

The Classical Review

<http://journals.cambridge.org/CAR>

Additional services for *The Classical Review*:

Email alerts: [Click here](#)

Subscriptions: [Click here](#)

Commercial reprints: [Click here](#)

Terms of use : [Click here](#)



Vindobonensis F and the Text of Plato

John Bubnet

The Classical Review / Volume 17 / Issue 01 / February 1903, pp 12 - 14

DOI: 10.1017/S0009840X00207142, Published online: 27 October 2009

Link to this article: http://journals.cambridge.org/abstract_S0009840X00207142

How to cite this article:

John Bubnet (1903). Vindobonensis F and the Text of Plato. The Classical Review, 17, pp 12-14 doi:10.1017/S0009840X00207142

Request Permissions : [Click here](#)

VINDOBONENSIS F AND THE TEXT OF PLATO.

AFTER Mr. Adam's review of my edition of the Republic in *C.R.* xvi. 215 ff. and Mr. Stuart Jones's article in *C.R.* xvi. 388 ff., Vind. 55 (suppl. phil. gr. 39) can no longer be called 'a neglected MS. of Plato.' Mr. Adam admits that in future, as the oldest representative of the class to which it belongs, F should be quoted instead of Laur. plut. 85. 7 (= Stallbaum's Flor. x., Schneider's Flor. R) and Ang. C, l. 7 (= Bekker's v, Schneider's Ang. B). There is no doubt of the seniority of F: it is written in a fourteenth century hand, and was, as appears from the *subscriptio*, in possession of Franciscus Barbarus in 1420, the very year in which Flor. x was written, while Ang. v is not earlier than the sixteenth century. In my opinion, Schanz has proved that x is derived from F (*Platonis opera* IX., pp. x, xi). Mr. Adam doubts this, and refers them to a common original. Similarly, I believe that Schanz has shown v to be derived from x (*Platocodex* pp. 73, 79 ff. 106 ff.). Here again Mr. Adam has doubts, and he might have appealed to the authority of Jordan, (*Hermes* xiii. 470 ff.), who derives F with xv from Laur. plut. 59, 1 (Schneider's Flor. A). I have not been able to accept this, because, though there is an undeniable relationship between the two MSS. in certain passages, it does not extend to the greater part of the Republic, and because there are many things which point to the independence of F. I cannot see my way, on the other hand, to believe that xv are independent of F, because they are full of readings that can be explained only as misunderstandings of the corrections made in F by a later hand. We are, I think, forced to conclude, either that xv are derived from F, or that by some strange coincidence, after F had been written, the same corrections were made in the supposed common original of F x v as were made in F itself by the later hand f. This is not an easy hypothesis, and therefore I do not adopt it. The main point is, however, that even if Mr. Adam were shown to be right, that would only strengthen my case. I am trying to prove the existence of a tradition independent of ADM in the Republic; but I only feel justified at present in calling a single witness to that tradition. Mr. Adam's argument, if it were conclusive, would show that there are at least three.

Mr. Stuart Jones also admits all that is vital to my case, namely that F 'follows a

tradition not derivable from ADM,' thus as he says, 'to a considerable extent rendering an appeal to the MSS. of the Renaissance (esp. Ξ q) superfluous.' This is the point for which I am contending. The Renaissance MSS. (and especially Mon. q) are full of readings which no one would dream of ascribing to anything but conjecture. When, therefore, we are driven to rely on their authority, we cannot be sure in a single case that we are relying on genuine tradition. Such readings have precisely as much or as little authority as the printed emendations of Renaissance scholars. Mr. Jones points out, it is true, that F too contains conjectures. That is the case, but they are of the harmless mediaeval type which is comparatively easy to deal with. F very often inserts words with the intention of making the grammatical structure of a sentence clear, and thereby it often, as in Rep. 330, c 6 (a case referred to by Mr. Jones) completely destroys the real meaning of a passage. But Byzantine interpolations of that class are a very different thing from the more plausible emendations of the Italian Renaissance. I hold, therefore, that I was quite entitled to say of F that in many places it alone 'preserves' the true reading, even if that reading happens to occur also in one or more Renaissance MSS. The burden of proof must always lie with those who hold that, in any given case, the true reading has been 'preserved' by MSS. like Ξ and q.

Mr. Jones's main argument, however, is not directed against this position, which is substantially his own, but against the hypothesis of the 'ancient vulgate.' With regard to that hypothesis, I wish to say that it was not invented by me and that my case for F does not stand or fall with it. The problem is an interesting one in itself, especially for the light which may be thrown by its solution on the text of the papyrus fragments. I do not think Mr. Jones has succeeded yet in overthrowing the theory, though perhaps I went too far in using the words *vel ex mea adnotatione satis apparebit*, if that is to be construed strictly as referring to the Republic alone. The fact is, as Mr. Jones allows, that, for a final solution of the question, it will be necessary to examine the text of the other dialogues as well, and I am at present engaged upon that task. When it is completed I shall be happy to discuss the

point again; for the present I prefer to say no more about it.¹ I will only observe that, even if the result of further inquiry were to confirm Mr. Jones's negative conclusion as to the 'ancient vulgate,' that would only strengthen my case for F. If F represents an 'ancient vulgate,' then its readings and those of the *testimonia* taken together only amount to the evidence of a single and not very trustworthy witness; but, if there was no such 'vulgate,' the very numerous agreements² between F and the quotations in ancient writers become more striking still. In any given case of agreement, we shall then have two witnesses instead of one, and F must be placed on a higher pedestal than I have ventured to claim for it. The working hypothesis—for it is nothing more at present—of a *vetus vulgata* is chiefly useful in enabling me to state the case for F in the most moderate form possible.

To return to the question immediately in hand, the relation of F to the leading MSS., I am now in a position to strengthen the case very considerably. I owe this to the great kindness of Prof. J. Král of Prag, who has collated a considerable portion of the MS. for me, there being no published collation except in the Republic. None of the dialogues which come before the *Gorgias* in the traditional order are con-

tained in F, but from that point onwards we have its help. I am now able to state that, just as in the Republic F is independent of ADM, so in the *Gorgias* and *Meno* it is independent of BTW. In Tetralogy VII, which is not contained in the Clarkianus, it is similarly independent of TW, though in the *Hippias minor*, *Io*, and *Menexenus*, it is very closely related to Bekker's Σ (Ven. 189), a fact already noted by Schanz (Platonis Opera IX, pp. xiv, xv). Schanz made considerable use of Σ (which he names S) in these dialogues, and he called attention to the agreement of that MS. with Dionysius of Halicarnassus and Stobaeus in the *Menexenus*. If any one cares to compare the readings of S in that dialogue³ with those of the two writers named, he will find that they raise the same sort of problem as the passage of Eusebius which I adduced. The readings of F are practically the same; but the precise relation between F and S (which is of earlier date than F) must be left for future inquiry.⁴

In order to prove the independence of F, I begin as in my former article (*C.R.* xvi. 98ff.) by showing that, from the nature of the errors which it makes, it must have been derived from an uncial codex. The following examples will make this clear.

GORGIAS.

BTW	F
448b ἡρόδικος	πρόδικος
467b σχέλια	ἐχεται ᾧ
505d ἀτελῇ	ἀτε δὴ
522e λέξαι	δέξαι
525c ἐν αἰδου	ἐδίδου

MENO.

BTW	F
70b εἴθικεν	τοικεν
80d εἶσει	ἰσθι
93b παραληπτὸν	γὰρ ἀληπτον

HIPPIAS MAIOR.

TW	F
286b φειδοστράτου	φιλοστράτου
287d μέλλει	με δέι
291a πᾶσι τοῖς	πλείστοις
292b δέξει	λέξειν
293a ἀεί	δῆ

³ A selection from them is given in the Appendix to Schanz's Plato, vol. IX.

⁴ The agreement of F with S does not appear in any dialogue before the *Hippias minor* for reasons given by Schanz (*loc. cit.*). Nothing, therefore, can be inferred from it with regard to the text of S in dialogues not contained in F.

¹ I must, however, at once correct an inadvertence on the part of Mr. Jones, which has led him to misrepresent my view. He attributes to me the statement that the recension represented by our best MSS. of Plato, was made 'possibly about the ninth century A.D.' Such a view has been, and is, held; but I have never been able to believe that anyone in the ninth century could have made a 'recension' of Plato. What I said was 'the 9th century A.D.,' not 'the ninth,' and I mentioned that date because I think it is the latest possible one. I should have no objection, so far as this argument is concerned, to putting the date of the recension earlier still. If, for instance, we were to adopt Usener's theory of the history of Plato's text, we should obviously have to do so. I may be allowed also to say that, in the *Praefatio* to vol. ii. of my Plato, in dealing with the views of A. Schaeffer, I expressly rejected the theory of a ninth century recension, so far as the Clarkianus is concerned.

² The number of these agreements is far greater than any one would be led to infer from what Mr. Jones says; but it is not so much their number as the peculiar way in which they occur that constitutes the real problem. It is not a question of making 'lists' of agreements. These would prove nothing. Mr. Jones himself has adopted the only sound method, namely that of taking passages of considerable extent and studying every variant in them. That is what I did also in my article (*C.R.* xvi. 98 ff.). I took a long passage which is quoted by Eusebius, and I noted almost every variant of importance. If that passage stood alone (and it certainly does not) it would of itself raise the problem.

TW	F
„ d εὐήθη	εὐνῆ
„ d γὰρ ἔχω	παρέχω
294d ἀγνοεῖσθαι	διαλέγεσθαι
298a θράσει	δράσει
299d εἰ τις αὐτῷ	ἐπὶ ἑαυτῷ
„ εἰς δ	οἷσθ'
300b αἱ ἡδοναὶ	δῆλον αἱ
302b ἐλεγχθῇ	ἐδέχθη

HIPPIAS MINOR.

TW	F
368b οἶαι αἱ πέρικαι	οἶμαι περ εἰ καὶ
371c κλισίη	καισίη
„ e εὐηθείας	εὐνείας
„ e ἡ οἱ	μοι
376a διὰ δύναμιν	αιαδυναμια

MENEXENUS.

239d δεῖ δὴ	δῆλη
-------------	------

Occasionally F alone has read the uncials correctly, while the leading MSS. have gone wrong, e.g.—

GORG. 492b. ἐπεὶ ὅσοις F: ἐπεὶ θεοῖς BT: ἐπεὶ δὲ οἷς W. The reading of F is new and obviously right, as it alone will account for the corruptions. The generally accepted reading, ἐπεὶ γε οἷς is based by Schanz on the authority of 'recens b,' and is clearly an attempt to repair the damage after it had been done. The ἐπεὶ τοῖς of the corrector of T is a less happy attempt of the same kind.

ib. 492d. ἀμόθεν γέ ποθεν (sic) F: ἄλλοθεν γέ ποθεν BTW. In Schanz's edition the true reading figures as Bekker's emendation.

ib. 514a. φῶμεν F: θῶμεν BTW. In Schanz's edition φῶμεν is adopted as a conjecture of Madvig's.

526d. ἀσκῶν F Eusebius: σκοπῶν BTW. Schanz does not adopt ἀσκῶν, but it is doubtless the true reading. It is noteworthy that two lines above T has γρ. ἀσκῶ as a note on σκοπῶ.

MENO 89e. ἄνυτος (bis) F: αὐτὸς (bis) BTW. The true reading has hitherto rested on the authority of 'Flor. x.'

HIPP. MA. 291d. ἐπαίειν F: ἐπαινεῖν TW. Schanz can only quote for ἐπαίειν 'γρ. apogr. E, Muretus.' (By 'apogr. E', he means E).

In conclusion, I wish to call attention to one or two correct readings of F which bear the mark of the highest antiquity.

GORG. 472b. ἐν πυθίον F: ἐν πυθοῖ BTW. The reference is to the ἀνάθημα of Aristokrates, son of Skellios, and the reading of F is confirmed, if confirmation were needed, by C.I.A., i. 422, 'Ἀριστοκράτης Σκελίου ἀνέθηκεν νικήσας Κεκροπίδ[ι] ἐν ἑορτῇ . . . Πυθίου. The right reading is already found in Stephanus, who no doubt got it from the second Basel edition, but it has up to the present had no authority but Par. 1815 (Bekker's I), a 16th century MS., which, in this part of the Gorgias, follows the text of F very closely indeed.

ib. 487c. τείσανδρον F: τίσανδρον BTW. No other MS. has succeeded in preserving the true spelling of the name.

ib. 493b. ἀιδὲς F: ἀειδὲς BTW. Here F alone has preserved the true form, to which attention was first called by the Petrie papyrus fragments of the Phaedo. Cf. my critical note on Phd. 79, a 4, from which it will be seen that T preserves the correct form in the Phaedo, though it has not done so here.

I think I may claim that a MS., which preserves such readings as these, is not to be lightly set aside. Whether it represents an 'ancient vulgate' or not, it certainly represents, though in a mutilated form, an older tradition in some respects than the leading MSS, and thus enables us to find a more solid basis for the text of Plato.

JOHN BURNET.

PLATONICA.—V.

(Continued from Vol. XVI. p. 16.)

THEAETETUS.

143 A δσάκις Ἀθήναζε ἀφικοίμην, ἐπανηρώτων τὸν Σωκράτη δ μὴ ἐμνημήμην.

δ μὴ μεμνήμην seems much more likely, and these perfect optatives are very liable to corruption.

148 c εἰ σε πρὸς δρόμον ἐπαινῶν μηδενὶ οὕτω δρομικῷ ἔφη τῶν νέων ἐντετυχηκέναι, εἴτα διαθέν τοῦ ἀκμάζοντος καὶ ταχίστου ἡττήθης, κ.τ.λ.

τοῦ ἀκμάζοντος καὶ ταχίστου seems an impossible expression. 'The swiftest runner' is