Response to Jens Høyrup's "Answer to Jeffrey Oaks' review of Jens Høyrup, *Jacopo da Firenze's* Tractatus Algorismi *and Early Italian Abbacus Culture*. Basel etc.; Birkhäuser, 2007."

Jens Høyrup's ten page answer to my review of his book seems to diverge from my main points of criticism. He spends almost the first half of his answer challenging my credibility. Only in the middle of page 5 does he begin to address the main points of my review.

This response is divided into two sections. First I comment on the important points, namely Høyrup's response to my criticisms of his arguments for his core thesis. Then I address his description of "how Oaks works". Both my review and Høyrup's answer are posted on the CSHPM web page: www.cshpm.org.

Høyrup's defense of his main argument

Recall that the Vatican manuscript (**V**) of Jacopo's treatise contains some chapters which are not found in the Florence (**F**) and Milan (**M**) manuscripts. In particular, **V** contains a couple chapters on algebra which are absent from **F** and **M**. Høyrup claims that the extra chapters were originally written by Jacopo in 1307, and were somehow omitted from the line which led to **F** and **M**.

I gave three main objections to Høyrup's evidence: (1) he misuses statistics regarding the spellings of words, (2) at the end of the first chapter he promises further evidence that the algebra in **V** dates well before 1328, but never delivers it, and (3) the cross references which he claims link the extra Chapter 22 with the common Chapters 14-15 do not exist.

Misuse of statistics

Høyrup tries to deflect the reader from examining his own misuse of statistics by claiming that I do not know a nil hypothesis when I see one.[1] In fact, he seems not to know how to properly formulate an alternative hypothesis. Høyrup's nil hypothesis H_0 is "the 7 *fact* of **F** [are] distributed randomly over the relevant 35 *fact*+*fatt* of **V**". (p. 15 n. 33) His alternative hypothesis H_A is "one of the two manuscripts [is] derived from an original which the other one follows quite precisely on this orthographic account." (p. 14) His calculation does indeed suggest that H_0 should be rejected, but statistical dependence of the spellings does not imply that one of the two manuscripts is close to the original. As I wrote in my review, the common source for **V** and **F** could have had the same 7 *ct*'s and 8 *tt*'s we find in both manuscripts, with the remaining 20 instances divided between *ct* and *tt*. The scribe of **F** changed the *ct*'s in this group to *tt*'s, while the scribe of **V** changed the *tt*'s in this group to *ct*'s.

Høyrup objects in his answer "It is not impossible but definitely less likely that two scribes should both have so weak preferences yet so stable that they sometimes but not always changed *dict* into *ditt* (**F**) repectively *ditt* into *dict* (**V**),[2] never making the opposite change, than that a single copyist [i.e. of **F** —J.O.] should be characterized by such a weak but systematic tendency to slide in one direction...". When I copy text I sometimes type what I am directly looking at, and other times what I have sounded out in my head. So it is very likely that a scribe preferring *ct* will copy visually *tt*, and change a *tt* to *ct* when sounding out words. Of course this scribe would not change a *tt* into a *ct*. Likewise the scribe who prefers *tt* will end up changing *some* of the *ct*'s into *tt*'s, but none the other way around.

[1] "Obviously he does not know what a nil hypothesis is, or does not recognize it if it is not named explicitly as such..."

[2] Here Høyrup mistakenly writes *dict/ditt* in place of *fact/fatt*.

Høyrup writes that for the distribution of *partire per/partire in* my alternative explanation is impossible because "in **V** this distribution corresponds to a *system*, which is absent from **F**." My simple answer to this is that systems—especially simple ones like this—can be imposed by copyists, not just by authors.

The promised evidence

Høyrup writes on p. 25 of his book "As we shall see, at least the algebra [of **V**] must be dated well before Paolo Gherardi's work from 1328..." I wrote in my review that the promised evidence is never given, and that Høyrup merely assumes his concusion as true. Høyrup writes "Sorry, the evience is in a scheme on p. 160, which lists shared examples (with and without shared numerical parameters and normalization division *in* versus *per*)."

The list of equations and the various parameters Høyrup indicates in his table only serve to show connections between manuscripts. They do not tell us which manuscripts were written before others. After explaining the table's notation, Høyrup comments on Paolo Gherardi. "Gherardi, as we see, follows Jacopo fairly closely in the six fundamental cases." (p. 161) And under "The Lucca Manuscript" he commences with "The two algebraic components of this conglomerate...are...largely to be described as somewhat free abridgements of Jacopo's algebra." (p. 163). Nowhere in this chapter does Høyrup make the case that the algebra in **V** predates the other early algebras. He simply assumes it. In fact, in a recent article Albrecht Heefer compares the Lucca manuscript with **V** and concludes "It is not established that the Lucca text is derived from Jacopo's treatise."[3]

I suggested in my review (p. 6) that Chapters 16 (on quadratic equations) and 17 (higher degree equations) of **V** are combined from different texts. I did this because Chapter 16 is for practical purposes identical to the corresponding chapter in a text from ca. 1365 (MS **A**), while van Egmond links Chapter 17 with a family of texts dating from ca. 1390-ca. 1470. Høyrup notes that even the rules for higher degree equations common to **V** and **A** are nearly identical, including a lacuna in one rule (17.15 in **V**—I missed this before). So the wording of Chapter 17 matches the ca. 1365 MS, while the organization of the equations matches the later family. I will not try to investigate this dilemma here. Even if we suppose that Chapters 16 and 17 were written together, it only securely dates their composition to no later than ca. 1365, not all the way back to 1307.

Cross-references

Høyrup wrote in his book that the overlap between Chapter 22 (found only in **V**) and Chapters 14 and 15 (in all three MSS) "turned out to consist of duly cross-referenced variations and supplements" (p. 24). I noted in my review that with one exception all cross references in Chapter 22 are to other problems in the same chapter. The exception is a reference to the rule to multiply the diameter of a circle by $3\frac{1}{7}$ to get the circumfrence. This rule is so common in Abbacus texts that the reference could easily be to a part of another book from which the scribe of **V** took Chapter 22, or even to a problem from Chapter 22 which was left out of **V**.

In his answer Høyrup brought up one other cross reference, from 22.5: "I want to know how many square *braccia* [the area of a circle] is without espying the circulation around". Høyrup considers this to be a reference to the problem in Chapter 15 in which the area of a circle is found from its circumfrence. Høyrup assures us that the problem of finding the area from the circumfrence "is very rare in abbacus geometries except those that were written in Provence in the early fourteenth century". Not so. I checked four 15th c. Italian practical geometry texts from Siena and Florence,

[3] [Heeffer 2008, 225]. In this article Heeffer accepts Høyrup's claim that the algebra in **V** belongs to Jacopo's original book.

and two of them find the area of a circle this way.[4] A third text, by an anonymous Sienese geometer, has the problem "If you want to produce in square *braccia* a circle without knowing the circumfrence..."[5] The phrases "without knowing the circumfrence" and "without espying the circulation around" are not references to a previous problem on finding the area from the circumfrence only. They are references to the common rule that the area of a circle can be found by multiplying the circumfrence by the diameter, then dividing by four. This rule is stated in the beginning of the Sienese book, and is used or stated in all the other texts I consulted.

So the passage in 22.5 of **V**, "without espying the circulation around", might easily point to another part of the book from which Chapter 22 was taken. Høyrup cannot produce *one* cross reference in Chapter 22 which can be convinvingly linked to Chapters 14 and 15.

Høyrup's opening attacks

Census: *singular or plural?*

Høyrup writes "Oaks seems to be unaware that the Latin verb allows us to decide whether the subject is singular or plural...". The noun in this case is *census*, and the singular verb is *equatur* ("equals"), as in the generic simple equations "Census equatur radicibus, et census equatur numero"[6] from Gerard's translation of al-Khwārizmī. Høyrup neglects that Gerard's work is a translation, and that the Arabic text might follow a different convention.

The verb to equate the two parts of an algebraic equation in medieval Arabic is *ʿadala* ("to equal"). The present singular form is *yaʿdil*, and the plural is *taʿdil* (the names of the powers are all masculine, so gender does not come into play). Curiously, about half of the equations with a plural subject have the singular verb.[7] Examples from al-Khwārizmī are "twelve *māl*s equals (*yaʿdil*) a *māl* and forty-four dirhams" ($12x^2 = x^2 + 44$) and "a *māl* and twenty dirhams and a quarter equals (*yaʿdil*) eleven roots and a quarter"[8] ($x^2 + 20\frac{1}{4} = 11\frac{1}{4}x$). There are also places where a plural verb is combined with a singular subject, like "half a sixth of a *māl* equal (*taʿdil*) a thing"[9] ($\frac{1}{2}\frac{1}{6}x^2 = x$), but these are rarer.

Such anomalies are not restricted to the critical edition of al-Khwārizmī's book. I consulted photocopies of the manuscripts of the algebra texts of Abū Kāmil (late 9th c.) and ʿAlī al-Sulamī (10th c.), and they show the same variation in the verb. The printed editions (some critical) of a few other works concur: al-Karajī's *al-Fakhrī* (11th c.), al-Fārisī's *Foundation of Rules on Foundation of Benefits* (ca. 1300), Ibn al-Hāʾim's *Commentary on the Poem of al-Yasamīn* (1387), and Ibn Ghāzī's *Aim of the Students in Commentary on "Desire of Reckoners"* (late 15th c.). All combine a plural subject with the singular verb about half the time (or more), and sometimes a singular subject with the plural verb.[10] Because this mixing of tenses is ubiquitous in Arabic algebra, there

[4] [Tommaso della Gazzaia 1982, 32; Orbetano da Montepulciano 1991, 57]. Tommaso solves the problem the same way as **V** with the same circumfrence of 22. Orbetano solves the problem with circumfrence 30 by squaring the circumfrence, then dividing by $12\frac{4}{7}$ (4π).

[5] [Anonymous 1986, 9]. I modernized the vocabulary in the translation. The Italian reads "Se tu volessi recare a braccia quadre uno tondo sanza sapere quanto giri d'intorno..." Although the wording is different this problem is solved the same way, with the same parameters, as 22.5 in **V**. The fourth book I checked is [Anonymous 1993].

[6] [Hughes 1986, 233 line II.4].

[7] Of course I considered case endings.

[8] [al-Khwārizmī 2007, 179;1, 183;1]. I write the plural of *māl* with the English suffix "s". Compare the first equation with the very next equation: "eleven *māl*s equal (*taʿdil*) forty-four dirhams" ($11x^2 = 44$) [al-Khwārizmī 2007, 179;3].

[9] [al-Khwārizmī 2007, 183;5].

[10] I stopped looking after checking through these books. Al-Karajī is inconsistent even in stating the generic equations. He has "things and number equals (*yaʿdil*) *māl*s" in two places, and he writes "*māl*s and number equal(s) things" once with the singular verb, and once with the pural. [Saidan 1986, 149;2, 149;3, 155;1, 160;1].

is no reason to think that al-Khwārizmī used the "correct" verbs, and later some ignorant copyist muddled the text.

I can only guess why Arabic equations often show the "wrong" conjugation for both plural and singular subjects. Perhaps the algebraists sometimes thought of the plural "left side" of an equation as a single expression. And maybe the singular subject "thing" with the plural verb in an equation like ʿAlī al-Sulamī's "So the thing equal four dirhams"[11] was imagined as the plural collection of four dirhams which it equals.

It should be no surprise, then, that I disregard the conjugation of the verb *ʿadala* when reading Arabic algebraic equations. The verb *does not* tell us whether the subject is singular or plural. So when I looked at Gerard's translation of al-Khwārizmī I naturally disregarded the difference between *equatur* (singular) and *equantur* (plural) in his equations. I did not even think to bring it up in my review.

For this response I have examined Gerard's verbs closely. Contrary to what we find in Arabic, he was mostly consistent in his conjugations (the two exceptions I found wrongly show the singular conjugation[12]). A comparison between the Latin and Arabic shows that Gerard changed the singular *yaʾdil* to plural in six instances, and the plural *taʿdil* to singular in six instances (not counting the places where I contended that *census* is plural). Now I see that Gerard did indeed intend that *census* is singular in stating the simplified equations. This, however, does not imply that al-Khwārizmī himself wrote the singular. I gave other arguments in my review that al-Khwārizmī second power term is plural, and as I wrote in footnote 30 "I have even more to say on this, but I should stop here." If anyone wishes to know more, write to me.

The meaning of conpiuti

On this point Høyrup is right. I misread the text. The passage should be translated as "And we have made a problem that, when the voyages were completed, he found himself with 54..." And as Høyrup notes, this is a peripheral point.

Because Høyrup frames his comments on my mistranslation with remarks on our correspondence, I should explain some events surrounding the publication of my review. As I told Amy Ackerberg-Hastings before publication, I would let Høyrup know about the review once the final version was available. At the time I thought that the editors might suggest some changes before posting it. I found out November 6 that they published my original .pdf file online. I wanted to send the link to Høyrup with an accompanying letter, which would have to wait until the next day, as I had no time that afternoon. Unfortunately, in the meantime another historian sent Høyrup the link, and Høyrup wrote me the November 7 note which he quotes in his answer. I thus lost the opportunity to be the one to inform him about the review.

In his note Høyrup asked about my evidence regarding the meaning of *conpiuto*. I e-mailed a friend in Italy to ask him about it. Unfortunately I did not hear back from him. I should have written something to Høyrup after a week or so, but I was distracted by other things.

On page 3 of his answer Høyrup writes that I never got back to him on his examples of the singular *census* from a 2003 letter. His does not mention that his letter is a seven and a half page commentary on an article I had submitted to *Historia Mathematica* (and later published as two

[11] [MS Vatican Sbath 5, f. 41a;11]. "The thing" is clearly nominative in this equation. I found three other instances of "the/a thing equal..." in this MS before I stopped looking.

[12] [Highes 1986, 249 line 54, 261 line 138]. The Arabic has the plural in both places.

separate articles), and his remarks on *census* occupy only part of one page. I did address most of his remarks either directly or by revamping the article.

Conclusion

It is unfortunate that Høyrup took the indirect approach of attempting to discredit me before defending his main points. But without any real arguments, it may have been his only option.

I am aware of only one other review of Høyrup's book. It is by Menso Folkerts, and it will appear soon in *Annals of Science*. Folkerts, too, argues against Høyrup's main claim that the chapters on algebra in the Vatican MS were part of Jacopo's original treatise.

—Jeffrey A. Oaks

Sources

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