Answer to Jeffrey Oaks' review of

Jens Høyrup, *Jacopo da Firenze's* Tractatus algorismi *and Early Italian Abbacus Culture*. Basel etc.; Birkhäuser, 2007.

http://www.cshpm.org/Archive/Bulletins/HoyrupReviewFall2009.pdf

Some six weeks ago (7 November, 2009), a colleague drew my attention to Jeffrey Oaks' online review of my above-mentioned book. After leafing it through and discovering two elementary gross mistakes I wrote an email to Oaks (8 November 2009), asking him the following:

> 1. Where is your evidence that *co*(*m* / *n*)*piuto* should mean "computed"?[1] May I quote two passages from Dante (Jacopo's contemporary, or almost so):
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> A. *Che se io desidero di sapere li principii de le cose naturali, incontanente che io so questi, e compiuto e terminato questo desiderio* (*Convivio* XIII.2). Thus, the *desiderio* [*de la scienza*] is, as soon as one knows the principles of things natural, calculated and finished.
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> B. *la nostra vita, sì come detto è, ed ancora d'ogni vivente qua giù, sia causata dal cielo e lo cielo a tutti questi cotali effetti, non per cerchio compiuto, ma per parte di quello a loro si scuopra* (*Convivio* XIII.6). Thus, our destiny is caused, not by the calculated (celestial) circle but by that part which is above the earth.
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> 9 more examples from the *Convivio* and two from the *Divina commedia* could be quoted.
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> Knowledge of the Italian language and its development already rulse [*sic*, sorry / JH] out your claim. Firstly, the *pi* in the second syllable corresponds to a Latin *pl*, cf. *più* (<*plus*), *piano* (<*planum*), *piovere* (<*pluere*), etc. Secondly, the participle of *computare* is, and was in the later Middle Ages, *computato.*
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> 2. Where do you find that Karpinski should have supposed the algebra to be a secondary insertion in Vat. lat. 4826?[2]

I received a quick reaction, with apologies that I had not received the review

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[1] The context is a problem about a merchant who makes two voyages and earns in the same proportion to his capital on both. Quite a few similar problems are found in Fibonacci's *Liber abbaci*. One wonders that Oaks can believe that a merchant does not perform and finish his voyages but "computes" them.

[2] This refers to a passage on p. 4 in the online version. All further references are to the same version, since this appears to be regarded by the CSHPM as the official (*viz* "the complete") version.

Oaks spells "Karpinsky", suggesting that he never looked at the writings of this American-born historian.

directly as at least one colleague had, and the statement that

> I cannot answer your two questions quickly, so I will get back to you later.

Question (1), if a good answer exists, should ask for nothing but a quotation from a dictionary or a historical grammar, and question (2), with the same proviso, for nothing but a bibliographical reference – obviously to a publication after Karpinski's paper on Jacopo's *Tractatus* from 1929, which has the title "The Italian Arithmetic and Algebra of Master Jacob of Florence, 1307".[3] None the less, by 21 December I have heard nothing more. I therefore feel entitled to conclude (being as outspoken as Oaks himself) that

– Oaks believes that Jacopo wrote in English albeit in phonetic orthography – perhaps according to the famous US principle that "if English was good enough for Jesus, it is good enough for me"? In any case, with that level of understanding of Italian, how is he able to speak about what is found in abbacus algebra treatises? – Except for my translation of Jacopo, none of them exist in English.
– Oaks refers to what he believes Karpinski wrote but never controlled his belief and did not notice that it is in conflict with Karpinski's 1929 title – which does not prevent him from being utterly self-assured.

These are of course peripheral points. I include them because they illustrate how Oaks works and what it is that earns him the editor's gratitude "for putting so much care and thought into his evaluation"[4].

But let us go to something more substantial, if not really concerning my book then as underpinning of Oaks claim that Høyrup "does not understand certain fundamentals of medieval algebra" (p. 4). I refer to the question whether the original version of al-Khwārizmī's *Algebra* defined the fundamental second-

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[3] I find no obvious candidates in the supposedly complete bibliography in *Historia Mathematica* **3** (1976), 193–202. Indeed, since Karpinski did not know in 1929 that the Vatican and Florence manuscripts of Jacopo's *Tractatus* differ, he had no reason to doubt that the algebra (which is only in the Vatican manuscript) was genuine. Only Warren Van Egmond observed the difference and located a third manuscript (now in Milan), equally without the algebra, which made him suppose that the algebra in the Vatican manuscript was a mid-fifteenth century intruder. Prima facie, this was a reasonable working hypothesis, even though I am confident that precise analysis of all three texts rules it out (Van Egmond does not agree, I should add); in any case, as Oaks admits, the text of the "Jacopo" algebra predates the 1360s.

[4] *Bulletin CSHPM–SCHPM* no. 35 (November 2009), p. 27.

degrees “cases” (equation types) in normalized form and prescribed corresponding algorithms (starting with the halving of the first-degree coefficient), and whether Gherardo da Cremona’s translation does so. The extant Arabic text, as can be seen in all editions and translations, gives non-normalized definitions (“possessions equal to number”, etc.) combined with algorithms valid for the normalized cases (and with explanation of how to normalize non-normalized equations[5]); abbacus algebra, in contrast (beginning with Jacopo) gives non-normalized definitions and corresponding rules (starting with a division by the second-degree coefficient).

This discussion has several layers, the first of which has to do with Gherardo’s translation. Oaks claims that this translation was identical (on this account) with the extant Arabic text. In order to uphold his position, he supposes that a single indubitable singular form in the translation is an error (p. 8); elsewhere, the noun itself does not allow us to decide when it occurs in the nominative, since *census* (Gherardo’s translation of *māl* / “possession”) follows the fourth declension. Unfortunately, Oaks seems to be unaware that the Latin verb allows us to decide whether the subject is singular or plural if the shapes coincide. This should not be difficult to grasp for an English speaker, since the same holds in English in the third person singular, present tense (“the sheep is black” / “the sheep are black”). In October 2003 I sent Oaks a list of 9 instances where the verb shows a singular (and thus a normalized equation) to be meant (and two where *quinque census* is seen to take the verb in plural, demonstrating that Gherardo made a difference).[6] October 10th I received the answer

> Dear Jens,
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> This is just a short note to acknowledge that I have received your comments, and that I plan on pondering them for some time before getting back to you. Thanks very much for spending so much time writing up the comments! [...] .

Oaks never got back, unless the present review is meant to do so.

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[5] For some reason, Oaks considers (p. 9, text just before note 30) the presence of explanations of how to reduce other cases to the normalized standard case as a falsification of the view that there is a standard case, namely the case to which the rule “halve the roots ...” applies. Such explanations follow *after* the presentation of the rule and the first exemplification (“sample equation”, in Oaks’ terminology, “standard example” in mine).

[6] This list was part of an extensive commentary to a manuscript which I received from Oaks for commenting. I also received the manuscript for refereeing from *Historia Mathematica* and sent the same commentary through that channel. Even from the journal editor I received confirmation that the commentary had been forwarded (10 October 2003).

On p. 9, Oaks quotes me (with a minor imprecision which I put in square brackets) for the statement that "In [all] the Latin treatises, all cases except 'roots equal number' ... are defined as normalized problems", and objects that "This is false. Robert of Chester's translation of al-Khwārizmī and the Latin translation of Abū Kāmil's *Algebra* both give the plural". Looking at my phrase I recognize that it can lead to a slight misunderstanding in as far as Robert is concerned. His *headings* have a plural, but the first "sample equation" / "basic example" which defines the case is normalized in all second-degree cases (not in the first-degree case, where the normalized equation *is* the solution; in this Robert agrees with Gherardo). So, if Oaks mistakes my "problems" for "defining headings", he is right. But when referring to the translation of Abū Kāmil he errs even with this reading. He seems to be unaware that (e.g.) the phrase[7]

> Census autem qui numero equa⟨n⟩tur est (sc. sunt) ut dicas

means that the manuscript has

> Census autem qui numero equatur est ut dicas

while the editor points out that the correct translation of the extant Arabic manuscript (from the early thirteenth century CE) would be

> Census autem qui numero equantur sunt ut dicas

which indeed illustrates my point: that there is a gradual sliding within the Arabic tradition toward non-normalization, first affecting the definitions of the cases but later also the rules, and among the treatises I refer to only completed by Bahāʾ al-Dīn al-ʿAmilī around 1600. Abbacus algebra, however, descends from a branch of Arabic algebra where the transition was already completed. I suppose Oaks is going to state this in one of his next publications, admitting that I have said so but claiming as done repeatedly in the present review that my reasons for doing so are completely wrong.

Remains the question of the extant Arabic text of al-Khwārizmī. As Oaks points out, all Arabic manuscripts used by Rashed for his critical edition from 2007 agree on the account of normalization. What he neglects to observe is that all of them, as stated clearly by Rashed, postdate Gherardo's translation by at

[7] Ed. Jacques Sesiano, "La version latine médiévale de l'Algèbre d'Abū Kāmil", pp. 315–452 (here p. 326) *in* M. Folkerts & J. P. Hogendijk (eds), *Vestigia Mathematica*. Studies in Medieval and Early Modern Mathematics in Honour of H. L. L. Busard. Amsterdam & Atlanta: Rodopi, 1993.

least a small century,[8] for which reason this translation has to be taken seriously (as done indeed by Rashed).

I shall waste no more time on Oaks' way to account for what I say about Jacopo's algebra and its relation to the Arabic discipline – obviously, who does not use Oaks' terminology (which may be useful as an analytical tool but has no counterpart in the texts[9]) or who disagrees with him can have understood little about medieval algebra.

Sweeping general statements like "much of his evidence is invalid" (p.2) or "he does not understand certain fundamentals of algebra" (p. 4) are of course outside discussion if they are not backed by specific arguments. Anyhow, I shall take up a few points which Oaks is likely to regard as such backing.

When discussing the relation between the Vatican- and the Florence manuscript of Jacopo's *Tractatus* (**V** respectively **F**) I make a statistical analysis of the distribution of certain orthographic disagreements between them (e.g., *facto* / *fatto*), in order to measure the statistical significance of an apparently telling distribution. The nil hypothesis (an alternative that has to be ruled out) is the assumption "the 7 *fact* of **F** [are] distributed randomly over the relevant 35 *fact+fatt* of **V**" (finding that the odds for having no *fatt* in **V** correspond to a *fact* in *F*, the actual situation is 13.2%). Oaks tells me (double emphasis his) that "Because scribes *do* have preferences, the spellings will *not* result from random variation". Obviously he does not know what a nil hypothesis is, or does not recognize it if it is not named explicitly as such; moreover, he does not discover that he is not always right, not even regarding the scribes of the two manuscripts in question. As he can read at the same page of the book (p. 14), the distributions of *que* / *che* (**V**) and *ke* / *che* (**F**) *are* stochastically independent.

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[8] Roshdi Rashed (ed., trans.), Al-Khwārizmī, *Le Commencement de l'algèbre*. (Collections Sciences dans l'histoire), p. 86. Paris: Blanchard, 2007. Actually, Rashed writes that all existing Arabic manuscripts "lui sont postérieurs de plus d'un siècle", but the Medina manuscript O is from 1222 CE (cf. p. 85).

[9] If anything, the word "example" (explained then to be "basic" or "paradigmatic") corresponds better to the texts than "sample equations". In Gherardo's translation they are introduced by the phrase "sicut si dicas" / "sicut cum dicitur" ("as if when you say" / "as when it is said"), and similarly, in Rashed's version invariably "c'est par exemple lorsque tu dis". The words of the Arabic text, like those of the Gherardo translation, change slightly from case to case, "for example you say", "just about as if you say", etc. (translation Ulrich Rebstock, private communication).

Oaks is of course right that this does not *prove* that "one of the two manuscripts [is] derived from an original which the other one follows quite precisely on this orthographic account", and that "the common source of the three manuscripts may have been something in between all them" (or at least he is right as far as **F** and **V** are concerned). 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**) respectively *ditt* into *dict* (**V**), never making the opposite change, than that a single copyist should be characterized by such a weak but systematic tendency to slide in one direction (cf. the distributions of *que* / *che* / *ke* of the same copyists, which allows no similar explanation). But precisely because this is not proof (and because one should not rule out that something happened by accident if chances that it could do so are as high as 13.2%), I only claim that the evidence *suggests* one of the two manuscripts to be faithful to the original and the other to slide away from it, after which I investigate other phenomena of the same kind – among which the distribution of divisions *in* [namely, division *in* parts] versus division *per* [namely, division *by* a number], also mentioned by Oaks. Here, however, Oaks' alternative explanation is impossible; he forgets to mention that in **V** this distribution corresponds to a *system*, which is absent from **F**, and which was demonstrably not understood by the copyists, who freely speak about dividing "*by n* parts". Would anybody suggest that the original contained half a system, which the copyist of **V** restored without understanding it, whereas the copyist of **F** destroyed it completely? So, here as elsewhere, Oaks takes part of my arguments, leaves out other parts, and concludes that my inference is invalid. Of course, if one leaves out the parallel postulate in a report of the *Elements*, he may claim that Euclid did not rule out Lobachevsky geometry.

On page 5, Oaks claims that in the chapters that discuss algebra "we do not find the promised evidence [that the algebra in **V** must precede that of Paolo Gherardi even though it is not necessarily due to Jacopo himself]. Instead, he treats this conclusion as already established". Sorry, the evidence is in a scheme on p. 160, which lists shared examples (with and without shared numerical parameters and normalization division *in* versus *per*). This scheme also shows that the algebra of **V** and the two algebras closest to it have only rules but no examples for the higher-degree cases; in contrast, Gherardi (writing in 1328) and the whole tradition after him (in so far as it offers examples and not only rules) have such examples. Gherardi and much of the tradition after him also introduce false rules for irreducible higher-degree cases – rules whose falseness Jacopo, if he be the author of the algebra in **V** and had known about them, would probably not have been able to discover. I admit that the print is small (5 pt. Times Roman), but the scheme fills a whole page, and there are references to it in the text. It ought not to be possible to overlook it (and Oaks has indeed noted its existence but speaks of it on p. 6 in a way that does not suggest he read its details). If one wants to control whether this evidence is valid and is somewhat long-sighted, then he might use a magnifying glass.

In a paper from 2008 (mis-cited by Oaks in note 12 as 2007, but correctly later), Warren Van Egmond grouped a large number of abbacus algebras in families on the basis of the equation types they deal with and on the premise of progress within the family.[10] Since Van Egmond did not take any parameters beyond the list of types into account (thus neither agreement / disagreement of formulation or examples nor general character and level[11]), the outcome is dubious. In particular, Van Egmond locates the algebra of **V** within a family which he names after the mid-fifteenth–century abbacist Benedetto da Firenze but starting with the Florence manuscript Bibl. naz., Fond. Prin. II. V. 152 from around 1390; he therefore concludes that it is a late insertion in **V** (which is a copy datable by watermarks to c. 1450).[12] Oaks at first takes over Van Egmond's family construction, with the words quoted in note 12 and the polemical remark that "If Chapter 17 [containing the reducible higher-degree cases / JH] really dates to 1307, then the Benedetto family has a very large gap in time which cannot be accounted for". Unfortunately for his argument, he mentions evidence a few lines later (borrowed tacitly from my book) that Chapter 16 (containing the first- and second-degree cases) of the algebra from **V** is also in a manuscript from c. 1365 (Florence, Ricc. 2263, henceforth **A**), concluding that "while Chapter 17 of **V** belongs to a tradition which began later in the century, **V**'s Chapter 16 was

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[10] "The Study of Higher-Order Equations in Italy before Pacioli", pp. 303–320 *in* Joseph W. Dauben et al (eds), *Mathematics Celestial and Terrestrial*: Festschrift für Menso Folkerts zum 65. Geburtstag. (Acta Leopoldina, 54). Halle: Wissenschaftliche Verlagsgesellschaft, 2008.

[11] I imagine (but this *is* an imagination based on no direct evidence whatsoever) that Van Egmond relied upon the material he collected for his magnificent doctoral work in the 1970s and had no occasion to control these parameters.

[12] Actually, Van Egmond uses an even stronger formulation (p. 313), namely that the algebra was "undoubtedly added to a manuscript containing some sections copied from Jacopo's earlier work". On Van Egmond's premise that *F* represents the original version, **V** does not copy "some sections" but almost everything (the main omission being a list of higher squares and products and, strange accident, all problems making use of the unit *canna* and of the method known as *welsche Praktik*).

originally written no later than 1365".[13] What Oaks does not say is that the agreement is verbatim (spellings and forgotten words apart), and that **A** also contains the material from Chapter 17 of **V**, still almost verbatim,[14] and sharing with **V** a lacuna of five omitted words (pointed out in my edition, p. 323). **A**, however, adds examples to four of the higher-degree cases it shares with **V**; these same examples are also found in the algebra section of Gherardi's *Libro di ragioni*, but in their choice of words Gherardi and **A** are much less similar than **V** and **A**.[15] **A** can thus be seen to be a mixed descendant of **V** (or an immediate source, given the shared lacuna) and of a precursor shared with Gherardi. Oaks' whole construction would have fallen to the ground if he had looked into the texts.[16] *If* Oaks is right in claiming (still p. 6) that "Warren Van Egmond provides the best argument in his article" that "the algebra in **V** does *not* belong to the 1307 original", then we must conclude that no good arguments can be found.

Oaks' claim on the same page that "the general trend in abbacus manuscripts from simple to complex, and toward improved organization is turned upside down if we place **V**'s Chapter 17 in 1307" is equally mistaken. Firstly, there *is* no such general tendency; it seems to be there within Van Egmond's single families because this is the premise he uses to construct them, and Oaks is thus caught in his own circular "reasoning" (quotes because Oaks merely claims and does not state the reasons). Secondly, the trend from simple to complex, to the extent it can be seen in the gross picture, involves the appearance of an increasing number of false rules, and here it fits very well that **V** precedes Gherardi (who offers the earliest false rules). Thirdly, if he had looked at the manuscript from

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[13] In contrast, Van Egmond treats Chapters 16 and 17 of **V** as one piece. Oaks seems to try to save his polemical remarks by introducing the distinction.

[14] In two places, **A** has "de'" (modern Italian *devi*) where **V** has "si vole" respectively "vole"; in one place, **A** has "chavatone" where **V** has "meno". There is complete agreement between the two manuscripts in the distribution of *in* versus *per* in the normalization division.

[15] Gherardi is also unaware of the double solution in the case *censi* and *number* equal to *things* and skips the corresponding case *cubes* and *things* equal to *censi*, present (together with the mirror image *censi* equal to *cubes* and *things*) in **A** with indication of the double solution (**V** only has the mirror image, but also has the double solution).

[16] He might even have limited himself to looking at the couple of lines that separate Chapters 16 and 17 (p. 320 in the edition): "Here I end the six rules combined with various examples. And begins [*sic*] the other rules that follow the six told above, as you see". Obviously, the writer thought the two to belong together.

which Van Egmond starts the "Benedetto family"[17] he would have discovered that this is a very sophisticated work, showing for instance how cubic equations with a second- but no first-degree term can be transformed into equations with a first- but no second-degree term and discussing the sequence of powers as a geometric series, far beyond anything given (or, most certainly, understood) by Jacopo.

On p. 5, Oaks quotes me for saying that Chapter 22 of **V**, a collection of mixed problems, "seems to overlap Chapters 14–15. At closer inspection, however, the apparent overlap turns out to consist of duly cross-referenced variations and supplements; no single genuine repetition can be found. This would hardly be the case if a later hand had glued another problem onto an original shorter treatise ...". Oaks objects that "all cross-references but one are made to problems *in the same chapter*" and accompanies this by a supposedly complete list (actually incomplete, even as regards the internal cross-references in Chapter 22, but that is unimportant). The one external cross-reference he finds he explains away (it "could easily be to another part of the (now lost?) book from which the chapter was taken. Or, equally likely, it might have been inserted by the scribe responsible for collecting together the different parts of **V**". He forgets (and never mentions) the evidence that two successive copyists (and all copyists back to the common ancestor of **V** and **A**) have been very meticulous exactly as copyists, preserving empty space within the line where a calculation was not performed, and conserving notes about things coming in wrong order because of an initial skipping of part of the coin list. He also overlooks a second external but implicit cross-reference, "I want to know how many square *braccia* [the area of a circle] is without espying the circulation around" (p. 352 in the edition). This is in the first circle problem in Chapter 22, and refers to the main way of finding the circle area in the geometric Chapter 15 (starting from the perimeter, which is very rare in abbacus geometries except those that were written in Provence in the early fourteenth century, and thus not likely to be present in Oaks hypothetical "(now lost?) book").

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[17] A fully adequate edition of the algebra was made by Raffaella Franci & Marisa Pancanti: Anonimo (sec. XIV), *Il trattato d'algibra* dal manoscritto Fond. Prin. II. V. 152 della Biblioteca Nazionale di Firenze. (Quaderni del Centro Studi della Matematica Medioevale, 18). Siena: Servizio Editoriale dell'Università di Siena, 1988.

Raffaella Franci has also discussed the contents of this algebra on several occasions.

Oaks mentions the manuscript on p. 6 (as *Tratato Sopra l'Arte Arismetricha*) and speaks about it as if he knew it directly. Obviously he forgot to insert the words "according to Van Egmond".

Oaks goes on to claim that “contrary to H.’s assertion, the wide range of parameters chosen for mixed problems in abbacus texts make (*sic*) non-repetition likely”. Once again he speaks without thinking. Those problems where repetition would have been likely to occur are precisely those where the same parameters recur very often (circles with diameter 14 and perimeter 44, ropes of length 50 *braccia* going from the top of a tower 40 *braccia* high and reaching across a moat 30 *braccia* large); problems with these parameters are found in Chapter 15 as well as in Chapter 22.

I could go on, but since every refutation of a fantasy which Oaks scribbles on a couple of lines asks for a cumbersome page of arguments, this would soon becomes another book. I shall also limit myself to mentioning only one of his instances of cheap rhetorical tricks. On p. 6 Oaks states that “even H. acknowledges that Chapter 18 [...] does not seem to fit the surrounding chapters” – as if anybody had noticed before that it does not! I do not “acknowledge”, I point out and discuss.

The passage [...] in the letter from Oaks quoted on p. 3 includes the admission (in connection with a mistake which first Barnabas Hughes and then I had pointed out to him[18]) that “I should have thought twice about what I wrote”. He still should, I suppose. With a jibe sometimes attributed to Einstein: “I have no objections to the fact that you think slowly, Professor; but I do have objections to the fact that you speak more swiftly than you think”.

Jens Høyrup
December 21, 2009

[18] Not knowing that *census* follows the fourth declension, Oaks had believed that Gherardo used the singular even when a numeral indicated a plural. Cf. above, note 6.