A Response to Van Egmond on Høyrup, *Jacopo da Firenze's* Tractatus Algorismi

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It is with little pleasure that I sit down to formulate my objections to the review, written by an appreciated colleague, of my edition and study of Jacopo da Firenze's *Tractatus algorismi*.[1] However, the misrepresentations and distorted arguments in the review are so dense and so serious that I feel obliged (to myself, to the publisher and editorial board, and to the scholarly field in question) to respond.

I have no complaints about the fact that the reviewer would have liked me to write a different book directed at the general and not a specialist public. If he thinks that a competitor to Frank Swetz' *Capitalism and Arithmetic* [1987] is needed (and it may well be), he should be in the optimal position to write it himself.

To start with the positive: I am grateful to the reviewer that he has discovered my mistaken transcription and translation of the rule of three; my mind must somehow have been infected by the ensuing identification of this third thing as 'the other that remains'.[2]

[1] Since the reviewer speaks of me almost solely as 'the editor', I shall refrain from mentioning his name except in quotations.

[2] In full, the rule runs as follows:

> *<S>e ci fosse data alcuna ragione nela quale se proponesse tre cose, sì debiamo multiplicare sempre la cosa che noi vogliamo sapere contra a quella che non è simegliante, et parti nela terza cosa, cioè, nell'altra che remane.*
>
> If some computation should be given to us in which three things were proposed, then we should always multiply the thing that we want to know against that which is not similar, and divide in the third thing, that is, in the other that remains.

Contrary to what the reviewer states, the rule gets its name *of three things* from the initial '*three things* [that] were proposed', not for the appearance of the third thing within the rule.

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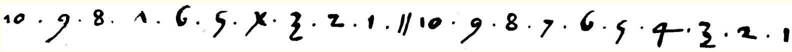
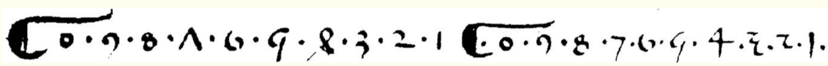
I am also glad that he noticed that my reproduction of the shapes of numerals on Høyrup 2007, 196 is wrong—not because I could not draw them correctly but because I mixed up two computer files with almost identical names. The shapes that are rendered on my page 196 are those of the Trivulziana manuscript (**M**, for Milan), and accordingly reappear on page 385. However, this manuscript (and thus what I render) does not omit the 1 written before the zero in the indication of the old shapes; it writes it to the right. But in the indication of the new shapes, it does omit it. The shapes in the Vatican manuscript (**V**) are:



The 'old' and 'new' shapes of the Arabic numerals according to **V**

The Riccardiana manuscript (**F**, for Florence), the one which the reviewer considers by far the oldest, omits the 1 in both places:



The 'old' and 'new' shapes of the Arabic numerals according to **F**

What the original author did is thus not clear at all.

According to the reviewer, the omission of the 1

> when combined with the reformatting of the tables [of continued division], might give the impression that the author wrote the zero separately and not always as part of the number 10. [39]

The 'reformatting' of which he speaks refers to a greater spatial separation of columns that have nothing to do with each other, and thus can give no impression of the kind. And indeed, the tables with continued divisions contain many remainder zeros, transferred to a separate column in the next row (in all three manuscripts). So, here the reviewer is mistaken on both accounts.

What he says [39] about 'a systematic rendering of the [Arabic] numeral "1" as the lower case letter "j"' is equally mistaken, and shows that he has not read the pages just before the edition itself explaining that this shape (simply a long "i" and not a separate letter “j”, which was only invented centuries later) is rendered “j” ‘when it represents the Roman numeral 1 and stands as the last in a sequence (thus j, vij, xiij, etc.)’ [Høyrup 2007, 190]. This follows the convention of the epoch and of the manuscript; and I cannot imagine that the reviewer does not know it. What I render “j” is everywhere a Roman numeral in the manuscript; it is long, and it is prolonged below the line; comparison with the correlated writings of numbers in Roman and Arabic style on fol. 2v leaves no doubt. At times, it is provided with a ‘phonetic complement’, a small “o” written above the numeral (for typographic convenience I omitted this from the edition, which was perhaps a mistake).

Another complaint also comes from the reviewer’s failure to read what I say about my editorial principles (and from misreading the edition). He writes that

> while comparing the text with the original, I found that the editor has omitted all of the corrections that the copyist himself made, perhaps because there were so many. [42]

This is simply nonsense. On the same page as before, he would have found that

> passages in < > repair copyist’s omissions, in the translation also copyist’s errors; the occasional superscript letters ($< >^{\mathrm{M+F}}$, $< >^{\mathrm{M}}$, $< >^{\mathrm{A}}$) refer to a manuscript or manuscript group on which the restitution is based. Letters, words and passages in { } are present in the manuscript by error; those that are deleted by the copyist are struck out in the text edition and omitted from the translation; words or passages that were at first omitted by the copyist and afterwards inserted above the line are marked ˆ ˆ, whereas insertion in the margin is marked * *. Editorial comments are in [ ], added words in the translation in ( ). Passages in italics in the edition correspond to the use of red ink in the manuscript. [Høyrup 2007, 190]

I cannot guarantee that I have not overlooked one or two corrections—editors make errors—but the reviewer speaks of omission of ‘all of the corrections’, which shows that he can have read very little of the text since he has not stumbled on any passage marked in this way.

Yet another complaint based on similar failure is that

> there is no common numbering for the paragraphs or sections of the text, so one cannot readily compare the texts in the two sections [containing, respectively, the editions of the Vatican and of the Trivulziana + Riccardiana manuscripts]. [43]

The reviewer has obviously not read page 380 [Høyrup 2007] (just before the edition of **M** and **F**, where indication of such things should be expected). There I write that

> for the numbering of paragraphs in **M**, I use those of my transcription of **V**; this should facilitate a comparison of these two manuscripts. Paragraphs that have no counterpart in **V** are assigned the number of the previous paragraph with an added letter A (and B if necessary); paragraphs that are displaced in **M** with respect to **V** are treated similarly, but the corresponding number in **V** is added in parenthesis.[3] For **F**, I indicate Simi's numbering.

Besides not reading the explanation of editorial principles, the reviewer has not even tried to compare the editions, since in this case he would have discovered that the numbering is the same for the Vatican (**V**) and the Trivulziana manuscript (**M**) to the extent that the differences make it possible. That I also indicate Annalisa Simi's numbering in her edition of the Riccardiana manuscript just below the corresponding number for **M** should not produce confusion but only facilitate comparison with her edition.

A final result of the reviewer's not reading the explanation of editorial principles is that he finds it 'extremely difficult to read' the edition of **M+F** [42]. For reasons explained in my book, it was reasonable to choose **M** as exemplar and to correct it where the reading of **F** was clearly better; this should be quite standard. Since only two manuscripts are involved, I then chose to indicate by super- and subscripts where one of the manuscripts deviates from the text that I had established in this way. This was intended to make it easier for the user to locate the deviations than if the apparatus had been put into footnotes. If the reviewer had read an italicized sentence on Høyrup 2007, 379–380:

[3] I omit the footnote in the original that gives examples.

> *Neglecting all superscript and subscript, one thus essentially gets a text which is close to the common archetype for the two manuscripts.*

he ought to have had no difficulty.

The reviewer is further dissatisfied that I did not make a critical edition of all three manuscripts. Actually, Annalisa Simi and the late Jean Cassinet had already prepared a critical edition of the Riccardiana and the Trivulziana manuscripts. As Jean Cassinet told me in 1999, they found the Vatican manuscript so different from the others that it was meaningless to make an edition of all three manuscripts—a claim that I still endorse. The expected appearance *in print* of this edition made my choice to prepare an edition of the Vatican manuscript obvious. But, as it turned out, the edition of **M** and **F** never did appear: the publisher lost the manuscript (after having brought the project so far that subscriptions were paid!), and those who took care of Cassinet's *Nachlass* did not find a copy [see 121n6 below]. At a late moment, I therefore decided to include what I call a 'semi-critical' edition of **M** and **F**—called thus because for **F** I relied on Simi's edition and not on the manuscript. This (except Cassinet's reason not to include **V**) is explained on pages 5 and 379 of my book. The reviewer's speculations and accusations in this respect are yet again built on a failing ability or will to read the work that he was supposed to review.

In other places, the reviewer has at least read enough to misrepresent what is written in the book. For instance, he writes that I

> came to this conviction [*viz.* that **V** represents the most authentic text] in 1997, when [I] first examined the algebra section in the Vatican manuscript and noticed how different it was from the traditional presentations of algebra that derived from the tradition of Mohammed bin Musa al-Khwarizmi. [41]

If that were the case, I would be a fool. If the reviewer's oft-repeated belief in the derivation of the *abbacus* tradition from the *Liber abbaci* were true, the differences should rather suggest a long development and thus a late date. Now, I still shared this belief with him in 1997, and only gave it up reluctantly years later.[4] What I wrote is indeed

[4] The 'detailed summary of the obscure 13th-century *Livero de l'abbecho* and [...] comparison with the *Liber abbaci*' [45]—actually, not only a summary

something different, namely, that I realized that the algebra of this manuscript 'might have astounding implications for our understanding of the origins of European vernacular algebra' [Høyrup 2007, vi]. This has nothing to do with the Jacobean authenticity or the exact date of the text, and precise investigation of any orderly *abbacus* presentation of algebra might have served the same purpose. (I disregard the chapter copied from Fibonacci's *Liber abbaci* in Benedetto da Firenze's encyclopedic *Trattato* and a few similar encyclopedias, but not Benedetto's own presentation). The Vatican manuscript just happened to contain the first *abbacus* algebra that I worked on in depth.

Admittedly, all of this is peripheral, even though the last point is connected to the reviewer's main complaint: my 'obsession with proving the authenticity of the Vatican text' [45]. This accusation, however, can easily be turned around.

The first scholar to describe **V** was Louis Karpinski [1929]. Since he had not seen the other manuscripts, he took it to represent Jacopo's original treatise. The next scholar to look at it was apparently the reviewer himself who, as I wrote [2007, 5],

> inspected it in the mid-seventies during the preparation of his global survey of Italian Renaissance manuscripts concerned with practical mathematics [1976; 1980]. [...] Van Egmond noticed that the manuscript which Karpinski had examined (Vatican MS Vat. Lat. 4826, henceforth **V**) could be dated by watermarks to the mid-fifteenth century, and that the algebra chapter (and certain other matters) were missing from two other manuscripts which also claim to contain Jacopo's *Tractatus algorismi* (Florence, Riccardiana MS 2236, undated;[5] henceforth **F**; and Milan, Trivulziana MS 90, c. 1410; henceforth **M**).[6] Because **M** can be dated by watermarks to c. 1410,

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but an analysis is presented—serves to show that the only argument that has ever been advanced for this generally accepted dependency of the *abbacus* tradition on Fibonacci is a fallacy.

[5] Høyrup 2007, 5n5:

> Van Egmond's dating [1980: 148] is misleading, since it is merely the date of Jacopo's original (which is given in all three manuscripts), not that of the manuscript.

[6] Høyrup 2007, 5n6:

> some 40 years before **V** (yet still a whole century after 1307), and since **V** contains rules for the fourth degree not present in the algebra of Paolo Gherardi's *Libro di ragioni* from 1328, Van Egmond decided (personal communication) 'that the algebra section of Vat. Lat. 4826 [was] a late 14th-century algebra text that [had] been inserted into a copy of Jacopo's early 14th-century algorism by a mid-15th-century copyist'.

The reviewer was apparently not aware that 'reducible fourth-degree equations were solved routinely in Arabic algebra at least since al-Karajī's time'. In his review, he calls this 'a very expansive claim' for which 'no source is ever given' [43]. It is indeed well known by everybody working on the history of Arabic algebra, and *should* also be known by anybody speaking about the 'achievements' of the *abbacus* masters and interested in distinguishing their innovations from their borrowings. Since the reviewer does not seem to know, I urge him to start with Roshdi Rashed's biography of al-Karajī [Rashed 1973, 243 col. B (the last six lines)].

Until recently, one of the reviewer's main arguments was based on his contention that **F** was from 1307.[7] Perhaps because of my objections [see 120n4], he has now understood that this claim cannot be upheld. Instead (and perhaps because of an 'obsession with

A transcription of **F** was made by Annalisa Simi in [1995]. A critical edition of **F** and **M** by the late Jean Cassinet and Annalisa Simi was almost finished in 1999, but it got stuck with the publisher and is not going to appear (Maryvonne Spiesser, personal communication), for which reason I give a transcription of **M** with indication of all not merely orthographic variants with respect to **F** in the Appendix.

As pointed out by Karpinski and Robbins [1929: 170], **F** had already been mentioned by Boncompagni in 1883 and by B. Lami, librarian of the Biblioteca Riccardiana, in 1754; however, they had not seen **F** and, therefore, could not know that it differs from **V** on important points.

[7] In an earlier paper, the reviewer refers indeed to 'an early 14th-century *Tractato de algorismo* [*sic*] found in Ricc. 2236 and therein specifically dated to the year 1307' [2008, 313]. In the same article and on the same page, he also transforms **M** into 'several later copies' of that manuscript which 'do not contain any algebra', without noticing that mistakes in **F** that are not found in **M** exclude this affiliation. **M** can thus not be a copy of **F**; the two must come from a common archetype.

proving the authenticity of' **F** on his part), he now explains that this manuscript

> is written on vellum and so cannot be precisely dated; but the fact that it uses vellum (which was largely abandoned for writing common texts by the middle of the 14th century), combined with its ink, handwriting, language, and style, make it clear that it was written in the early 14th century, and thus must be accepted as the oldest text. [40]

Yet his own catalogue of *abbacus* manuscripts shows that two thirds of the conserved *abbacus* manuscripts written on vellum are from the 15th or the early 16th century, and that the *corsiva gotica cancelleresca* (the script of **F**) was used in *abbacus* books at least until the very end of the 14th century.[8] I cannot judge the ink, and I fear that the reviewer is in no better situation. Moreover, I can see nothing in the language which could not just as easily belong to the early 15th as to the early 14th century. This really looks to me like 'weak stylistic impressions' [42], whereas my references to the stylistic homogeneity of **V** and its partial agreement with features of **M** and **F** build precisely on 'the hard evidence of textual comparison' which the reviewer then characterizes as 'the editor's complex linguistic arguments [and] detailed discussion of alternate spellings, words, phrases, and word ratios' which 'will bore anyone but the most dedicated student of Italian linguistics' [45].[9]

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[8] See Van Egmond 1980. 15th- and 16th-century manuscripts on vellum are mentioned on pages 73, 96, 143, 158, 165, 168, 173, 175, 178, 232, 247, 257, 261 (twice), 262 and 275; *corsiva gotica cancelleresca* used after 1350 is mentioned on pages 48, 137, 138, 211 and 250. Quite apart from what I may have overlooked, both lists are likely to be incomplete because dates stated in the manuscript or derived from internal evidence belong with the original and not with the actual copy.

[9] As I explain [Høyrup 2007, 55], similarities with a *Trattato di tutta l'arte dell'abacho* apparently written in Avignon in 1334 (as argued convincingly by Jean Cassinet) indicate that the compiler of the shared archetype for **M** and **F**, if not working in Provence, used material which was produced there—and indeed during the first half of the fourteenth century. However, the obvious deviations from this common archetype are at least as many in **F** as in **M**. Even if **F** should be written before (say) 1340, it is therefore not to be considered better than **M**.

The other main argument concerns the algebra contained in the Vatican manuscript. The reviewer sticks to his original opinion that it is a mid 15th-century insertion into a late copy of Jacopo's treatise, though he now adds arguments developed in Van Egmond 2008. He states that

> two late 14th century algebra texts [from the 1390s] now in the Biblioteca Nazionale Centrale di Firenze, Fond. Prin. II. V. 152, folios 153r–166r, and Conv. Sopp. G. 7. 1137, folios 110r–111v, give exactly the same equations as the Vatican text in exactly the same order [Van Egmond 2008, 313]. [44]

for which reason they must be regarded as sources for the Vatican algebra. At this moment of writing, I do not have access to the latter manuscript. But in the paper to which the reviewer refers, he himself states that it deals with 22 equations (read 'equation types'), not with 20 as does the Vatican manuscript algebra. Moreover, concerning the former manuscript, the same article states (correctly) that it contains 25 equations (i.e., equation types). So, already on this elementary level, the reviewer is unable to remember what he published two years ago. Worse is that 'the hard evidence of textual comparison' would have destroyed his claim completely. What he compares are just abstract equation types, rather than the level of the treatises or their words or their examples (the actual equations). Florence, Fond. Prin. II. V. 152 is a very advanced treatise. Its last three equations are of types $ax^3 + bx^2 = n$, $ax^3 = bx^2 + n$, and $bx^2 = ax^3 + n$; and it is shown how to reduce these to equations without a second-degree term—exactly the trick Cardano used 150 years later. The treatise also contains a discussion of the sequence of algebraic powers and schemes for the multiplication of polynomials, all of which is absent from the Vatical algebra (and certainly far beyond its author's horizon).

If we look at the examples given in the Vatican algebra and in Florence, Fond. Prin. II. V. 152, they are also very different.[10] On the other hand, the Vatican examples are shared with various algebras from the earlier 14th century (Gherardi and others), as shown in

[10] One, a very popular type, is shared; but the same type is also shared with Gherardi. The numerical parameters of the three are different.

a scheme given on Høyrup 2007, 160. Moreover, a *Trattato dell'alcibra amuchabile* (included in Florence, Riccardiana 2263) written *ca* 1365 (as dated by watermarks by the reviewer in 1980) contains everything from the Vatican algebra in so identical a form that it would not only be sensible but also very easy to make a critical edition of the two. In one place, however, the Vatican algebra leaves spaces (stating that its original did so) where it should have transformed $4\sqrt{54}$ into a pure square root. Here, the *Alcibra amuchabile* has $\sqrt{864}$, showing that it represents a more developed form of the treatise. The *Alcibra amuchabile* contains a few more equation types and for these it agrees with Gherardi with one exception; and where the Vatican algebra contains no examples, the younger treatise also has the same examples as Gherardi. However, the agreement with Gherardi's formulations is not nearly as close as with those of the Vatican algebra.[11] All of this is described in my book.

In conclusion, the Vatican algebra can be safely ascribed to the first half of the 14th century. The reviewer's neglect of all evidence showing this vitiates his objections.

A third argument against the genuineness of the Vatican manuscript is the reviewer's rejection of my characterization of this manuscript as 'a meticulous (yet not blameless) library or bookseller's copy made from another meticulous copy'. He protests that it contains a number of erasures and insertions of forgotten words between the lines and in the margin. But he overlooks that in the era when no corrections in proof could be made, this is in fact evidence of meticulous copying. On occasion, the copyist even corrects one spelling (which he has used elsewhere) into another one which is also used elsewhere, showing that he is trying to follow the orthography of his original.

I shall stop here, even though other distortions could be listed. Readers who are interested in what is really to be found in my book and cannot afford the exorbitant price or get it from a library may (at least for the time being) find the first 48 pages on the Google

[11] In Van Egmond 2008, 305, the reviewer claimed that this algebra repeats Gherardi's 15 equation types 'in exactly the same order'. This is simply not true: Gherardi's no. 8 is no. 11 in the *Alcibra amuchabile*, and his no. 14 is missing from the other treatise. This is not the only incorrect statement in that article.

books website. Preprint versions of my text editions and related papers can also be found at http://www.akira.ruc.dk/~jensh/Selected themes/Abbacus mathematics/.

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