Stemmata, Philology and Textual History: A Response to Alberto Varvaro

It is agreeable to read the positive observations that Alberto Varvaro makes in his review article in *Medioevo Romanzo* about some aspects of my work on Froissart's *Chroniques* (XXXIV, 2010, pp. 145–153); I also take it as a compliment that he has devoted no less than nine pages to a discussion of my essay on the textual tradition of Froissart's Book III. However, as his criticisms of my essay lead him to reject all of my conclusions out of hand, I have felt it necessary to respond at least briefly to some of his objections.[1]

I remain convinced that, despite his repeated assertions to the contrary, Varvaro and I essentially agree on the methodology, disagreeing only on particular arguments, but on a more fundamental level we certainly hold different — though complementary — views on the purpose of stemmatic analysis. For Varvaro its only possible use is towards a critical edition: a stemma allows a scholar to select a base manuscript and witnesses whose variants need to be taken into account.[2] As a textual historian and a historian of the book however I am interested in stemmata because they can elucidate aspects of a text's history, such as its reception history or the conditions under which particular copies were produced. I have no wish to publish an edition of Book III of the *Chroniques* (critical or not), but am fully aware of the importance of a stemma for anyone wishing to engage in such a project, which is why I summarised the implications of my work for such an undertaking (n. 66).[3] Our different aims also explain why Varvaro's own research on Book IV has led to an outcome which is satisfactory for him, because it allows him to select a suitable base for his edition, but which is far less so from my point of view, because one third of the textual tradition is not allocated a place in his stemma (my p. 45).

The same considerations partly explain also our differences of opinion as regards the importance of P50 in any research on Book III. Varvaro's logic is as follows (pp. 146–147): first one has to test Kervyn de Lettenhove's and Mirot's conclusion that P50 contains a different authorial redaction. If one concludes that it does, then the singularly most important question is whether P50 represents the older or younger version. If one decides that the latter is the case (as both Kervyn and Mirot do), then there is no reason to give serious consideration to the other manuscripts, as any scholar would only want to edit the latest expression of authorial intent.

I see several problems with this reasoning. First, I remain unconvinced by Varvaro's methodological doubt about the *communis opinio* regarding the differences between P50 and the other manuscripts. As he himself says, Kervyn 'ne dà una lista minuziosa' (p. 146), which is obviously why 'Croenen non dà indicazioni precise' (n. 12).[4] All scholars who have

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[1]I am grateful to my friend and colleague Alberto Varvaro for the frank exchange we had leading up to our respective contributions. I am equally grateful to Professor Lino Leonardi for accepting my text here in *Medioevo Romanzo*.

[2]That is, for example, why Varvaro is surprised that I did not 'eliminate' the *descriptus* P605 (n. 17). Varvaro appears to belief that this MS was copied by Raoul Tanguy (p. 147). This is mistaken and would suggest that he is unaware of *P605*'s palaeographical and codicological characteristics. Cf. my p. 28 and 58.

[3]Strangely enough Varvaro comes to the same conclusions as I do and even calls them a 'colpo magistrale' (p. 150) but then claims that I fail to understand their importance.

[4]The variant chapters are § 109–110, § 120–122, § 145, § 261, § 276–278, § 288–290 and §

written about this agree that the pattern of rewriting in Book III is entirely similar to that found in the various authorial versions of Books I and II: the text of many chapters remains unchanged, some chapters are rewritten, and some chapters are added or deleted. If Varvaro has good reasons to doubt the validity of this conclusion then he should tell us why, because 'lo studioso ha il dovere di esaminare ed esporre gli argomenti' and not dismiss other scholars 'ciecamente' (p. 147) because their conclusions do not suit him. Varvaro's suggestion that Bes, Mon and Bre are 'interventisti' (p. 147) and therefore comparable to P50 is misguided, as should be clear from my discussion of the scribal variation in these manuscripts (pp. 42–44). The variation in Bes and Mon has resulted in an *abridged* text, not a *different* text.[5] The differences in Bre are caused by scribal rephrasing in an effort to 'improve' the text, as Varvaro himself notes in his study of Book IV. This again is very much unlike P50, whose readings normally concur very closely with those of the better witnesses, except in those chapters that have been rewritten by the author.

To revisit Kervyn's conclusions about the differences between P50 and the rest of the textual tradition would require a separate inquiry, but one which, despite Varvaro's assertion that this 'doveva essere fatto prima' (n. 10), could not be conducted in a methodologically sound way unless one knew exactly which witnesses to compare the variant chapters to. One would therefore first need a stemma of the other redaction before any meaningful detailed textual work could take place. This would also require the preparation of editions of the relevant chapters, which would largely duplicate the work currently being undertaken by Peter Ainsworth (my n. 5). That is why I decided to delay my study of the relative chronology of the redactions until Ainsworth's edition is completed (my n. 40). Such an inquiry remains very important, because I do not accept, as Varvaro claims, 'a occhi chiusi la loro [=Kervyn's and Mirot's] conclusione piú importante', i.e. 'che ci siano due redazioni e che P50, e solo P50, ci trasmetta la seconda' (p. 146). I only subscribe to the first part of this statement and am agnostic about the last (my pp. 20–25).

There is a further reason why from a book historian's point of view it is fully justified to delay such study. If P50 is the single witness to one of the two known authorial redactions of Book III, then the only difference a study of the relative chronology could make is to the order of O1 and O2. The rest of the stemma would stay entirely the same, as the relations between the descendants of O1 would not be affected. There is no evidence of contamination between the witnesses of the two redactions, and the P50 text probably had a small reception anyway (my n. 20). That is why a book historian is mostly interested in that part of the stemma showing the "first" redaction, even if for the philologist only P50 counts because 'è questo il testimone che deve essere alla base di una futura edizione' (p. 147).

P50 also plays a crucial part in the third fundamental point on which Varvaro takes issue with my research. He claims that I fail to understand that 'in filologia è l'errore che importa' (p. 147–148), an assumption which underlies all of his further comments. This is of course a simplistic formulation of the classical philological method and Varvaro must realise that it is not the errors *per se*, but the non-original readings that matter, because it is these that can reveal the common ancestry of witnesses. In most cases we can only distinguish between non-original and authorial readings because the former are in one way or other 'faulty'. In the case of Froissart's Book III however we are not limited in this way. Indeed, if we accept that P50 represents a different authorial version (and we must do so until the time when Varvaro or someone else presents valid counter-arguments), then any agreement between a reading of

306–307 of Mirot's edition; § 308 is absent from P50.

[5]The abridgment in Bes is relatively minor as well as localised, and has a codicological explanation.

P50 and any of the other witnesses 'deve essere considerata come prova che tale lezione risale all'autore' (p. 147, also my p. 27). As a corollary we can identify the alternative readings in these passages as non-original and therefore potentially useful, even if under usual circumstances they would be considered 'varianti indifferenti' (p. 150–151). That then also dispenses us from having to establish in each case 'quale lezione sia erronea e quale no' (p. 150).

This is the methodological justification for why otherwise insignificant variants as 'requoy / privé' (example 7) and 'requoy / couvert' (example 18), and the examples 8 and 9, justify the definition of the α family.[6] This is also the basis for stating that the variants given in examples 11, 14 and 15 show that Bes cannot be the ancestor of the rest of the α family, but B88 could, because unlike Bes, every time B88 has non-original readings they are repeated (sometimes adulterated) in all the members of the β, γ and δ families.

Varvaro ignores this logic and claims that nearly all the variant places quoted are 'varianti indifferenti' (p. 150). He also requires that I give 'la minima prova' for the hypothesis that B88 is indeed the ancestor of the β, γ and δ families, which 'peraltro non sarebbe facile, dato che B88 è un misero frammento di cui non restano che otto fogli' (p. 149). This is a most curious statement, as since Karl Popper it is usually well understood that such a hypothesis could not be proven in any epistemologically meaningful way, but could only be falsified. In order to do so it would suffice to find a variant place where B88 has a non-original reading and where at least one member of my β, γ and δ families has a reading also found in P50. Despite having collated the entire eight surviving folios of B88 (which contain significantly more text than Varvaro used for his article on Book IV) I could not find any such variant place; every time B88 has a non-original reading it is also transmitted across the β, γ and δ families. The same counter-argument applies to Varvaro's insistance that I *prove* that P50 represents a separate authorial redaction (pp. 147, 149), which is equally non-sensical if he thinks it could be possible to adduce stemmatic proof. To disprove the hypothesis however it would suffice to find a common error shared between P50 and any other witness of Book III.[7]

Space does not allow me to comment on the other textual families, just as for practical reasons it was necessary to cut back substantially on the original draft of my essay, including the range of comments, variants and witnesses to be included. I will therefore only comment on one final aspect: the implication that two different academic standards should be applied to judge Varvaro's work, who as 'uomo solo ... non h[a] mai disposto né di collaboratori né di finanziamenti' (p. 148), and my own. When Varvaro states that I am 'aiutato da alcuni collaboratori' (p. 145) he has misunderstood my situation, because like him I had to carry out my research largely on my own. The relatively modest support I received from the British Academy allowed me (amongst other things) to pay my two PhD students for ten days to help me with photocopying and transcription work, but most of the texts were still transcribed by myself; I also carried out all of the textual analysis.

Having had the opportunity over the last year to reconsider the material on which my stemma is based I am more than ever aware that it is open to improvement (p. 55–56). I remain convinced that P50 represents an authorial version (O2) different from the rest of the

[6]It should be clear that in both cases 'requoy' is the authorial reading, found in P475 (and P605), L67 and P50, witnesses independently descended from the two authorial versions.

[7]The material to do so is available online: *The Online Froissart*, ed. by Peter Ainsworth and Godfried Croenen, version 1.1 (Sheffield, 2010), <http://www.hrionline.ac.uk/onlinefroissart> [accessed 3 June 2010].

manuscript tradition of Book III (regardless of its relation to O1). I also remain confident about the basic soundness of the textual families α , δ and β. As I have already indicated (p. 42–44, 56) the Mon and Bre copies are particularly difficult to place satisfactorily. Recent research seems to indicate that they may actually be part of the β family (or that β and γ share a common ancestor which would be a sibling of δ).[8] The study of the Books I and II parts of B88 may suggest that the β, γ and δ families were not actually derived from B88 but from a lost sibling almost identical to B88. Leaving orthographical variants aside, the non-original readings of P475 and L67 are almost always unique and therefore stemmatically useless. In the few cases where they are not, the variants are probably parallellistic (Varvaro, p. 149). I concluded on that basis that there are no good arguments for introducing further hypothetical ancestors between O1 and any combination of α , L67 and P475. It remains to be seen if that part of the stemma can be improved upon.

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[8]Dirk Schoenaers, "*Getranslateerd uuten Franssoyse*: Translation from French into Dutch in Holland in the 15th century. The case of Gerard Potter's Middle Dutch Translation of Froissart's *Chroniques*", PhD thesis University of Liverpool, 2010, chapters 2 and 3.