. Yet already in the 1920s Remigio Sabbadini had examined, acutely if only briefly, “the method of the Humanists” (Sabbadini 1922), and even before the Second World War so too did a few other scholars, most notably Berthold L. Ullman and P. O. Kristeller, who were not themselves Italian but were closely associated with Italian scholars and insititutions (thus Kristeller taught German at the Scuola Normale Superiore di Pisa from 1933 to 1938, before the Fascist racial laws forced him to emigrate to America). But it was not until after 1945 that these studies of philological method really took off, especially in Italy and England, perhaps in part as a sober and dignified reaction against excesses of the rhetorical culture of Fascism. These were the years of such pathbreaking studies as those by José Ruizschaet on Lipsius and Tacitus (Ruyschaert 1949), by Giuseppe Billonovich on Petrarch (Billonovich 1951), and by Carlo Dionisotti on Filetico and Virgil (Dionisotti 1958), to mention only a few particularly remarkable examples. For Timpanaro, the crucial figure in this scholarly movement was certainly Alessandro Perosa (1910–98), who studied at the Scuola Normale Superiore di Pisa from 1928 to 1932 and then was the administrative secretary there (a post also involving teaching duties) from 1933 to 1953, and was professor at the University of Florence from 1959 to 1980. Much influenced by his teacher Pasquali, Perosa turned away in the 1940s from the study of Greek and Latin literature themselves and devoted himself henceforth to research into the Italian Renaissance Humanists—research conducted with the learning and interests of a trained Classical scholar. He investigated above all Politian as a Humanist philologist and demonstrated convincingly the range and brilliance of his Classical scholarship and the almost inextricable mixture of traditionality and originality in his methodology (his papers are collected in Perosa 2000). Perosa’s catalogue of the 1944 exhibit on Politian at the Laurentian Library in Florence (Perea 1954) provided one extraordinary example of how the methods of a long-dead scholar could be analyzed tersely, technically, and nonetheless quite interestingly; and Perosa’s seminars at Pisa must have supplied many more—Timpanaro himself refers to them with unmixed admiration (below, p. 46). And some of Timpanaro’s own earliest and already most polished work belongs very clearly to this line of research (e.g., Timpanaro 1951).

In the 1950s, when Timpanaro’s Genesis of Lachmann’s Method was in the course of gestation, Classical studies in Italy had not yet fully recovered from Pasquali’s death and were languishing to a certain extent. But in those same years Humanist studies were burgeoning there, and it does not seem at all unlikely that it was from this lively, fresh, and challenging field that Timpanaro derived some of his deepest inspiration and at least some of the models and standards for his own highly personal brand of scholarship. This might well help to explain why his book on Lachmann begins, perhaps somewhat oddly, not with the ancient grammarians who first invented some of the philological practices that Timpanaro investigates, nor with the great eighteenth-century biblical scholars who first explicitly formulated them, nor with the nineteenth-century German Classicists who were Lachmann’s most immediate predecessors, but rather with the Humanists of the Italian Renaissance, and above all with Politian. And it represents a point of some difference between Timpanaro and Pasquali, for the latter, for all his love and knowledge of Italian literature and of the reception of the Classics, never quite achieved a degree of familiarity with the Renaissance Humanists comparable to that of his student Timpanaro, preferring instead to concentrate on such traditionally literary Italian Classics as Dante, Petrarch, and Boccaccio.

Finally, it is not entirely impossible that some role in shaping the young Timpanaro’s sense of the history of scholarship—the nature and importance of the field, the kinds of questions to ask, the strategies available for answering them—was also played by the historian Arnaldo Moniglano (1908–87). Moniglano had left Italy for England in 1939, but after the war he reestablished intense contacts with Italian colleagues, especially younger ones; in 1964 he accepted a position at the Scuola Normale Superiore di Pisa, while retaining his chair at the University of London. A close intellectual and personal friendship developed between the older historian and the younger Classicist during the 1950s and lasted until Moniglano’s death; it is documented both in the books and articles of each to be found in the personal libraries of the other (now both part of the library of the
Momigliano did indeed influence Timpanaro in any significant way, that the latter never acknowledged this in his writings, given his tendency scrupulously to record all his intellectual debts—even Timpanaro’s keen awareness of his disagreements with Momigliano in other matters would scarcely have prevented him from recognizing such a debt to him in this one. Viewed from above and outside, the fundamental affinities in method between the two scholars are unmistakable, and Momigliano is probably the most important Italian, or indeed European, intellectual figure active in the field of the history of Classical scholarship in the late 1950s who could have exerted any degree of formative influence on Timpanaro. But the likeliest proximate explanatory context for Timpanaro’s work remains not Classical scholarship at all in the narrow sense but the history of Italian Humanism.

In any case it should cause no surprise that Timpanaro’s work on the history of Classical scholarship is here associated so closely with the history of science, for it is only in a narrow and historically provincial view of the history of science that this discipline can be thought to be restricted to the study of the physical and biological sciences. “Science” and its equivalents in many languages designate any disciplined and institutionalized effort to apply the sustained exercise of reason to the study of man and his world, and precisely this was certainly Timpanaro’s deeply felt and often asserted understanding of the nature of Classical philology. Of course there are many other components to the professional activity of the Classical scholar (as, for that matter, of the natural scientist as well) besides purely rational ones—taste, intuition, empathy, experience, to name only these. But in the present book Timpanaro concentrates as far as possible on some of the technical procedures of Classical scholarship that are most clearly an expression of rationality and considers their sources and scope. After all, there can be no doubt that the method of examining the filiation of manuscripts is a triumph of reason, for it is quite irrational to accept the readings found in a manuscript merely because it happens to be near to hand or is old or because other scholars have accepted it in the past.

Thus the story Timpanaro tells in this book is that of the gradual triumph of reason over the forces of habit, laziness, religious intolerance, and stupidity. For all his materialism, Timpanaro seems to acknowledge a teleological force that certainly does not operate in the wider field of human history as a whole but at least seems to a certain extent to do so in this narrower, technical domain, driving his story forward as a kind of motor and endowing it with a forceful internal dynamic. This may be why he emphasizes that some figures, such as Gottfried Hermann, lag behind the point at which others have already arrived, and why he always takes pains to isolate as clearly as possible the individual steps forward made by this scholar or that one, be it Le Clerc or Madvig or many others. An expectation, if not of the inev-
tability, then at least of the strong probability of rational progress within the limited confines of a particular science seems to underlie his account and makes it difficult for Timpanaro to account satisfactorily for two kinds of exceptions to this progress, which he nonetheless takes care insistently and repeatedly to point out: that progress is not continuous but encounters relapses and regressions, as in Germany at the beginning of the nineteenth century; and that scholars' theoretical precepts often do not square fully with their actual practice (for example, in the cases of Bentley, Ernesti, and Orelli).

But the story Timpanaro tells is not a purely internal one, in the sense of involving nothing more than increasing degrees of rationalization of scientific procedures, determined only by strictly scientific considerations. It is also external, for the process he describes is partly impeded, but also partly supported and encouraged by the fact that the techniques of text edition do not exist in isolation from the other regions in the larger culture around them but are instead profoundly influenced by them. After all, one of the texts whose edition was at issue in this period was the New Testament, and questions of which reading to choose in that text could easily acquire not only a scholarly weight but also a doctrinal one. Timpanaro takes care, following Pasquale, to point to the importance of the division between Catholics and Protestants, and within Protestantism to the influence of heretical currents, for the field of textual criticism of the New Testament in the seventeenth and eighteenth centuries (although both scholars were oddly uninterested in the no-less-important role of Old Testament criticism during the same period). So too, Timpanaro explores at length the relations between Classical textual criticism and Indo-European linguistics, and points to the impact of the general rise of irrationalist tendencies at the end of the nineteenth century on the value attributed to Lachmann's method. Despite the narrowness of his focus, Timpanaro is careful to situate his theme as broadly as possible: his story is one that moves from the fifteenth century to the twentieth, from Italy to France to Holland to England to Germany and back once more to Italy and France, from Humanists to theologians to Classical scholars.

In his scholarship as in his politics, Timpanaro may have been a progressivist, but he was very far from being triumphalistic—quite the contrary. He saw irrationality as difficult to achieve and as always endangered by the threatening forces of various kinds of irrationality—in this regard his study of Lachmann's method is at one with his political and environmentalist writings. It is after all quite rational to apply Lachmann's method to those cases in which it is appropriate to do so—where there is one archetype, mechanical transmission, and no contamination—and it would be quite irrational not to do so; there are indeed such cases, and it is a triumph of human reason, not perhaps the greatest one but certainly worthy of admiration and investigation, to have discovered this method and to have learned to apply it. Yet the success of a method inevitably leads some people to attempt irrationally to overextend it and to abuse it by applying it to cases to which it is not in fact applicable—so already Cobet, perhaps the true villain of Timpanaro's story, and so too Maas—and in so doing to cast discredit irrationally on a method that, applied appropriately, would have led to entirely acceptable results.

In a certain sense, then, one underlying purpose of Timpanaro's study is to rescue Lachmann's method against Lachmann's own errors and against his teacher Pasquale's overstatement polemics. Pasquale had suggested that that method was not Lachmann's, but only in order to discredit it further by driving a wedge between it and his celebrated name. Timpanaro proves that the method is not Lachmann's, but so as to demonstrate that it was not merely the invention of a single person, however gifted, but rather a culmination of many centuries of philological insights and hard work. What this book shows is that Lachmann's method was a shared discovery in which many scholars of different generations, countries, disciplines, and interests all had a hand. It was not inevitable that it should have been discovered—quite the contrary—and once it was discovered, its limits and value were not always assessed correctly—again, quite the contrary. All the more reason that the story of its discovery, as it is recounted here, however astutely, has its own deeply human fascination and rather melancholy pathos.

Timpanaro was as scrupulous in his scholarship as he was intransigent in his politics. One result was that this study was constantly revised, corrected, and updated over the course of almost four decades: it began its life in the form of two lengthy articles (Timpanaro 1959, 1960) and went through three editions as a book in Italian (1963, 1981, 1983) and one in German translation (1971). Each time it was republished, the originally literal and austere line of argumentation became more and more enriched (or should one say encrusted?) by further references to and discussion of the secondary literature that had appeared since the preceding publication. For Timpanaro's work was widely discussed and broadly influential. In general it may be said that recent scholarship has never fundamentally contradicted the findings of the historical portion of the book but only confirmed and deepened them, whereas the positions Timpanaro adopted in the systematic sections concerning the limits of the validity of Lachmann's method have sparked considerable discussion, controversy, and disagreement.

Another result was that after a certain point, sometime around 1986,
Timpanaro, always a perfectionist, but now aged, fatigued, and depressed, finally abandoned the attempt to revise his book yet again. A moving testimony to this decision is the hitherto unpublished essay, a rejoinder to Michael Reeve, one of his critics, which was presumably written around 1986 and appears posthumously in this edition in Additional Materials A, "[Final Remarks on Bipartite Stemmata]"—and no less moving testimony is provided by the fact that Timpanaro chose not to publish it. The reader of this volume should be aware that in the years after 1986, Timpanaro refused repeatedly to allow his book to be translated into English, claiming that it was by now obsolete and that he no longer had the energy required to update it. It is translated now by the generous permission of his wife and literary executor, Maria Augusta Timpanaro Morelli. My own view is that the historical portion of this work remains almost entirely valid, that the systematic sections can be enormously stimulating to students and scholars engaged in the problems of textual edition, and that the fact that Timpanaro himself no longer felt able to correct and revise his work as he wished to should not prevent it from being made available to the many readers who will be able to benefit from it—and who will be able to decide on their own whether he was right, or whether I was.

The basis for this translation was the last Italian edition published during Timpanaro’s lifetime; I was able to make use of his personal copy, with a number of marginal annotations in his hand. Considering the importance of this book and its evolution through a series of different versions, it has seemed appropriate to indicate that development as tacitly but as clearly as possible. Hence while the numbered footnotes are all Timpanaro’s own, the notes signaled by letters have been inserted by myself in order to indicate textual divergences between the final Italian edition and the earlier Italian and German ones; these divergences are presented in a genetic critical apparatus in Additional Materials B, "Differences among the Various Editions." All divergences in the text that are not merely stylistic have been indicated; on the other hand, no divergences in the notes have been indicated, as this would have swollen the apparatus beyond any degree of usefulness. The reader who chooses to ignore these notes can be confident that the text he reads represents the fully considered views of Timpanaro toward the end of his life, but whoever dips into their riches will be able to trace out in detail the development of his thought over a span of more than four decades and to admire all the more his scrupulous honesty and generosity of spirit.

Timpanaro was generally quite careful in his citations of primary and secondary sources, but the method of citation he used, although well established in Italy, may seem haphazard to American readers. I have systematized his references in the following way: author’s surname (with first initial or other indication in case of homonyms); the year of publication of the edition or translation of reference, followed in parentheses by the original year of publication of the work; colon; volume and page number, separated by a period, if the work comprises more than one volume, otherwise only the page number. The full bibliographical details can be found in the bibliography of works referred to by Timpanaro.

I have also added a selective bibliography of scholarly works on the various topics treated by Timpanaro that have appeared since 1986, the date of the last Italian edition of this book. This bibliography is of course far from complete, but I hope it provides at least a starting point for readers who wish to deepen their acquaintance with these topics and to find out how Timpanaro’s views are regarded today.

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At the conclusion of what has been an intense and very agreeable labor, it is a great pleasure to thank all those who have made it possible.

First of all I thank Timpanaro’s wife and literary executor, Maria Augusta Timpanaro Morelli, who has supported this project from the beginning, for giving me permission to translate this book and for making available to me Timpanaro’s own copy of the 1986 edition and the unpublished text that is included in an appendix. Her hope, and mine, is that with this translation her late husband will continue to find and inspire readers not only in Italy but throughout the world.

I am also very grateful to Mario Telò, an advanced student at the Scuola Normale Superiore di Pisa, who has assisted this publication in many ways. In particular, he collated the various editions of the work and thereby furnished the materials indispensable for the genetic critical apparatus; he systematized and checked all of Timpanaro’s references and prepared the bibliography of works cited by Timpanaro; he helped me to put together the bibliography of items that have appeared since 1986 (Additional Materials C, “Recent Bibliography”); he prepared a first transcript of the very difficult, and at times illegible, manuscript of Timpanaro’s unpublished essay; and he checked and corrected my English translation with care and elegance. It is a source of great satisfaction to me, and would certainly have been to Timpanaro as well, to know that Italy is still producing young Classical scholars of the very highest quality.

Riccardo Di Donato (Pisa) has improved this edition by his suggestions for the introduction and by his generosity in making available to me a preliminary transcription of the correspondence between Momigliano and Timpanaro, which he has edited and which will appear in 2005. Antonio Cardini (Pisa) helped with the decipherment of Timpanaro’s unpublished essay.
At a conference on Sebastiano Timpanaro and his parents that took place on 22–23 August 2003 in Torricino, in the province of Messina in Sicily, I learned much about Timpanaro and his family and presented a first version of part of the introduction to this volume. My thanks to the organizers, Michele Feo, Vincenzo Fera, Giacomo Ferrai, and Silvia Rizzo, and to the Centro Interdipartimentale di Studi Umanistici, Facoltà di Lettere e Filosofia, Università degli Studi di Messina, for inviting me to this conference, and to all those who participated in the lively discussion after my lecture.

Michael Reese (Cambridge) had the generosity and kindness not only to lend his support to this project at an early stage but also to subject the first version of the translation of the opening chapters to a searching criticism from which, I hope, it has emerged much improved. J. E. G. Zettel (Columbia) read through the whole translation in its penultimate form and made many suggestions for its improvement.

Franz Martin Scherer (Heidelberg), Jan-Dirk Mueller (Munich), and Antje Wessels (Berlin) were very helpful in tracking down Timpanaro’s references to German works of scholarship that could not be traced in Tuscany. The penultimate version of the whole introduction was read by Giuseppe Cambiano (Pisa), Antonio Carlini, Carlo Ginzburg (UCLA), Filippomaria Pontani (Pisa), Lucia Prassulco (Pisa), Mario Telò, Claudia Wassmann (Chicago), and Isabelle Wienen (Fribourg) and has benefited greatly from their observations and criticisms. Above all, Anthony Grafton (Princeton) subjected it to an incisive, erudite, and deeply generous critique, which has undoubtedly improved it.

My editors at the University of Chicago Press, Alan Thomas and Susan Bielstein, were always supportive, imaginative, resourceful, and (what is even rarer) patient.

My heartfelt thanks to all.

Glenn W. Most
Baratto-Florence-Pisa
June 2004

BIBLIOGRAPHY


Editor's Introduction


Editor’s Note to the Reader

There are two separate systems of annotation in the present edition:

1. Notes indicated by superscript numbers or asterisks formed part of the original Italian edition of Timpanaro’s work and are keyed to footnotes at the bottom of the page.

2. Notes indicated by superscript letters have been added by the present editor in order to mark differences among the various editions of the Italian original and are keyed to Additional Materials B, “Differences among the Various Editions,” on pages 216–33 below.

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THE GENESIS OF LACHMANN'S METHOD
Preface to the First Edition

A first version of this study appeared earlier as Timpanaro 1959, 1960. That it can now reappear in a revised and enlarged form is due to my friend Gianfranco Folena and to the director of the Biblioteca del Saggiatore, Prof. Bruno Migliorini. To both of them I express my deep gratitude.

The modifications and additions have been more numerous than I anticipated. Nonetheless, the aims and limits of my work remain the same ones as are indicated in its brief introduction. This is not a history of textual criticism—that would demand a much greater breadth of treatment—but merely an investigation that aims to clarify the gradual formation and then the crisis of that “genealogical method” that goes under Lachmann’s name: an investigation born, one might say, in the margins of Passquali 1952a (1954), of which it presupposes the reader’s familiarity and to which it constantly refers. And because many of the methodological problems faced by Lachmann, his contemporaries, and his followers are still unresolved, it seemed to me that it might be useful if I made some contribution to the discussion concerning them; see in particular chapters 6 and 8 and the second and third appendices [appendices B and C].

In the course of revising this essay, I received valuable contributions from Konrad Müller (a textual critic who knows the history of his discipline as few others do), Fritz Bornmann, and E. J. Kenney. Thanks to them, chapter 5 and the first two appendices appear here in a more complete and correct version. The third appendix, which is published here for the first time, owes much to the friendly collaboration of Vincenzo di Benedetto and Alfredo Stussi. I have also taken account of suggestions and criticisms by Eduard Fraenkel, Antonio Carlini, Antonio La Penna, Manfredo Manfredi, and Jean Panvin.

What Eugenio Grassi’s friendship meant to me I cannot express adequately. An incomparable expert on Greek language and style, animated by the aspiration to submit all hypotheses to rigorous verification and to clear the field of the many ambitious and arbitrary constructions in which Clas-
Preface to the First Edition

scholarly philology abounds, he was a unique guide and judge, for me as for his other friends: now that we can no longer avail ourselves of his illuminating criticism, our work has become all the more difficult and uncertain. Besides the interpretation of ancient texts (the activity for which he was best fitted by nature), he also made extremely acute contributions to the methodology of textual criticism: among the posthumous writings published in Grassi 1961, see his observations on the concept of an archetype and the difficulties it presents. It is to be hoped that those observations, together with others he has left us, will be reconsidered and further developed by future scholars.

S.T.

Preface to the Second Edition

The first edition of this little volume, which appeared in 1963 (= Timpanaro 1963a), has been out of print for many years. In 1971 a German edition, Die Entstehung der Lachmannschen Methode, appeared in Hamburg, published by Helmut Buske (= Timpanaro 1971), with additions and corrections due in part to myself, in part to my friend the translator, Dieter Irner, who is also an important scholar on Demosthenes and the Greek medical writers, and to his colleague Volker Schmidt. But this second Italian edition is not simply a retranslation of that German edition into the Italian language. From 1971 until now I have felt the need to deepen and broaden my research on various points, even without aiming at a degree of completeness that would be impossible anyway. Moreover, two important works have appeared that, more than all others, have induced me to add corrections and additions to my work: Rizzo 1973 (an exemplary book, whose wealth of historical sense and interpretative acumen is far fuller than the title would suggest) and Kenney 1974, of which a revised and enlarged Italian edition will appear shortly. Kenney’s book, notable for its lucidity of exposition and sureness of judgment, is a history of textual criticism from the Renaissance to the present—something never even attempted until now (this fact was neglected by certain reviewers, learned and acute, but too sarcastic and inclined to criticize before reading attentively), comprising a much broader range of material than the present study. As is only obvious, Kenney has reconsidered many of the arguments and problems I had already discussed, and has often done so in greater depth. Hence I have derived great profit for this new edition from his book, as from Rizzo’s.

It would take too long to list here other recent works that I have found useful for this new edition: the reader will find them cited in the course of the volume. One danger I ran, paradoxically, was to take too much account of all these new contributions—not that they did not deserve it, but I did not wish my little book, even if corrected and enlarged, to become “bloated” just in order to repeat badly everything that Kenney and other future schol-
ars had said and will say so well; it was supposed to stay within the limits of its specific subject matter, which were already explained clearly in the preface to the first edition, reprinted above; and the first two chapters in particular were supposed to retain their introductory character. It was not easy for me, in those first two chapters, to reconcile the need to be "streamlined" with the need to be not too incompletely informative: I hope that I have not succeeded too badly. Upon certain figures and problems of seventeenth- and eighteenth-century philology, about which interest has been particularly lively in recent years, I did linger, perhaps a bit too much with respect to the general economy of the book, even if too little with respect to their importance: I would like to return to deal with them elsewhere soon, although I recognize that the history of the philology of that period is indissolubly linked to the history of the movements of religious reform, a field in which it will not be easy for me to acquire an adequate preparation.

To counterbalance my many revisions and additions, I have performed only a few little cuts in comparison with the German edition. Both my friends Erner and Schmidt, whom I have already mentioned, but especially the former, had inserted into that edition some notes briefly indicating the results of their own research on particular problems of textual criticism. Although these notes were very interesting in themselves, they ran the danger of remaining somewhat extraneous to the continuity of my exposition; and anyway, Erner and Schmidt themselves later developed those ideas in separate works and will go on, I believe, to develop them further. For that reason I have left out some of their contributions and condensed others into a few words. To Erner I have a further debt of gratitude: he was the first to mention to me, and then to supply, Lutz-Hensel 1975.

Appendix C, on the other hand, has been much reworked to take account of recent studies. Among the many reasons that lead to disproportionately increasing the number of bipartite stemmata, I have now emphasized more forcefully the one that from the very beginning had seemed to me the most important one, and I have tried to explain its mechanism better. I wish I could have been briefer: I do not believe that exclusively and abstractly stemmatological problems should be overvalued in textual criticism, but, precisely in order to reduce them to their correct dimensions, it was necessary to examine in a bit more depth certain arguments and to unmask certain specious sophisms.

Whoever has seen in this work of mine a desire to diminish the figure of Lachmann has misinterpreted my intentions. I wanted instead to show how "Lachmann's method" was the result of a collective effort in which other excellent philologists besides Lachmann participated, each one with his own intellectual character. What mattered to me was to define each one's role, to clarify the influences and relations of collaboration and antithesis, to show how the "crisis" of the method (a crisis that has deprived it of its absolute validity but has not at all vitiated its usefulness) was manifested not only in the period after Lachmann, which I discuss in my last chapter, but was already found in nuce in philologists' studies long before Lachmann, and even in Lachmann himself in certain oscillations and contradictions beneath his invariably self-confident and peremptory tone. I had no desire to make licentious and antihistorical claims for priority, to hunt for "precursors"—even though it is not without interest to determine from whom Lachmann derived certain ideas and even certain technical terms, and to present, even if only in brief hints, an image of "the man Lachmann" that is free not only of moralistic censure but also of that hagiography of which some of his overly zealous disciples made him the object. In my view, it is not at all useless, even in the history of a rigorously technical discipline, to take account of biographical elements, to seek to "make the characters come alive," without of course making any concessions to an episodic and gossiply biographies.

My revision and enlargement of this work have been greatly assisted not only by works published in these last years but also by personal or epistolary exchanges of ideas with various scholars. Here too I have preferred to declare my debts as I go along, in the course of my exposition. But here I wish to mention in particular Giovanni Battista Alberti, Severino Caprioli, Antonio La Penna, Scvela Mariotti, Dante Nardo, Antonio Enzo Quaglio, Wolfgang Schmid, Alfonso Trabia, and Gian Piero Zarrì. To Dante Nardo I am deeply grateful not only for suggestions of considerable importance, derived from his experience as a textual critic and student of the history of philology (the essays that he will publish soon on Giulio Pontederia and Pietro Canal will show these two Latinists, who are not known well enough, under an almost entirely new light*), but also for the friendship he showed me by supporting the publication of this edition of my work with the Librivan publishing house. And an expression of thanks just as warm and affectionate is due to the director of the series, Sergio Romagnoli, for accepting this little book.

Twenty years have passed since an early and cruel death extinguished at only thirty-three years the man to whose memory this work was dedicated, and is still dedicated. But the memory of his lucid intellect and of his melancholy, concealed under irony, still remains present in his friends, as do ad-

* On Pontederia see now Nardo 1981; on Pietro Canal, a distinguished Venetian philologist of the nineteenth century, whom the character of his studies excludes from the scope of this little volume, important studies edited by Nardo and his students have already been published and others will follow.
miration for what he achieved in such a brief time and mourning for what he was not destined to accomplish.

s.t.

This edition has sold out in a short time; I have prepared only a corrected reprint. I have rectified typographical errors and my own mistakes in the text (I am grateful to G. B. Alberti, R. Führer, A. Golzio, M. D. Reeve, A. Rotondò, and A. Stusi for pointing out several to me); I have made a minimum of additions at the back of the volume, but I have left the treatment substantially unchanged without subjecting it to further revisions that would have ended up deforming it and destroying its original physiognomy. I believe that this little book can still serve generally to orient readers and that it remains valid in many essential points. In others it is outdated; but perhaps it has contributed toward stimulating new ideas and new research in a field of studies in which much work still remains to be done in depth. But other scholars will continue that work better than I can, bringing fresh energy and new ideas to it.\footnote{1. Among these discussions of the art of conjecture before the nineteenth century, the two best are the Ars critica of Le Clerc 1730 (1697) and Morel 1766 (= Quintin 1846: 969–1120). The work of Le Clerc also treats some questions that today we would assign to recension. On this see below, pp. 65–65, 6820. On Morel, see Kenney 1974: 44–46. Robortello 1657, a short treatise, is worth recalling without too much anachronistic severity, since it represents the first attempt at this kind of discussion (although obviously the attempt is imperfect, in certain aspects it anticipates the future); on this see Carlini 1667: 65–70, Kenney 1974: 29–36. But the Middle Ages (twelfth century) had already possessed in Nicola Maniacata of Rome an isolated scholar—in several respects an interesting one—who enunciated certain theoretical principles of textual criticism. He is the subject of a series of studies that are valuable (although occasionally tending toward exaggeration): Peri 1967 and 1977.}

Introduction

Of the two parts into which Lachmann divided textual criticism—recensio [recension] and emendatio [emendation]—the second had been practiced since antiquity. In the seventeenth and eighteenth centuries it had also been the object of good methodological discussions, to the degree that this was possible considering that by its nature it is an "art" rather than a "science."\footnote{1. Among these discussions of the art of conjecture before the nineteenth century, the two best are the Ars critica of Le Clerc 1730 (1697) and Morel 1766 (= Quintin 1846: 969–1120). The work of Le Clerc also treats some questions that today we would assign to recension. On this see below, pp. 65–65, 6820. On Morel, see Kenney 1974: 44–46. Robortello 1657, a short treatise, is worth recalling without too much anachronistic severity, since it represents the first attempt at this kind of discussion (although obviously the attempt is imperfect, in certain aspects it anticipates the future); on this see Carlini 1667: 65–70, Kenney 1974: 29–36. But the Middle Ages (twelfth century) had already possessed in Nicola Maniacata of Rome an isolated scholar—in several respects an interesting one—who enunciated certain theoretical principles of textual criticism. He is the subject of a series of studies that are valuable (although occasionally tending toward exaggeration): Peri 1967 and 1977.} In the nineteenth century, methods of emendation were refined further (this was especially due to progress in the study of the language and style of the various epochs and authors), but they were not transformed in a revolutionary way; nor can one say that, as far as divinatory talent is concerned, even the best conjectural critics of that century were superior to a Turnebus or a Bentley or a Reiske.

Instead, the great novelty of nineteenth-century textual criticism was the scientific foundation of recensio. But how this was attained; how much of "Lachmann's method" should really be attributed to Lachmann, and how much should be claimed for his predecessors and contemporaries instead; through what phases Lachmann himself passed in the course of developing his method—all this still remains to be clarified. The histories of Classical scholarship say almost nothing about it. Valuable hints concerning these
historical matters can be found in some discussions of textual criticism: Quentin 1926: 27–38 and passim; Dain 1975 (1949): 160–86; Giarratano 1951: 106–231 and above all, Pasquali 1952a (1934), in which, as in all his writings, philology and the history of philology are closely united (see especially chap. 1, “Lachmann’s Method,” which has already appeared separately as Pasquali 1931). Nonetheless the need is felt for a broader study, such as Joseph Bédier called for as early as 1928. I wished to make an attempt; others, after me, will do better. I only want to warn readers that the first two chapters have a purely introductory character; they do not aim at all to trace out a history of textual criticism from the Humanists until the end of the eighteenth century, but only to isolate some historical presuppositions and partial anticipations of “Lachmann’s method.”

1. In the following pages other writings by Pasquali are cited. Kenney 1974 has recently dealt with the subject more fully. The biography of Lachmann (Hertz 1851) offers very little on the questions that interest us here.

3. Bédier 1928. As is well known, the ideas Bédier proposed in this article are highly debatable (see below, chap. 3, n. 223), but he was right to deplore the lack of a study on the genesis of “Lachmann’s method” (1928: 183–7).

Emendatio ope codicum from the Humanists to Bentley

In the vast majority of cases, the editiones princeps [first printed editions] made by the Humanists were based on recent manuscripts, since these were easier to get hold of and more comfortable for the typesetters to read. Hence these editions for the most part reproduced a text that had been adjusted and “pretified” by copyist-interpolators. This text, propagated from one edition to another, constituted the “vulgate.”

In order to improve and correct the vulgate where it did not seem satisfactory, one could have recourse either to conjectures or to collation of manuscripts considered more authoritative. Classical philologists followed both these paths from the Humanist period until the end of the eighteenth century, some expressing their preference for the one, some for the other. The two approaches are described by Ruhnken at the beginning of his famous Elogium Tiberii Hemsterhuisii [Eulogy for Tiberius Hemsterhuis]: “Therefore scholars embarked upon one method of doing Criticism or the other, depending on the differences among their native talents. Some rashly uprooted things that were solidly established and should not have been disturbed at all, and harassed things that were certain by means of uncertain conjectures; others did nothing more than gather together the materials provided by manuscripts.” It is easy to see that Ruhnken is indicating the harmful exaggerations of the two approaches here rather than their positive aspects: evaluating his hero’s achievement in a way that was certainly exaggerated, he wanted to show that Hemsterhuis had been the first scholar to harmonize the two requirements and to establish the true, balanced art cri-
Eodem tempore codicis from the Humanists to Bentley

able copies—recent manuscripts or printed editions—with the genuine reading of a codex perpetuus [very ancient manuscript] that he has found in the Laurentian Library or to which some other Humanist has drawn his attention.

Up to this point there is nothing substantially new in comparison with the ancient grammarians: for example, controversies regarding textual criticism in Aulus Gellius’s Noctes Atticae [which constitute the principal model of the Miscellanea in its compositional structure] are often resolved by recourse to manuscripts of venerable antiquity (sometimes too venerable to be believable). But Politian often goes on to add a consideration of a genealogical nature in order to reinforce his preference for the older manuscript: recent manuscripts are copies of the older one; hence they do not have the value of an independent tradition.

Later—in the nineteenth century, as we shall see, and unfortunately even today—this procedure, which has received the technical name of eliminatio codicum descriptionem [elimination of derivative manuscripts], has often become a convenient expedient for saving the Classical philologist time and trouble: insufficient evidence, or even the simple observation that there is a mass of recentiores [more recent witnesses] alongside a manuscript of considerable antiquity, has too easily suggested that the more recent ones derived from the older one. Did Politian too sometimes work in this way? In the first edition of this book I suggested that he had done so, and even now I cannot bring myself to exclude this possibility altogether. And yet even then I cited a well-known example of an eliminatio that Politian based on solid evidence; and now I am inclined to believe, though with some reservation, that Rizzo is right (1973: 315ff) to maintain that such cases make up if not the totality, at least the great majority! In chapter 2 of his first Miscellanea, Politian demonstrates that the Laurentian manuscript 49,7 of Cicero’s Epistulae familares, which had one quite out of order by an error

5. Cf., e.g., Aulus Gellius 1.17.1, 1.21.21, 2.3.5; 9.74; 12.10.6; 13.21.16; 18.5.11. More generally on the use of manuscripts by Greek and Latin grammarians, cf. Lehrs 1882: 344–49. Oscillations between age and large numbers of manuscripts as a criterion for judging were not lacking: on Galen’s ideas on this subject cf., e.g., Bröcker 1887: 417.

6. E.g., Misc., ch. 5 ("an extremely old manuscript of the Argumenta of Valerius Flaccus [... from which I think all the other available ones are derived"); cf. the passage of the second Miscellanea cited below, n. 8, 25 (to which we shall refer shortly), 41, 96, 93, 95. Cf. the famous subscriptio to the incunable containing the Sibyllae of Statius inter alia: "I have come upon a text of the Sibyl of Statius [... from which alone all the other available manuscripts seem to have emanated, though it is full of errors and corrupt and, what is more, I think, reduced to half its original extent" (cf. Perosa 1955: 13, and below, n. 9); this is not the place to linger on the very controversial problem of just which manuscript Politian saw; and the analogous subscriptio to Apicenus (Rizzo 1973: 315ff).
of binding, is the ancestor of a group of more recent Laurentian manuscripts in which the same disturbance in the order of the letters is found without this being explicable by a displacement of the quires.\(^7\) He eliminated the apographs of an old manuscript of Valerius Flaccus by analogous reasoning, as the second *Miscellanea* now prove.\(^8\) Thus Politian not only venerates\(^9\) the oldest manuscripts in general: he also has the beginnings of a historical understanding of manuscript traditions. He is also aware that when a conjecture is necessary, it must find its starting point in the oldest stage of the tradition that we can reach, not in the deceptive patchings-up that corruptions have undergone in the more recent manuscripts (*Misc.* chap. 57, cf. chap. 29)—a criterion that will not be fully recognized until the age of Lachmann.

What is more, Politian already understood that the manuscripts (at least the oldest and most valuable ones) had to be collated not occasionally but systematically, registering all the readings that diverged from the vulgate text, including those that were certainly erroneous but that might turn out to be useful for restoring the text. This is the criterion he asserted in the *subscriptions* to the writers *De re rustica*, to Pliny, Statius, Pindar, and Terence; he had a full and justified awareness of its methodological novelty, even if earlier Humanists and, probably, medieval scribes had already begun to apply it.\(^7\) In this regard he was a precursor of Erasmus and Wolf (see below, pp. 71–74) and was already beginning to overcome the erroneous concept of *emendatio ope codicum*, which implies that collations are made not constantly but only occasionally.

Politian's orientation toward conservative textual criticism, his polemic against the copyist-interpolators,\(^10\) and his tendency to belittle\(^11\) recent manuscripts as copies of a *tvetessitismus* still preserved recur in Pier Vettori, with greater support from arguments and examples.\(^12\) Always disinclined to make conjectures, he is especially hesitant when the old manuscripts unanimously attest a reading: "I scarcely think that all the old manuscripts can be subject to the same error."\(^13\) This was a legitimate hesitation until scholars attained

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7. In turn Laur. 49, 7 derives from Laur. 49, 9 of the ninth century. Politian noticed this too (see the beginning of the same chap. 25), but he did not take the time to supply the evidence: "The fact that this one is copied from that one is clear from many proofs which I shall omit now." The whole question is well clarified by Kienel 1901:400–406.


9. Cf. the passages cited by Retsina 1955:15, 22, 26, 38, 66.

10. Among the many passages that could be cited, see his preface to the *Epistulae familiares* of Cicero (*Vett.* 1566 [1538]:69–70). Vettori writes inter alia: "These perverse corrections have inflicted upon them [i.e., writers] wounds no fewer than those dealt them by time itself and the ignorance of earlier centuries."


12. Vettori 1571:166. Cf. 71: "Since by nature I was always shy about changing anything rashly in someone else's writings.

13. On his edition of the *Agamemnon*, cf. Frantsel 1950:1:34–45. On Vettori's personality in general, which even now is not sufficiently known (some indications in Grafoni 1957:162–70), we expect much more new information from Lucia Cesareini Marinelli's research. On a group of important students and followers of Vettori, who distinguished themselves *inter alia* in *emendationes* of the footsteps of Politian, see Grafoni 1977b:175–76 and, with particular reference to the *Corpus iuris*, before and after Vettori, Caposoli 1969 (Caposoli's fundamental study extends into the sixteenth century as well), and Trope 1971.

14. Erasmus 1538 [1500]:209 (Chilius I, century VI, adage 36). In the first edition (Paris, 1500) this adage is not yet to be found; it appears for the first time in the Venice edition of 1508, as N. G. Wilson informs me.


16. So (or else, certainly erroneously, enim?) the editions of the *Adagia* available to me. The Homeric text (II. 20.308) has καὶ παλαιὸν παλἀκι, τὸι κριν., etc. The accusative *τινῶς* is an adaptation required by the context in Erasmus; the other changes will have been errors of citation from memory.
the Humanists (like the ancients before them: cf. Cicero, *Ad Att.* 16.3.1) meant by the term *archeotypum* or *codex archeotypus* only the "official text" checked by the author and intended to be published afterward in further copies. A wider and deeper examination (Rizzo 1973: 308–17) has made it clear that alongside that meaning (perhaps the prevailing one) the term also has many other usages in the Humanist age, among them the one that will go on to prevail later, 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 lacunae—from which all the others are derived. It is in this sense that Merula, in the preface of his Plautus edition of 1472, applies the term *archeotypum* to the lost *amus liber* [single book] from which the copies still extant of the Plautine comedies are derived (Rizzo 1973: 314), even if he introduces the word with a *velut* (as it were), which gives it an almost metaphorical meaning; and Politian uses the same term in his commentary on Statius's *Silvae* for Poggio's manuscript, which he had seen and judged *mendaces* [full of errors] and *dimidiatus* [reduced to half its original extent]. What is still lacking, it seems to me, before we arrive at the "Lachmannian" usage of *archeotypum* (or of the adjective *archeotypus*), is the *limitation* of the term to the lost ancestors alone and, what is more, to ones distinct from the original or official text. Politian himself applies the term *archetype* more than once to the Pandects first preserved in Pisa, then in Florence, that is, to a manuscript that was still extant and that he thought was one of the official texts Justinian circulated to various cities.

In any case, the importance of the passage of Erasmus we have cited consists not in his application of the term *archeotypum* to a lost common copy—as we have seen, at least Merula had preceded Erasmus in giving this meaning to the term, even if with a somewhat cautious wording—and probably Erasmus too considered legitimate the other meanings commonly given it in the Humanist period—but rather in his energetic affirmation of the right to correct a reading that appears erroneous without allowing oneself to be intimidated by the *consensus codicum* [consensus of the manuscripts] (as, even after Erasmus, Pier Vettori allowed himself to be, as we saw just now): it was not the case that each and every copyist committed the same error independently of the others, by an improbable phenomenon of polygenesis spreading through the whole tradition; instead, it was a single copyist who was responsible for the error, and subsequent copyists repeated it because it


18. See the passages cited by Rizzo 1973: 313 and 3138; and, for other Humanists who were in contact with Politian or underwent his influence, Caprioli 1968: 393–404 and passim.

19. See his letter to Janus Dousa of 1394 (Scaliger 1617: 12): "We have made many observations on authors in both languages, which could give birth to a monstrous progeny of Variants. Old Readings, Miscellanies and other things of this type, with which the ambition of Philologists is nowadays wont to run riot.... But in order for our sleepless nights to bear fruit, we undertook to interpret and purely authors as wholes." This demand for wholesomeness was later developed fully in the great historical and chronological works of his more mature years. Cf. Renays 1859: 16–47.

20. Scaliger 1886 (1777): 4 of the Castigationes; "Moreover, I suspect that Gallic copy was written in Langobardian letters, since the errors that have been disseminated in later manuscripts by inexperienced scribes seem to have arisen precisely from those crabby characters, as we shall indicate carefully in the proper place." By "Langobardian letters" we must understand here pre-Caroline minuscule and not Beneventan script, as is made clear by the fact that on pp. 23 and 73 Scaliger hypothesizes errors due to the confusion between a and a: "because there is no difference between these letters in the Langobardian script" (p. 235); "a and a are the same in Langobardian characters" (p. 75). On the rather broad usage of the term Langobardian letters in the Humanist period, cf. Casamassima 1964: 566–67; Rizzo 1973: 122–23. For other confusions of letters that Scaliger considered significant, cf. below, Appendix B, n. 3.

21. See again Appendix B, n. 3.
century interpolators, who gave particular offense to this enemy of stylistic allurements and love of the austere nature of archaic Latin; but he understood that the old manuscripts too were contaminated by corruptions that had to be healed by conjecture: “Just as more recent editions must be weighed against old copies, just as gold at an assay, so too the manuscripts must be correctly weighed in the scales of judgment.”

And on the basis of the perfectly legitimate hypothesis of an archetype in rather bad shape, he felt himself all too authorized to transpose sections of poems, especially of Tibullus, so as to provide them with a logical order. Caution in recessio, excessive boldness in emendatio: we shall find the same contrast once again in Lachmann. From his Italian predecessors and contemporaries, especially from Ver- toli, Scaliger derived and developed further the demand for a complete collation of the manuscripts in his edition of Catullus and in the edition of Valerius Flaccus he scarcely sketched out; later, in his edition of Manilius, he returned to the practice of merely occasional collations. See, e.g., the Castigationes (Scaliger 1584 [1777]: 105). For Scaliger's fondness for archaism one may recall his Latin version of the Orphic hymns and his famous judgment on Ennius (quoted by Bernays 1831: 284).

In the Progymnasmata Scaliger 1600 (1759): 8.

Heyne 1817 (1755): xviii–xix protested against these transpositions even before Haupt 1874: 154–57; 160–61 did.

On Scaliger, A. T. Grafton (of whom I cited a preparatory work, Grafton 1975) has now published volume 1 of a large-scale complete study: Grafton 1983 (important for Scaliger’s precursors too). The second volume is eagerly awaited. To the disagreement that I had expressed in n. 21 below, Grafton replies courteously, repeating his thesis (1983: 320), and points out to me that the chronological computations Scaliger had to perform for the great works of his second period of activity demanded just as much patience and attention of him as had been required for doing complete collations of manuscripts. This is quite true; but that patience and attention were a conditio sine qua non for the great chronological works to which Scaliger devoted himself, whereas a critical edition could also be prepared by relying solely upon conjectural criticism and neglecting to do a systematic recessio first, even despite an awareness of the risks to which such a procedure would give rise (see below, p. 73). It is beyond doubt that Scaliger’s relations with Verloni and his followers were ruined both by personal reasons and by Verloni’s conservatism as a textual critic (cf. Grafton 1983: 184–85), but it continues to seem impossible to me that this was the reason why Scaliger regressed methodologically and became convinced that systematic collations of the manuscripts were superficial (as opposed to what he had believed when he had edited Catullus). To be sure, as far as I know he never provided a theoretical justification for this regression. Cf. also what I point out in chap. 2, nn. 39 and 47, concerning Ernesti and Wolf. Neither of them repudiated the principles they had espoused with such lucidity, yet they did not have the self-consistency to apply them in all their editions. Cf. also Jocelyn 1841: 66–70.

35. For Catullus, cf. Grafton 1975: 158–61; for Valerius Flaccus, Winkler 1971: 83 and n. 21; for Manilius, Grafton 1975: 174–76. To me it seems implausible to attribute the reason for this methodological regression to the worsening of relations between Scaliger and Italian Classical philologists, as Grafton suggests. On the other hand, it is true that Scaliger’s taste for textual criticism had not weakened at the time of his Manilius edition (Grafton 1975: 175) and already Housman in a letter cited by Grafton, ibid., n. 71. Instead, I would think of a decrease in the patience and powers of concentration necessary for complete collations, which can become weaker even when a scholar is still very interested in problems of textual criticism.


29. Such contradictions are well brought out by Kenney 1974: 57–65 (and they are repeated in Kenney 1980). The characterization of N. Heinssen given by L. Müller 1869:
other Dutchmen, for example, in Jan van Broekhuizen (Broekhuuis). And in general in the seventeenth century, and to some extent still in the eighteenth, it was more in breadth than in depth that the examination of manuscripts made progress. An extreme example of this tendency is the Jesuit Girolamo Lagomarsini, a pure and simple collector of variants drawn from innumerable manuscripts and printed editions of Cicero (whereas another Italian student of Cicero, Gaspare Garatoni, was later to demonstrate a much superior natural talent for Classical philology). With only a few exceptions, which the progress of research may well reveal to have been more numerous, the hints at a history of the manuscript tradition which we have noted in Politian, Erasmus, and above all Scaliger, were not developed very much.

Richard Bentley, the most brilliant Classical philologist between the end of the seventeenth century and the first decades of the eighteenth, and one of the most brilliant Classical philologists of all times, possessed a great capacity for evaluating manuscripts (even if not for reconstructing their genealogy) and for distinguishing genuine readings from interpolated ones.

"No one who knows Bentley well will doubt that a new editor of Horace, once he has eliminated most of Bentley's conjectures (what in fact is not difficult), will find that after him he has almost nothing left to do for the con-

stution of the text": this judgment of Lachmann's, repeated by other first-rate scholars, is substantially true, even if it runs the risk of giving too simplistic a picture of Bentley's mode of operation, in which the recourse to manuscripts (in the case of Horace, often recentiores) and the work of conjectural emendation did not follow first the one and then the other, but were interconnected, and indeed for the most part the latter preceded the former. But Bentley's healthy distrust for the vulgate was illuminated almost a hundred years after his death by none other than Lachmann, who discovered it by starting out from the field in which it was most visible, New Testament criticism (see below, pp. 631, 85f.). For the most part, this aspect of Bentley's activity as a textual critic remained concealed from the Classical philologists who were his contemporaries or immediate successors by another, more conspicuous one: his conjectural criticism, extraordinarily ingenious (just think of his emendations to Callimachus and Manilius), but often rash. He himself contributed to this impression with some of his vigorous pronouncements, like the celebrated "For us, reason and the facts are worth more than a hundred manuscripts," or like the perhaps even more characteristic passage in the preface to his edition of Horace in which he maintains that conjecture, precisely because in it the Classical philologist's personal responsibility is entirely at stake, ends up yielding more secure results than accepting the transmitted reading or choosing between variants does.


152 On this point cf. now, with great precision, Brink 1978: 1.54-48, esp. 1.147-48.

But I believe that Brink exaggerates in trying to rehabilitate Bentley's Horatian conjectures and in maintaining, in the footsteps of Paul Maas, that the transmitted text of Horace is extensively corrupt (see also below, n. 37). Besides, it is not true that Wilamowitz (as Brink reports 1978: 1.44) said that the text of Horace "has no need" of conjectures but rather that it has "very little need" ("verschwendungswürbig"; cf. Wilamowitz 1927: 356; the difference (due perhaps to the Italian translator of Brink's English text?) is not negligible.

153 On Callimachus, cf. Pfeiffer 1976: 133, and Pfeiffer 1949-53: 1.11-12v (see also Hemmerdingen 1977: 490-92). On Manilius, Housman 1937 (1935): xvi-xvi. This is not the place to linger on the splendid conjectural contributions of Bentley to these authors and to many others.

154 In his edition of Horace (Bentley 1711), note on Car. 3.27.5 (for analogous pronouncements by other Classical philologists, cf. Kenney 1974: 420, 99). In point of fact, to those words Bentley added, "especially with the further vote of the old Vatican manuscript." But in any case the text he supported against the text of the better tradition is mistaken.

155 Bentley 1711: 2 (preface): "In these Horatian labors, then, we offer more readings by means of conjecture than with the aid of manuscripts, and, unless I am entirely mistaken, for the most part more certain ones for when there are variant readings, authority
to the first reading offered by just any manuscript, this argument had a cer-
tain degree of truth (cf. Kenney 1974: 72–73); but it tended to set up as the
goal of a critical edition not the historically most probable text but the best
text that the editor's taste and mentality could imagine. I do not believe that,
as some have said, Bentley's edition of Milton's Paradise Lost, which is full
of arbitrary conjectures, is evidence of his senile decline or of his lesser fa-
milarity with English poetry than with Latin and Greek poetry (even though
there may well be some truth in this latter explanation); instead, I think we
should seriously consider Brink's somewhat paradoxical suggestion (Brink
1978: 1163–64) that Bentley's Miltonic conjectures, misguided on the level
of textual criticism, were an indirect form of literary criticism, opposing a
different taste to Milton's and to the corresponding poetic language. But, in
my opinion, something similar also happens in Bentley's edition of Horace,
even if to a lesser degree; in the overwhelming majority of cases, Bentley's
hundreds of conjectures on Horace are "corrections" not of the transmitted
reading but of the poet; and many of them betray a lack of understanding of
that element irreducible to pure rationality in the strict sense, which is in-
herent in any poetic language, in quite different forms and degrees.\textsuperscript{37}

\textsuperscript{37} itself often deludes people, and encourages the deplorable itch to emend; but when con-
jec tures are proposed against the testimony of all the manuscripts, not only do fear and a
sense of shame tweak one's ear, but reason alone and the clarity of the meanings and ne-
necessary itself dominate. Furthermore, if you produce a variant reading from one manu-
script or another, you achieve nothing by claiming authority for one or two witnesses
against a hundred, unless you believe it with enough arguments to settle the matter on
their own almost without the testimony of a manuscript. So don't worship scribes alone:
no, venture your own wisdom, so that it is only when you have tested on their own the in-
dividual points against the general drift of the discourse and the character of the language
that you pronounce your opinion and deliver your verdict." On the value assumed in the
Enlightenment by Horace's sapere aude, see Venturi 1559; but the pious Bentley, a fer-
cious adversary not only of atheists but also of deists (see below, p. 651), limited his own
"Enlightenment" to textual criticism.

37. I have lingered a bit on this point because some writings about Bentley (Gould
1963; Shackleton Bailey 1965; Brink 1978: 1087–1064) that add new points of view and
very intelligent considerations nonetheless tend toward an indiscriminate exaltation of all
of Bentley, as though his greatness would be diminished by any recognition of the limits
possessed by every scholar, even the greatest. This is particularly noticeable in the essay by
Brink, who in another respect is the very one who has dug most deeply into Bentley's per-
sonality. Many critics of Bentley's conjectural boldness can be accused of a myopic con-
servatism, but it would be difficult to nullify the observations of a Housman in this way:
but see what he says in the already cited preface to his edition of Manilius (Housman 1937
[1903]: xvi intro–xvii) concerning the "faults" of Bentley's Manilius, which are "the faults
of Bentley's other critical works"; and he repeats analogous reservations in the preface to
his edition of Lucan, Housman 1927 (1926): xxii–xxxii. Bentley gave his worst perf-

The English Classical philologists of the second half of the eighteenth century and the first half of the nineteenth who were inferior to Bentley in
talent and breadth of horizon but nevertheless followed his powerful ex-
ample in textual criticism (Musgrave, Porson, Dobree, Elmsley) were above
all conjectural critics, endowed with a refined knowledge of linguistic and
metrical usage, especially regarding the recitative parts of Greek tragedy and
comedy. But they also felt the need to check the manuscripts. If Porson "was
conditioned by the fact that he never stirred from England, where only re-
centiores manuscripts" of Euripides\textsuperscript{38} and the other tragedians existed, be-
fore him Samuel Musgrave ventured as far as Paris and collated two impor-
tant manuscripts of Euripides there, and after him Peter Elmsley went to
Italy, studied the Laurentian manuscript of Sophocles (he was the first to re-
ocnize clearly its superiority),\textsuperscript{39} and collated and evaluated Vatican manu-
scripts of Euripides, for the most part correctly.\textsuperscript{40} To Elmsley we also owe
the suggestion that all the manuscripts of Aeschylus derive "from the same
copy, which appears to have survived alone the general wreck of ancient lit-
erature."\textsuperscript{41} We already know that the concept of an archetype goes back
three centuries before Elmsley. All the same, his strictly "medieval" concep-
tion of the archetype of Aeschylus, as the sole manuscript to have escaped a
"shipwreck" that befell civilization, is interesting because it anticipates
Madvig's and Lachmann's formulations. It is altogether another matter that
one can no longer think today of an archetype of this kind for the text of
Aeschylus.

But my wish not to separate his English followers from Bentley has led me
to jump too far ahead in my story. We must now take a step back, in or-
der to show how New Testament philology gave rise to great progress in
the methodology of textual criticism.\textsuperscript{42}
The Need for a Systematic Recensio
in the Eighteenth Century

As we have suggested, it was above all the study of the Greek New Testament that made the technique of recensio progress beyond the point it had reached with Scaliger. This was observed by Giorgio Pasqualli 19342 (1934): 8: "with regard to recensio, philologia profana [. . .] is still, without knowing it, a tributary of philologia sacra"; he also indicated the reasons for it. The New Testament has an extremely rich manuscript tradition; conjectural criticism can achieve little or nothing; hence the problems of choosing among the innumerable variants and assessing the different degrees of authority of the manuscripts moved to the forefront. And here every question of textual criticism aroused a particularly lively interest, since it went beyond pure philology and implied, or at least could imply, questions of theology.

The editio princeps of the Greek New Testament, edited by Erasmus, was one of that great Humanist's least successful editions, for he prepared it in haste and based it on Byzantine manuscripts of little value. But here too that phenomenon occurred that we described at the beginning of chapter 1: most of the subsequent editions reproduced the text of the editio princeps, with some contamination. One of these editions, the so-called textus receptus, published by Elzevier of Leiden (1624, 1633), had an enormous diffusion and was adopted by the Protestant churches. From then on it was

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1. Wassink 1979: 75–77 has observed that for the most part Erasmus's original contributions to textual criticism should not be sought in his editions of Classical texts, prepared hastily for the printers’ use, but must be gleaned from his Latin translations of Greek texts.

2. Gregory 1901–1903: 2:37–42 (still fundamental). There is a more concise but very clear exposition in Hendriksen in Werner-Wels 1882–1903: 2:68–9. More over 1968 (1964): chapters 3 and 4 (see also the addenda at the end of the second edition) are rich in information and very up to date, but the author does not characterize the individual personalities of the New Testament critics of the eighteenth century distinctly enough, and, as chapters 6 and 7 demonstrate, he does not always have a clear understanding of the principles and methods of more recent textual criticism to a certain extent his historical exposition is thereby also impaired.

3. Pasqualli 1934: 9; and see all of his fine tribute to Wetstein, which concludes: "Even in a field as technical as textual criticism, the greatest discoveries are for the most part the work of men of noble spirit."


5. See below, n. 43.
a remarkable philologist from Verona who remained almost completely isolated, so that in Italy that prejudice lasted even into the nineteenth century: many of the readings in Plautus that Tommaso Vallauri defended were vulgar readings of very little or no documentary authority, or even quite recent conjectures, to which Ritschl often opposed not his own conjectures but readings from the Ambrosian palimpsest (Milan, Biblioteca Ambrosiana G 82 sup.). The most stubborn defenders of the *receptus* were Protestants, but the very spirit of the Reformation encouraged the textual criticism of the New Testament and consequently that of Classical texts too. Wettstein always appealed against his persecutors to the principles that had inspired the Reformation. He understood that those principles remained alive not in the restrictive dogmatism of the great Protestant churches (Lutheran, Calvinist, Anglican) in the countries where they had won the victory and been recognized by the political powers or were even identified with them, but rather within the *heretical* currents of Protestantism itself, in developments that were rationalistic or, often, simultaneously rationalistic and mystical, but of a mysticism that was not contemplative and inert but subsersive. It was to such currents that the principal New Testament critics belonged. Jean Le Clerc was an Arminian, as were already two other men whose characters were stronger than his and whose interests were wider, Gerhard Johannes Vossius and Grotius, who both found the time to make distinguished contributions to various kinds of philology, including Classical philology (but this was especially true of Grotius, despite his dedication to law, to theology, to active politics); Wettstein was a Socinian, or at least was suspected of Socinianism; Semler was strongly rationalistic; even the more timorous Lutheran Bengel was a Pietist and adhered to millenarian tendencies in his commentary on the Apocalypse, exerting an influence in this direction on the English Methodists. In comparison, the Catholics, except for one distinguished heterodox, Richard Simon, contributed very little to the criticism of the Greek New Testament in the seventeenth and eighteenth centuries; and textual studies of the Latin Vulgate as well ceased almost entirely once the Sistine and Clementine editions had established a text with eclectic criteria.

8. On the Sistine and Clementine editions, see Quentin 1946: 18–20. On attempts undertaken in Catholic milieu to base editions of Patriotic texts upon systematic collations of manuscripts, cf. Petrimonio 1966 (accurate and intelligent, but with some apologetic exaggerations). Rudolf Pfeiffer’s Catholic viewpoint prevented this distinguished and much lamented scholar not only from understanding the development of New Testament textual criticism, but even from narrating it, even after its importance for the history of Classical philology had been pointed out: in Pfeiffer 1976, Bengel, Semler, and Griesbach are not even named, while Wettstein is merely alluded to once, insignificantly, and Le Clerc is in a certain sense restored to the mainstream of Catholicism thanks to his edition of the works of *Esdræmon* (179).

9. See, e.g., below, n. 38. After so much neglect of Simon as a textual critic of the New Testament, it is pleasant to read the claims made for him by Reynolds-Wilson 1991 (1968): 188; yet, with regard to the methodology of textual criticism in the strict sense, they go too far. Chapters 29–32 of Simon 1689: 336–416 contain not so much innovative methodological criteria as rather important individual observations [e.g., 350 on the value of the so-called *codex Bezae of Cambridge* combined with other assertions that are still quite old-fashioned: e.g., Simon—who had indeed observed the process of belittlement in the transmission of texts, as we shall see in a passage cited later—declares that he prefers the “simpler” reading compared to the one that “contains an expression that seems more forceful” (177; that is, he prefers the *lectio facilior*), and he still believes in the criterion of the majority of manuscripts (ibid.). It is intolerable to claim that Bentley’s *Proposals* (below, p. 65f.) “scarcely mark any advance” beyond the work of Simon (Reynolds-Wilson 1991 [1968]: 187).
The Need for a Systematic Recapitulation word for word, but "whole phrases, or in order to save time they even read whole sentences and only write them down afterward" (1730 [1697]: 2.2). He requires that a conjecture be able to explain the genesis of the corruption (1730 [1697]: 2.277), but he does not establish this as an absolute requirement ("if it can be done"); indeed, he admits (1730 [1697]: 2.278 and esp. 9) the existence of "inexplicable" corruptions, since the copyist or, what amounts to the same thing, the person who reads aloud to him may even have substituted for the text of the model completely different words referring to thoughts that occupied his mind at that moment—we are not far from the "Freudian slip," indeed, we have gone too far beyond it in a certain respect, since a such a substitution would not have been facilitated by similarities of sound or sense, by what Freud will call Begünstigungen [favoring conditions]. He has no interest in the genealogy of manuscripts but limits himself in general to preferring the oldest manuscripts (1730 [1697]: 2.293); but we should not forget that it was only later that the rehabilitation of the recentiores became justified and fruitful, and that the first, necessary stage had to be one of distrust for the all-too-often interpolated manuscripts of the Humanist age. We shall refer shortly to Le Clerc's contributions regarding other editorial criteria (usus scribendi [the author's habitual style], lectio difficultior [the more difficult reading]): in this way we shall see even more clearly how the Ars critica (which, let us recall, had many editions and hence a broad diffusion) paved the way in large measure for the immediately subsequent development of New Testament textual criticism.b

But history follows winding roads, even the history of a limited problem; and so the first project of editing the New Testament which overcame the general conception of the textus receptus was due not to one of the religious reformers to whom we have referred and to whom we shall return later, but to Richard Bentley, a man as brilliant and bold in philology as he was orthodox in matters of religion. And it was, precisely, a religiously orthodox aim that inspired him, besides the philological problem itself: he wished to defend the authority of the biblical text against the "free thinkers" (in reality not atheists but deists) led by Anthony Collins. For free thinkers, the existence of so many variants in the New Testament manuscript tradition, such as had been amassed by Mill in his edition of 1707, already mentioned (above, p. 59), was an argument against the Gospels' authenticity and truth; for Protestant theologians of strict observance, it was a reason to fear a similar polemical use of the text's "uncertainty" and hence to refuse to part company with the receptus; for Bentley, it was an incentive to establish the text more solidly and thereby to defeat skepticism.11 So he planned an edition

10. Le Clerc 1730 (1697): 2.269, Le Clerc adds: "For what writer has ever polished sentences so perfectly that the subject matter can never be expressed better?" Wilanowicz 1983 (1918): 173–74 will make an analogous observation; he regards this as an achievement of recent textual criticism, which has overcome the "demand for absolute perfection implicit in the canonical authority of antiquity." Cfr. also Le Clerc 1730 (1697): 2.10–11, 259, and elsewhere.

11. See the polemic against Collins in Bentley 1713 = Bentley 1856–58: 5.287–368 (esp. 347–61).
based on a comparison of the oldest Greek manuscripts with the Latin Vulgate and the citations in the Patristic texts, which ought to have restored for us the state of the tradition as it had been at the time of the Council of Nicaea. He recognized that recensio had to take precedence before conjectural criticism in a textual tradition that was so rich and ancient (Bentley 1836–38: 3,488). But even though, as we have seen, his project was dictated by intentions that were anything but subversive in religious matters, it nonetheless encountered the theologians’ opposition. And so Bentley ended up giving up this plan, also because of his commitment to other projects and because of the very difficulty of completing so enormous a task.

There were certainly more practicable ways to improve the receptus. One could draw upon Mill’s apparatus and introduce more reliable individual readings into the text; in certain cases, one could also have recourse to conjectures. The first procedure was already followed in 1799–19, that is, before Bentley’s Proposals, by the theologian and mathematician Edward Wells; the first and second ones together, by the Presbyterian Daniel Mac. Both improved the receptus very notably; they performed a courageous and philologically valuable deed. Nonetheless this was still just occasional corrections, ope codicum and, more rarely, ingenii. Bentley’s project was methodologically more innovative, because he intended to set aside the receptus altogether and to refer constantly to the manuscripts. It was along this path that Johann Albrecht Bengel and Johann Jacob Wettstein, the two greatest New Testament critics of the eighteenth century (whom we have already mentioned for their religious positions), moved and made further progress. More cautious than Wells or Mac regarding interventions into the text (not least because in continental, Calvinist and Lutheran Europe,


13. Part of the material he left unpublished, now preserved in the library of Trinity College, Cambridge, was later published by Ellis 1864.

14. Metzger 1968 (1964): 109 and n. 1. I have not been able to inspect the edition of Wells.


this was much riskier than in Anglican England), they were more acute regarding theoretical questions.

Each felt needs unknown to the other: hence their polemics and their mutual incomprehension. Bengel has the merit of having been the first to try to determine the relations of kinship among manuscripts: “Manuscripts are closely related to one another if they have the same ancient arrangements of text on the page, subscriptions, and other subsidiary features.” Besides these kinds of evidence he also argued on the basis of shared readings; though he did not yet go so far as to distinguish between shared corruptions, the only truly probative evidence, and shared correct readings. He imagined that in the distant future the whole history of the New Testament tradition could be summarized in a tabula genealogica, that is, in what will later come to be called a stemma codicum. What is more, Bengel also saw lucidly that such a genealogical classification would furnish a secure criterion for choosing among the variants, thereby allowing editors to overcome the old and

16. Bengel 1765a (1734): 18. The 1763 volume also contains (pp. 625–932) Bengel’s other writings on New Testament textual criticism, including the Prodrömox Novi Testamenti Græci recte conturto adnotandœ (already published at the beginning of Bengel 1755). Bengel also edited Cicero’s Epistulae familiares and some Patristic texts besides the one just cited. On him cf. Newe 1893 (useful, despite the irritatingly apologetic tone); Noelle 1913; and now Mälzer 1970, an ample monograph that, however, discusses Bengel as a theologian much more fully than as a textual critic (in any case see chap. 6, useful for various epistolary testimonia). On the expression apparatus criticus (used by Bengel, perhaps for the first time) and on his use of symbols (Greek letters, which however indicate not manuscripts but “degrees of value” of the various readings), cf. Kenney 1974: 156 and n. 41 for the symbols, also Mätzer 1968 (1964): 115 and Mälzer 1970: 162. Wettstein will be the first one who uses symbols to indicate the manuscripts (capital letters for MSS in uncials, Arabic numerals for the MSS in minuscules); cf. Metzger 1968 (1964): 114 and, for a precursor in this usage (Savile 1861, Kenney 1974: 137 infra). In general, on the development of the technique of critical editions, see Kenney 1974: 152–57. But see also, for the Humanist age, Rizzo 1973: 301–25 and the passages listed in the index (p. 390) under “sigle per indicare mss.”

17. Bengel 1765a (1734): 18: “but if the readings themselves are collateral, they tend to go together”; he goes on to cite various groupings of manuscripts.

18. Bengel 1765a (1734): 20. I will not cite the whole passage, already quoted by Gregory 1900–1909: 4,008 and by Pasquali 1932a (1954): 9. Bengel added: “Magnus constitutanea nostra silvam habent: sed manus de tabula, ne risum periculo expunat veritas [Our conjectures are based on a lot of material: but hands off, lest the truth be exposed to the risk of laughter]. Hence he believed that the attempt had to be postponed to better times, Manus de tabula, with aner understood, is a well-known proverbial expression in Latin to say “Snoop” “Enough!”. Bengel uses it with the traditional meaning, but at the same time he alludes jokingly to the tabula genealogica of which he has just spoken.
deceitful criterion of the majority of manuscripts. "Two or more groups, often agreeing, are worth the same as one; two or more manuscripts of a single group are worth the same as one when they agree with one another. But when they disagree with one another, a group or a manuscript agreeing with many does away with the present error of its comrades (i.e., with the error of its present comrades);" hence the important thing is not that a reading be attested by the majority of the manuscripts but by the majority of the families; only within each family does the majority of the manuscripts have a value for reconstructing its ancestor's reading. This is already the procedure that Lachmann will later develop, and that Paul Maas (1958 [1927]: p. 6, sec. 8) will call eliminatio lectionum singularum [elimination of unique readings]—an infelicitous expression, but we too will use it for lack of a better one! a procedure to follow whenever the tradition is not too contaminated. Further on Bengel repeats even more explicitly that it is the consensus of manuscripts belonging to different families that guarantees the antiquity of a reading.Obviously, in a tradition as contaminated as that of the New Testament Bengel could not apply these criteria immediately (nor could more recent scholars do so); they became fruitful only when they were applied to simpler and more mechanical traditions. What is more, Bengel's fear of controversy (which in fact broke out immediately) and persecution led him to refuse to accept into the text "even one syllable that had not already been

10. Bengel 1763a (1734): 11: "the present error of its comrades" must mean a difference of reading that is found in a group of manuscripts existing now but that was not found in their model.

20. Bengel 1763a (1734): 65: "But a difference among the witnesses closest to the source, the first hand, and most distant from one another, does have value; in this way they reveal the genuine reading by their agreement." Ibid., 68: "If that agreement embraces a diversity of manuscripts, all doubt will be annulled." It is apparent from the whole context that Bengel understands this "distance" or "diversity" more in the sense of belonging to different families than as geographical distance. So too later, Grünbach 1796 (1774): 1.1xxii: "And yet if those witnesses which can really be considered different agree with one another in a friendly way, that should finally be considered an agreement which lends authority to them." Nonetheless, since Bengel distinguished a natio Asiatica and a natio Africana in the New Testament manuscript tradition, the "distance" came to assume a geographical meaning as well, as already in a reference by Bentley and later, more explicitly, in Lachmann: see below, p. 85f. The geographical meaning becomes more explicit in a later writing of Bengel's, Bengel 1765b (1734): sec. viii, regula vii: "But these manuscripts were diffused through the churches of all the ages and climates (et climatum), and in spite of the multitude of variants they come so close to the original text that they show the genuine reading all together." Here clima, as already in post-Classical Latin, can only mean "zone," "region."


24. Wettstein 1733: 235, and even more 1734: 226-28; Wettstein 1733: 32: 1.166-67. Even before Wettstein, the criterion of the majority of manuscripts had been codified by Maurusch 1721: 53. Wettstein had no particular faith in the oldest manuscripts, agreeing with Bengel in this point (see below, n. 35); this was the principal reason for his gradual separation from Bentley.

** On Wettstein there is only one point I am anxious to emphasize here. As it is well known, the edition of 1733-35 represents a refusal (perhaps a forced refusal) to insert into the text the readings differing from the receptus, which Wettstein considered better. But the Prolegomena of that edition are so spoken, felicitously inconsistent with that refusal: the attack on the receptus is repeated and developed there, the polemic against Bengel's reliability is repeated with a forceful, sometimes even excessive, tone, the accusations of "impiety" are rejected with undiminished energy. My citation on this page is at n. 35 already indicated this, but did so too hastily. On Wettstein's precursors regarding the formulation of internal criteria for the selection of variants, editorial technique (the use of signs), and many other methodological principles, Armando Golio has collected a considerable amount of
Nonetheless, Wettstein's Prolegomena of 1730, which represent the most interesting phase of his thought, assigned the first place in the choice of readings to internal criteria: usus scribendi and lectio difficultior. He and Bengel were in agreement on this question. It is only when two readings are equivalent in themselves, declares Bengel, "that the decision is referred to a more accurate examination of the manuscripts"; a position opposed to that of Lachmann, who will recur to indicium [judgment] only when two readings have the same external authority.

One of these internal criteria, the usus scribendi, was already well known to the ancient grammarians; the philologists of the fifteenth to seventeenth centuries then made ample use of it, even if they employed it perhaps more for conjectural emendation than for choice among variants. Sporadic anticipations of the criterion of the lectio difficultior can also be found from antiquity until the seventeenth century; as far as I know, the first to formulate it precisely was Jean Le Clerc. Thus Wettstein and Bengel found the material and have evaluated it intelligently; I hope that the results of his researches will be published soon. Another field in which Godzio has enriched, modified, and also corrected many of the matters discussed in this work of mine is the conceptual and methodological contribution made by Orelli and Madvig, which now turns out to be more significant than what I had indicated (see below, pp. 90f., 99f., 102f.), even if the importance assigned to Madvig was a point to which I was particularly committed.

Bengel 1763a (1734): 18. This position is now reaffirmed by Waszink 1976: 87, with good reason.

Especially Aristarchus Lethe 1882: 314–16; Pasqualli 1952a [1934]: 233, 240–41; Pfeiffer 1968: 247–28 (but one might still have expected to find something more here). Aristarchus certainly applied the idea of usus scribendi excessively, hyper-analogically.

27. It is asserted as a criterion for conjectural emendations, e.g., by Le Clerc 1730 (1697): 2:170–83.

28. E.g., in Galen, Medici Opera, ed. Kühn, 18.1.1005; 17.1.98, 101, 110 ("this was the ancient reading, but it was altered by many interpreters in order to make it clearer"). But Galen uses this internal criterion only so as to confirm the authority of the oldest manuscripts, which remain the fundamental principle for him. Some further details on Galen's procedure are furnished by D. inner in the German edition of my book, Timpanaro 1972: 19. Probus (in Servius excerpt on Virgil, Arnei 12.605) followed the criterion of the most archaic reading (which is a special case of the lectio difficultior) in order to prefer floruit to ft vento in one passage of Virgil. I cannot bring myself to believe that this is an archaism that Probus arbitrarily introduced; I hope to discuss this passage in more detail elsewhere. In the Middle Ages a reference to the lectio difficultior is found in Imerius cf. Kantorowicz 1931: 31. In the seventeenth century, Simon 1680: 173–76 800 observed that copies tend to banalize, but he did not derive from this observation an explicit criterion for choice among variants.

Le Clerc 1730 (1697): 2:292: "If one of them [i.e., the readings] is more obscure and the others clearer, then the more obscure one is likely to be true, the others glosses." The only defect of this formulation is the too restrictive character of the concept lectio facilior terrain already prepared for them; but it was left to them, and particularly to Wettstein, to develop more fully the theoretical assertion and practical application of these two norms. It was only later that Wettstein, preoccupied by accusations of subjectivism in the choice among variants, ended up adhering above all to the criterion of the majority of manuscripts, but without ever repudiating internal criteria.

In the second half of the eighteenth century another New Testament critic, Johann Salomo Semler, distinguished between ausserliches and innerliches Alter [external age and internal age], that is, between the antiquity of a manuscript and the antiquity of the readings attested by it: a manuscript that is more recent than another one can preserve readings that are more ancient. Bengel had already noticed this, as others had even earlier, but not or clarior, which for Le Clerc always had its origin in a marginal gloss that had intruded into the text and substituted for the original reading, or at least in conscious banalizations; yet at least as often, if not more often, the origin is an unconscious banalization.

Bengel 1763a (1734): 17: "Where the one [sc. reading] is more easy, the other less so, the one that is old, weighty, brief, is preferred; the one that charms us by its greater per- spicacity and fullness, as though it had been introduced deliberately, is generally set aside."

A fuller discussion is in Wettstein 1730: 179, 184 (on lectio difficultior) and 188 (on usus scribendi, from which he rightly distinguishes the repetition of a passage with identical words, which is suspected of levelling and is therefore to be rejected in favor of the "varied" expression). I shall not cite in their entirety the passages of Wettstein, which are already quoted by Pasqualli 1952a [1934]: 10–12. It should be noted that already in Bengel, and then in Wettstein and Gräfenbach, and even in recent manuals, the lectio brevior appears as a subspecies of the lectio difficultior—but in fact the lectio brevior is a much more uncertain criterion, since if the fuller reading can derive from the desire to make the text clearer or from interpolations of various kinds, the brief reading can be caused by omissions (Dain 1975 [1949]: 301, especially by unconscious elimination of words not strictly necessary to the context yet still present in the authentic text: cf. Timpanaro 1976: 33–40; other examples in Ripello 1977: 120–2). In this point, Le Clerc proved himself to be more cautious, for in his epistle inserted into Kistner's edition (cf. above, p. 61) he had maintained the authenticity of two words that are not strictly necessary in part of the tradition of Matt. 1:11 and that are absent in other witnesses, "for there was no reason why these words should have been added, for they are obscure and add nothing to clarify the meaning of the passage: on the contrary, for these very reasons they could have been eliminated as obscure and useless." At already in the case of the lectio facilior, Le Clerc speaks of an intentional alteration, which is not the most frequent case; but in itself his argumentation is entirely correct and demonstrates that the lectio longior can even be the lectio difficultior.

31. The Animalversusiones et cantiones for choice among variants are no longer to be found in the much fuller Prolegomena to Wettstein 1751: 54, but they are republished separately at the back of the edition.

32. Semler 1757: 88–89, Semler 1765: 368 also polemicizes against the criterion of the majority of manuscripts but does so only very briefly. In this regard he makes no progress beyond Bengel, but a little later Ernesti will go another step in the criticism of "pro-
with such clarity." 33 In the end Johann Jacob Griesbach summarized the results of earlier criticism in a didactically perfect form in the Prolegomena to his second edition, 34 but although by now he fully recognized the inconsistency of the receptus, he too did not free himself from it courageously enough.35,36,37

In the meantime, Classical philologists had noticed that they had fallen behind the theologians in textual criticism. In 1730 Wetstein (1730: 166)...

could still point to the critics of profane texts as an example that students of the New Testament should follow; but by 1770 the roles had been reversed after the works of Wetstein himself, Bengel, and Semler, and Johann Jacob Reiske wrote, "We should not treat profane authors with less scrupulous veneration than the New Testament. For the very same reason that we carefully collate manuscripts of the New Testament, it is only fair that we inspect the manuscripts of Demosthenes and all the other ancient authors too and dig out and publish their readings. For this is the only way to demonstrate the historical truth of any text, be it sacred or profane, on the basis of the consensus of many ancient manuscripts of approved reliability."

In fact, Reiske did more for the text of the Attic orators and of Atticists like Libanius with his splendid conjectures than by investigating the manuscript tradition (many of his conjectures were later confirmed by manuscripts of which he had no knowledge).38 But the need to use the manuscripts as the text's constant foundation instead of only making occasional collations was reaffirmed a little later with great clarity by Ernests in the preface to his edition of Tacitus, and once again by Friedrich August Wolf at the beginning of his Prolegomena ad Homerus. 39 Each of these scholars observed correctly that to follow the old method of having recourse to the manuscripts only where the vulgate was not satisfactory resulted in leaving in the text a large number of small corruptions and lectiones faciles that, for better or worse, made some sense and hence did not arouse suspicion. Wolf writes: "A true, continuous, and systematic recension differs greatly from this frivolous and desultory method. In the latter we want only to cure indiscriminately the wounds that are conspicuous or are revealed by somet..."
der the witnesses for every reading, not only for those that are suspect. It changes, only for the most serious reasons, readings that all of these approve. It accepts, only when they are supported by witnesses, others that are worthy in themselves of the author and accurate and elegant in their form. Not uncommonly, then, when the witnesses require it, a true recension replaces attractive readings with less attractive ones. It takes off bandages and lays bare the sores. Finally, it cures not only manifest ills, as bad doctors do, but hidden ones too.” 39 Only a procedure of this sort deserves the name of recentio and not of mere recognitio (F. A. Wolf 1895 [1795]: 45); and only after a systematic recentio will one be able to go on to conjectural emendatio, for which Wolf did not feel much sympathy anyway. 40 Thus it was that the old concept of emendatio epe codecumus was completely overcome.

But Wolf’s recognition of the need for a systematic recentio and for a repudiation of the vulgate is not accompanied by a too exclusive faith in the most ancient manuscripts. It will have been Semler 41 who positively influenced him in this recognition of recentiores non deteriores [the more recent manuscripts are not the worse ones], which, as we know, is in itself not new, but it is worth the trouble to quote Wolf’s own formulation (F. A. Wolf 1895 [1795]: 46): “For newness in manuscripts is no more a vice than youth in men. In this case, too, old age does not always bring wisdom. Insofar as each follows an old and good authority well, it is a good witness.” The comparison between a manuscript’s “youth” and a human being’s is little more than a graceful witticism, but the final phrase explains well why a recentior can on principle be not at all a deterior: the recent copyist may have copied an ancient and good manuscript well (and, Wolf seems to imply, directly). 42

So too, during the last three decades of the eighteenth century, the need for a genealogical study of the manuscripts, which we have seen Bengel assert in the field of New Testament studies, gradually spread among Classical philologists. Ernesti clarified even better than Bengel had done the principle that several manuscripts deriving from the same ancestor had the same value as only one; 43 Christian Gottlob Heyne and the Alsatian Jean Schweig...

39. F. A. Wolf 1895 [1795]: 43-45. Cf. also Wolf’s preface to his edition of Plato’s Symposium (F. A. Wolf 1821: vii – F. A. Wolf 1869: 1:135-40): “But this cannot happen if manuscripts and old editions are only checked and compared occasionally, for individual obscure or apparently erroneous passages.” But so already Ernesti 1821 (1772): vi: “In previous ages, those who set about to edit ancient writers thought it was enough to check manuscripts and printed editions in those passages where they got stuck […]. In this way […] they ended up leaving many things untouched that could have been extended from those same manuscripts and printed editions. And yet no one is so sharp-eyed that he can see all the faults of the vulgate reading on his own, and not sometimes approve corruptions as though they were correct.” In Ernesti, however, these fine theoretical pronouncements almost never found any practical applications; both his edition of Tacitus and his (better) edition of Cicero are based in substance upon preceding editions, not upon manuscripts, as C. G. Zumpt 1831: iii-xxvii very rightly observed regarding Cicero. From this point of view, his edition of Callimachus (Ernesti 1786) is better; in the preface (fol. 5b) Ernesti asserts that all the non-interpolated manuscripts of the Hymnus are derived from a single lost model, on the basis of their agreement “in lacunas and in readings” (cf. Pfeiffer 1949: 53: 2.1v).

40. “A pleasant pastime” is what he calls it at the beginning of the Prolegomena: F. A. Wolf 1895 [1795]: 45 (cf. 44) and F. A. Wolf 1869: 1:143. All the same, Wolf is not an un-critical conservative; echoing Bentley’s famous phrase (above, p. 55), he admits that one must prefer “talent” to “treasure chests full of parchments” (1895: vii), but he maintains that emendation should not precede recension nor, even less, take its place; and, like Ernesti, he insists on the importance of emendation for healing “latent errors” (see above). But we should bear in mind that although Wolf is speaking in general terms in the Prolegomena, he is thinking above all of the Homeric text, one of the very few from antiquity that have no need of conjectural criticism (except for the problem of the interpolated verses). He will later judge emendation more favorably, in F. A. Wolf 1869: 2:82: “… of the more ancient [of the manuscripts]”.

41. See above, p. 69, and n. 38 for the relations between Wolf and Semler.

42. Cf. Kantrowic 1941: 21-32; Pasquini 1952a [1954]: 46. Their arguments are formulated more clearly and fully than Wolf’s but are not substantially different from his.

43. Ernesti 1801 [1772]: xxviii: “When manuscripts are involved, one should make sure that we have not as many in number as possible, but as many of those that possess as it were the legal right to give an opinion. … For if you have a hundred manuscripts of the same book, but it is certain that they are derived from a single apologist, then all together they only have the right and force of a single book.” Grisebach 1796 [1774]: 1:xxxii will express this concept with very similar words. But although Italy was a philologically quite “depressed” milieu (in which, nonetheless, Verona constituted an exception), Domenico Vallarsi had already enunciated this principle there in the preface to his edition of Saint Jerome (Vallarsi 1766 [1734], reprinted in Migne 1845: p. xxix, para. 35): manuscripts that agree in errors or in arbitrary changes (criticisme aequi) are worth “not more than one manuscript”; evidently Vallarsi wrongly neglected the possibility of contamination when he considered the arbitrary acts of critics. And above all, textual criticism of Dante in the second half of the eighteenth century possessed at Verona itself a philologist of European standing in the figure of Bartolomeo Perazzini (Perazzini 1755), to whom Volena 1865: 65-69 has justly called our attention. Perazzini, besides repeating the aforementioned genealogical principle (probably under Vallarsi’s influence), also wrote passionate polemics against the defenders of the vulgate that have much in common with those of the New Testament philologists who were his contemporaries or a little earlier than he. This similarity is increased by the fact that the Divine Comedy was considered a “sacred text” too for a complicated series of religious and artistic reasons, though to a much lesser degree than the New Testament. Perazzini speaks explicitly of a “prudence of a sacred text” (1755: 16), and he repeats: “not should any text be sacred, unless it has first been perfectly emended”; and again: “Hence it is not I who should be called an innovator, but rather those who altered the text which was once received” (i.e., in this case, not the recension in the sense of the New Testament, but the oldest stage of the tradition, the antepasa lectio, as he says immediately before).
häuser tried to reconstruct genealogies, the former for the manuscripts of Tibullus, the latter for the *Manual of Epictetus*. To be sure, these scholars' attempts turned out quite imperfectly, not only because of their inexperience and incomplete knowledge of the manuscript material but above all for the objective reason that even today makes it impossible in so many cases to trace a *stemma codicum*; contamination. Heyne—a philologist whose greatest originality certainly did not consist in textual criticism but who even as a textual critic is more valuable than is generally said—was well aware of this phenomenon of contamination; for the New Testament, Griesbach was too.

So too, the manuscript tradition of Homer was too contaminated to permit the fulfillment of Wolf's proposal that the manuscripts be organized “into classes and families.” But thanks to his use of the Venetian scholia discovered by Vullois, Wolf was able to achieve something else in his *Prolegomena*; the history of a text in antiquity. In this way he prepared the way not for Lachmann, but rather for Jähn's and Wilamowitz's concept of *Textgeschichte* [history of a text] and for all the nineteenth- and twentieth-century studies on "ancient variants and ancient editions" (to repeat the title of one of Pasquale's chapters). For Wolf, the Homeric question itself was nothing more than the first phase, oral and popular, of the history of the text of the Iliad and Odyssey; in the *Prolegomena* it is only discussed in these terms and not as a problem in literary history.

44. Heyne 1817 (1555): xii–xlviii, but what Heyne provides is in the first instance a genealogy of the editions of Tibullus, and only secondarily one of the manuscripts. Schweighäuser 1798: preface: to classify the manuscripts, Schweighäuser relied on shared corruptions less than on the reciprocal arrangement of the text of Epictetus's *Manual* and of Simplicius's commentary on it (cf. Timpanaro 1955: 70). Schweighäuser had more merits in the *eliminatio codicum descriptorum*, see below, p. 99.

45. Heyne 1817 (1755): xx, xxxvi (where he speaks of "apographs, perhaps prepared with others, or made out of them"). Heyne's fame as a textual critic, and as a Classical philologist in general, was impaired by the scornful tone adopted toward him by his students Wolf and Lachmann (and, in a less technical field, Friedrich Schlegel), in great measure unjustly; for Lachmann in particular, see Lachmann 1876: 1:106.

46. Griesbach 1796 (1774): lxviii: "The readings of the one recession have been introduced into the manuscripts of the other family," etc. Already Semler (1767) had often observed that in different passages the very same manuscript belongs to different "recessions."

47. F. A. Wolf 1885 (1795): 44. In his editions of other authors (Plato, Cicero, etc.), which in certain cases would have allowed an application of the genealogical method, Wolf limited himself to hasty *recognitiones* in contrast with his principles.

48. Simon 1686 had already arrived at this concept of a "history of the text," but he had had no followers among Classical philologists. C.L. for now, Pfeiffer 1976: 190.


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3

The First Phase of Lachmann's Activity as a Textual Critic

After the considerable progress achieved by the method of textual criticism during the eighteenth century, we witness a return to old positions in the first fifteen years of the nineteenth century. Gottfried Hermann and Immanuel Bekker, the two greatest textual critics of the generation after Wolf, differed greatly from one another in many ways, but both remained quite unaffected by the need for a systematic *recensio* as it had been adumbrated by the great New Testament critics and by Ernesti and Wolf. Hermann was an admirable expert on Greek language and style and supplied contributions of decisive importance to the study of meter, but he had no interest in manuscript tradition: his editions are based not on manuscripts but on preceding editions, and the improvements he contributed to the text of the Greek poets are the fruits of conjecture or, when he does choose between variants, are based solely on internal criteria. To be sure, he succeeded very often in resolving once and for all textual difficulties that had remained unsolved until then; for after all, a thorough knowledge of an author's language and style always remains the first and essential condition for restoring his text. Yet his complete indifference with regard to the documentary foundation of the classical texts represents not only one aspect of his lack of understanding for the new Classical philology of Wolf and Boeckh but also a step backward compared to the textual criticism of the eighteenth century.

Bekker, on the other hand, was an indefatigable explorer of manuscripts:

1. Cf. John 1839: 20; Soupe 1841: 5 = Soupe 1856: 81; writes, "Whoever wishes to perform the art of criticism properly must first of all examine the manuscripts and seek out and investigate their characteristics as carefully as possible. I recall that you [i.e., G. Hermann] gave this advice very often." It remains uncertain whether Soupe *captanarum benevolentiae causa* is attributing here to his teacher something that the latter had in reality never said, or whether Hermann really did give this advice to his students without going on to apply it himself; we have already noticed a similar contrast between theory and practice in Ernesti and in Wolf himself; cf. chap. 2, nn. 39, 47. Cf. the addendum to this chapter.