Thinking of the stemma one should try to avoid thinking of a tree and its branches. The few disparate textual witnesses that we do have are often nothing more than a small pile of twigs and branches, of which we will probably never know where in the tree(s) of transmission they had their place. Lachmann’s method, the stemmatological approach in textual criticism is the other extreme, as it promises its adepts a beautiful tree, even if many of the branches remain invisible forever. It can be summarised as follows: “The steps in preparing a new edition are: identifying and studying comparatively the surviving manuscripts of the text (exemplars); identifying the characteristic errors that appear to distinguish the major branches of the stemma; reconstructing the stemma in detail by seeking the tree that accounts most parsimoniously for the occurrence of characteristic errors in terms of the relative recency of common descent among exemplars; selecting for further analysis only those readings evidently closest to the author’s original, and eliminating from further consideration those variants that contain no additional information; collating the selected manuscripts word by word; and finally, choosing among the alternative wordings in the effort to reconstruct the closest possible approximation to the original text, footnoting the rejected alternatives in the new edition’s apparatus criticus …”.1

A quarter of a century ago I wrote an article with the title “Establishing the stemma: fact or fiction?”.2 The question in the title was a rhetorical one, of course. I had become convinced of the fictional qualities of most traditional stemmatology. The reason for writing that article was primarily rage. I was angry because I felt that, while I was editing an Arabic text, I had been left in the lurch by the existing scholarly literature on editorial technique. After having lost much valuable time I came out with a few ideas of my own with special reference to medieval Arabic literature, and wrote these down. The article became, quite to my amazement, rather popular and it was placed on a number of academic reading lists. It has even been translated into Turkish.3 The publisher in Istanbul had made a collective volume, with texts by Paul Maas (about him more, hereafter) and the Canadian scholar Barbara Bordalejo (University of Saskatchewan). How the Turkish publisher came to this combination of authors I do not know, nor did I question it, although I can understand the publisher’s idea to take an old voice, a modern voice and a counter-voice. Yet, between the two professionals, Maas and Bordalejo, I felt as the odd man out. I am not present in a professional stemmatological gatherings, such as Studia Stemmatologica, which is a series of international workshops on stemmatology,4 nor do I show up in settings of the Textual Communities Project, or other places where the stemma is revered, and where digital humanities pop up like mushrooms.5 I am not proud of my ignorance and my anti-social behaviour, but sometimes I just want to edit a text, and therefore I need method. So my orientation towards stemmas is mainly a practical and pragmatical one, while I do take into account the peculiarities of textual transmission in an Islamic context.

Here is what happened a long time ago. I worked, intermittently between 1972 and 1989, on what I wished to be the critical edition of a short work on the division of the sciences by the Egyptian polymath Ibn al-Akfânî (d. 749/1348).6 He was an interesting intellectual in Mamluk Cairo, and is now completely forgotten. His booklet on the sciences is a relatively short text of under twenty thousand words. With a “critical edition” I meant to bring out a version of the text that the author might recognise as his own (if I may paraphrase the concept of archetype in this way). While working on this edition I was, quite to my surprise, confronted with an extensive transmission of that text. As a result of my bibliographical homework the existence of some sixty-six manuscripts in countries on four continents had come to my knowledge. That was about five times as much as the number mentioned by Carl Brockelmann.7 Among these manuscripts

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1 Cîrène – Žiomek – Schwager 2010:3.
3 Witkam 2011.
4 Apparently based in Helsinki.
5 As long as these initiatives result in something tangible and verifiable, such as Digital Philology: A Journal of Medieval Cultures, which started to appear in Spring 2012 from Johns Hopkins University Press, I have no objection.
6 The edition was published as my PhD thesis, Witkam 1989. The sixty-six manuscripts are listed on pp. 109–16.
7 Brockelmann 1937–49, II:137; Supplementband II:169–70.
was a branch of summaries, which in course of time had been, quite falsely, attributed to Avicenna. There was also some evidence of a Judaeo-Arabic version, which may somehow have a connection with the outspoken anti-Jewish opinions of Ibn al-Akfānī. With these sixty-six manuscripts, one would say, there was enough material to come out with a critical edition, which eventually I did, but not as I had thought I would do. However, the large number of surviving manuscripts implied an unexpected and unpleasant paradox, which I will describe in a moment.

Sixty-six manuscripts is a lot and that large number posed several problems, not only in the initial stages of the recension of the available material. The first practical question I asked myself was: “Will I be able to get hold of copies or microfilms of all these manuscripts, if I cannot inspect them by autopsy?” Practice proved to be even more recalcitrant than I had anticipated. Of sixteen of these sixty-six manuscripts I could not get hold of any other sort of reproduction. For some of these sixteen hard-to-get manuscripts I even had to acquiesce in the fact that they apparently were lost between the moment that they were described to me something about survival rates in recent times. That is, if we can extrapolate the survival number in a linear way – which maybe we cannot, if only because the rough total of sixty-six hides information that is far more complex than can be caught within that single number. A matrix between dates and places of copying would already give a more nuanced image. In course of time, the loss rates of the older, medieval manuscripts increase. The circumstance that I had to consider was that more than ten out of a total of sixty-six bibliographically attested manuscripts were lost or unavailable, told me something about survival rates in recent times.

Another approach is to try to calculate an average life span for Islamic manuscripts. For reasons that I will not now elaborate on (and

9 Buringh 2010:249.
10 The al-Dhawq al-Salīm of the modern Arab philologists.
11 With my sincere apologies to Mr. Buringh for such a blatant simplification of his carefully worded arguments and ingenious calculations.
12 In the long run, the linear argument does not hold, of course.
most of it is guesswork, anyway) I estimate the average life span for manuscripts in the Middle East from the pre-modern period13 at some three hundred years. A period of three centuries can be observed, for instance, in the older collections of the Leiden University Library. Many of the Arabic manuscripts purchased by Orientalist scholars in the Middle East in the early seventeenth century date from the fourteenth century. As soon as they were incorporated in a library, their survival chance greatly increased, of course.

A few other issues are to be considered. Bibliographical progress for Islamic manuscripts is slow and its quality is doubtful, especially in the countries of origin. Many collections have never been properly described. Several scholars and institutions have tried to give a follow-up to Brockelmann’s history of Arabic literature, but nobody has succeeded. I have the impression that outside Western Europe and North America the book is hardly used, if only because very few people are willing to read German.14 I am not sure that nowadays there are many other branches of scholarship in which the most recent reference work dates back to 1947. Yet, bibliographical backwardness implies an increase in the potential number of surviving manuscripts. That fact asks for a better definition of what is meant with survival. Objective survival I see as the estimated total number of manuscripts assumed to be in existence now, while the term subjective survival refers to the manuscripts actually at hand or available for use in one form or another.

Another significant difference between manuscripts in the East and in the West is the fact that printing as a means of dissemination of knowledge was introduced fairly late, often only in the first half of the nineteenth century, sometimes even much later. This not only means that manuscripts have continued to be made till a much more recent date (with a survival rate increasing by the century), but also that in the initial phase of the spread of printing in the world of Islam numerous editions of varying textual quality were produced, none critical. More often than not such editions are just printed manuscripts, and contaminated ones at that. In the case of Ibn al-Akfnī’s Division of the Sciences, three uncritical editions existed already before I published my edition in 1989.15 Two of these early editions had in the meantime become as rare as manuscripts. Nor was my critical edition of 1989 the final one. At least two more uncritical or rather “not so critical” editions were published afterwards.16

But there is more to be said. My philological ambitions in this project went further than producing a critical edition tout court. I also wanted to reconstruct the life and work of the author and to place him within an intellectual history. My bibliographical research resulted in a list of forty-nine titles of works that, with different degrees of certainty, could be associated with Ibn al-Akfnī.17 In the early stages of my search for manuscripts of Ibn al-Akfnī’s works I had, more out of ignorance than anything else, made a fatal mistake. I had assumed that Cairo, where he had died in 1348 of the plague, would be the place where the most important manuscripts of his work were preserved. After a prolonged stay in Cairo in the winter of 1973–74 this assumption proved to be untrue. The manuscripts that I was looking for simply were not there, and that could not only be explained by the xenophobia of Egyptian librarians.18 My mistake had been that I had not sufficiently realised that not only people travel but that they take their manuscripts with them. Many of Cairo’s great book collections of the Mamluk period had, after the Ottoman conquest of 1517, been unable to resist the centrifugal forces of the new Islamic metropolis, Constantinople. It is quite possible that the presence of these Egyptian, and Syrian, libraries in Constantinople has contributed to the intellectual and scholarly golden age of the Ottoman capital in the second half of the sixteenth century. In my research on Ibn al-Akkānī this proved to be the case as well. Only after I had been able to consult the extensive manuscript holdings in the Süleymaniye library in Istanbul, it

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13 I prefer the term “pre-modern” over “medieval”. The periodicity of Western and Oriental history has too little in common to allow for an overall use of the word “medieval”.

14 Nor does one see very often a reference to the Arabic translation (Tārīḫ al-adab al-ʿarabī) made under the supervision of Maḥmūd Fāhrī Ḥiḡāzī and published between 1993–95 in Cairo by al-Hay’ā al-Miṣrīyya al-ʿĀmma lil-Kitāb in nine volumes, for that matter.

15 See the description of these editions in Witkam 1989:118–27.

16 In 1990 there appeared the edition by ʿAbd al-Munʿim ibn Muhammad ʿUmār and ʿAbd al-Raḥmān (Cairo, Dār al-Fikr al-ʿArabī), and in 1998 appeared the edition by Ḥaḥmūd Fākhūrī, Muḥammad Kamāl and Husayn al-Ṣiddiq (Beirut, Maktatbat Lubnān Nāṣirīn).

17 Witkam 1989:50–52. I divided the works into four categories: preserved works with certain attribution (19), works with certain attribution of which no manuscripts are presently known to exist (11), loosely associated works, but probably not written by Ibn al-Akfnī (16), loosely associated works and certainly not written by Ibn al-Akfnī (3). The first two categories show yet another type of loss rate.

18 Examples of that are quoted in Witkam 1987:111–25. Not much has changed since the twenty-five years that have passed since then.
was evident that my study of the life and work of Ibn al-Akfānī could somehow be completed.

With my fifty available manuscripts of Ibn al-Akfānī’s Division of the Sciences I started to make collations in order to find out the relationship between the manuscripts. I would try to eliminate as many direct copies of manuscripts as possible from my list of textual witnesses, as from the beginning it was clear to me that even fifty manuscripts was just too much to work with. This was a practical consideration about work that, at the time (in the 1970s) had to be done entirely by hand. If one wishes to write the history of the transmission and reception of a work, it is important to have an abundance of witnesses, yes, but when the purpose is to produce a critical edition, elimination of witnesses is an absolute must. In my simplicity I had assumed that out of the fifty available manuscripts a considerable number could be eliminated, leaving me with a limited number of manuscripts that together could get me nearer to the archetype. This proved to be untrue as well, due to the paradox that I have just formulated, but of which I was not aware at the time. Only some ten percent of the fifty manuscripts proved to be direct or indirect copies of other manuscripts known to me. That too could be useful information for the calculation of loss rates of manuscripts. What I had on my table were in fact a few stages only of a calculation of loss rates of manuscripts. What I to me. That too could be useful information for the direct or indirect copies of other manuscripts known about work that, at the time (in the 1970s) had to do with practical reality. On the contrary, philological practice proves to be that recensions are not closed, but open. That idea was successfully followed up by the book by Martin West,21 which was meant to replace Maas’ Textkritik. West describes the “open recension” as “when […] all those manuscripts in which worthwhile variants […] appear for the first time, are not related perspicuously and do not allow us to construct an archetype”.22 Once the archetype cannot be reconstructed, which is the first phase, the second phase, the emendation, changes in a considerable way.23 Maas had had his critics as well, however, but these are much less known than Maas’ book on textual criticism, and at the time I was not sufficiently aware of the stemmatological discussions. How controversial Maas’ work in fact was, immediately upon publication, becomes clear from the review of Textkritik by Giorgio Pasquali, a text much longer than the reviewed book.24 An example of Pasquali’s criticism make this clear. On the recensio Maas writes: “When a witness, J, shows all faults of another preserved witness, F, plus at least one more, then J must descend from F”. To this Pasquali comments that in such a case J does not necessarily descend from F, because the extra fault can be accidental.25 It is a typical passage, as such accidents do not exist in Maas’ universe, where everything is complete and predetermined.

My plan B, the next-best solution, was to restrict myself for my edition of Ibn al-Akfānī’s Division of the Sciences to old manuscripts or manuscripts that somehow could be directly linked to the author. I consciously neglected thereby the possibility that there would be a young manuscript directly copied from a witness with the best readings. This resulted into my selection of seven manuscripts (out of the fifty-five available ones). These seven manuscripts were kept in Bologna, Cambridge, the Escorial, Jerusalem (in a private library that was hidden after the Israeli occupation of old-Jerusalem, but a microfilm of it was available in the library of the Arab League in Cairo), Leningrad, Paris and Princeton. All seven manuscripts could somehow be linked to the era or entourage of the author. Of the seven

21 West 1973. A similar approach is proposed by Robson 1988, who goes, however, much further than textual criticism in the narrow sense of the word.
23 See the explication of the two-phase concept in the Lachmannian method by Berschin 2007:251–57.
24 Originally published as Pasquali 1929, also available in Pasquali 1986. With thanks to Alessandro Bausi for this reference.
manuscripts, the one from Princeton was selected on paleographical grounds only, and I have never lost my doubts about its textual value, or rather lack thereof. The critical apparatus of my edition\textsuperscript{28} shows some recurrent clusters of sigla, but not sufficiently for a safe elimination of one or more of the selected manuscripts. A typical case of an open recension.

During the collation I had observed that all seven manuscripts contained smaller or larger lacunae. All were slightly incomplete, mostly so the Jerusalem manuscript where the copyist had skipped the entire epilogue which contained an explanation of basic philosophical terminology. He wrote: “...I suppressed the definitions here, and who wants to know these can find them in books on logic, philosophy and the natural sciences, as these are loaded (mashhūna) with this”\textsuperscript{27}. So here I had an intelligent copyist, who did not wish to waste his time on elementary matters. The final result of my work was an eclectic edition of Ibn al-Akfānī’s Division of the Sciences. In it, justice was done to the wealth of manuscript witnesses, both in the body of the text and in the critical apparatus, but it was in no way the archetype or anything near that. Once I had decided, more by circumstance than by choice, to produce an eclectic edition I felt free to convert the entire text into the orthography of Modern Standard Arabic. Editors in the Arab world do so anyway. I felt justified to impose a modern orthography onto an old text because the orthography of all seven selected manuscripts was incomplete, inconsistent, different and mutually exclusive. The choice for the orthography of Modern Standard Arabic was the only way out of that problem.

Then I wrote the article “Establishing the stemming, fact or fiction?”. I was really angry, at my teachers, and, indirectly also at Paul Maas,\textsuperscript{28} for not having warned me for the enormous divide between theory and practice, between ideal and reality. I certainly had lost time, but in it, I had acquired experience by trial and error.

**Quoted bibliography**

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\begin{footnotes}
\item[28] At the time, he was nothing but an iconic name for me. After I had read more about his life, notably how he had been persecuted by the Nazis (see Mensching 1987), and after the quarter century that has passed since my initial stemmatological work, I have become both milder and more sceptical.
\end{footnotes}


Jan Just Witkam
Leiden University Centre
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