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passage of scholiastic character in Clem. Al. *Ecl. Proph.* p. 154, 4 Stählin (according to whose index it is the sole example in Clement), but it may be due there to the excerptor and not to Clement himself. Examples from v/A.D. are Cyr. Al. *Comm. in Is.* 53, 10-12 (*P.G.* 70, 1188A and 1189D), Procl. *In Eucl.* p. 122, 1 Fr., Pap. Flor. 384, 29. I have not, however, in spite of a fairly extensive search, noticed another instance in Athanasius.

W. L. LORIMER.

THE CHESTER BEATTY PAPYRUS AND THE CAESAREAN TEXT.

THERE has hitherto been a general disposition to treat the Chester Beatty papyrus as a witness to the Caesarean text in the Gospel of Mark, in spite of the doubt somewhat too modestly expressed by Burkitt. The impression began with Sir Frederic Kenyon's introduction, in which he finds in the agreements of the papyrus with W, Θ , famm. 1 and 13 evidence of its strong affinity with this text. Its total agreements with this group of MSS in Mark are, he says, 323, as against 189 with the group NABD . 'It is clear, therefore,' he continues, 'that in this Gospel the character of the text is definitely Caesarean.' This conclusion, however, overlooks the fact that the Caesarean text was reached by pooling the disagreements of the group of MSS in question with the Textus Receptus. Canon Streeter expressly described it as 'the text which results from the purely objective process of deducting Byzantine readings from members of Fam. Θ '. He even said that 'only those variants of Fam. Θ which differ from the Byzantine text (which practically = the Textus Receptus) are worth quoting at all'. A glance at Sir Frederic Kenyon's apparatus criticus, however, shews that a large proportion of the agreements of the papyrus with members of the Θ group are also agreements with the Textus Receptus, and consequently 'not worth quoting at all' as witnesses to the Caesarean text. The list, given on another page of the Introduction, of readings which do not appear in the Textus Receptus, doubtless presents a truer picture of the affinities of the papyrus with the hypothetical Caesarean text.

There is another error, however, in Sir Frederic Kenyon's calculation. The 323 agreements with the group are obtained by adding together the agreements with its individual members, so that an agreement with W and Θ counts as two, an agreement with Θ fam. 1 and fam. 13 as three, and an agreement with all four as four agreements. The total number of agreements with the group is in fact more than twice as many as the total number of variants noted. This is an obvious oversight, and the question of the Caesarean affinities of the papyrus evidently requires

reconsideration. As, moreover, 28, 565, and 700 are equally regarded as belonging to the family, their agreements should also be taken into account, and the agreements of the whole group and its various combinations should be properly weighed.

A tabulation of the agreements of the papyrus with these seven authorities, based on an independent collation, in which, however, I have followed Sir Frederic Kenyon's example in ignoring variants peculiar to D, leads to the following results :

	With ζ .	Against ζ .	Total.
W@ famm. 1. 13. 28. 565. 700	16	2	18
W@ famm. 1. 13. 565. 700	3	—	3
W@ fam. 13. 28. 565. 700	3	6	9
W@ fam. 1. 28. 565. 700	2	3	5
W@ famm. 1. 13. 28. 565	—	4	4
W@ famm. 1. 13. 28. 700	3	—	3
W famm. 1. 13. 28. 565. 700	3	—	3
@ famm. 1. 13. 28. 565. 700	5	1	6
W@ famm. 1. 13. 28	1	—	1
W@ fam. 1. 565. 700	—	2	2
W@ fam. 13. 28. 565	—	1	1
W@ fam. 13. 28. 700	2	—	2
W@ fam. 13. 565. 700	1	1	2
W@ 28. 565. 700	1	2	3
W famm. 1. 13. 28. 565	1	—	1
W famm. 1. 13. 28. 700	4	2	6
W fam. 1. 28. 565. 700	—	2	2
W fam. 13. 28. 565. 700	1	—	1
@ fam. 1. 28. 565. 700	1	—	1
@ famm. 1. 13. 565. 700	—	1	1
@ famm. 1. 13. 28. 565	1	—	1
Famm. 1. 13. 28. 565. 700	3	1	4
W@ famm. 1. 13	1	—	1
W@ fam. 1. 28	—	1	1
W@ fam. 13. 28	1	—	1
W@ fam. 1. 565	—	3	3
W@ 28. 565	—	1	1
W@ 565. 700	1	—	1
W famm. 1. 13. 28	8	2	10
W fam. 13. 565. 700	1	—	1
W fam. 13. 28. 700	2	—	2
@ famm. 1. 13. 565	1	—	1
@ 28. 565. 700	—	1	1
Famm. 1. 13. 28. 700	2	—	2

	With ζ .	Against ζ .	Total.
W@ fam. 1	—	1	1
W@ 28	—	1	1
W@ 565	—	2	2
W famm. 1. 13	—	1	1
W 28. 565	—	1	1
W 28. 700	—	1	1
W fam. 13. 28	4	3	7
W fam. 1. 700	1	—	1
W fam. 1. 28	—	1	1
@ 565. 700	—	2	2
@ fam. 1. 28	—	1	1
Fam. 13. 565. 700	—	1	1
Famm. 1. 13. 28	1	1	2
W fam, 1	3	4	7
W fam. 13	2	8	10
W 28	1	1	2
W 565	—	1	1
@ 565	—	3	3
Fam. 1. 28	—	1	1
Fam. 13. 28	1	1	2
28. 565	1	—	1
28. 700	—	1	1
W	3	8	11
@	—	3	3
Fam. 1	—	1	1
Fam. 13	1	1	2
28	—	3	3
565	—	3	3
700	—	1	1
	—	—	—
Total agreements of group	86	92	178

The total for the whole group is therefore 178, of which 86 are in agreement with the Textus Receptus and 92 against it. The total number of variants on which these figures are based is 217, so that the disagreements of the group with the papyrus would be 39. If the 22 singular readings of P^{48} are deducted, the number is 17. If these were the only relevant facts, the preponderantly 'Caesarean' character of the papyrus would indeed be clear. Even so, the preponderance is hardly so great as Sir Frederic Kenyon's figures suggest, for the agreements with the group NABD number 143, 84 with the Textus Receptus and 59 against it. The figures for the MSS and families composing the 'Caesarean'

group, however, and for their various combinations, suggest grave doubts whether these aggregate figures have any real significance at all, and whether the evidence of the papyrus, which Sir Frederic Kenyon thinks goes 'far towards completing the disintegration of the so-called "Western" text', does not equally tend to disintegrate the W@ family. The individual agreements are as follows :

	With ζ .	Against ζ .	Total.
W	69	65	134
@	43	42	85
Fam. 1	60	35	95
Fam. 13	72	37	109
28	68	45	113
565	45	44	89
700	55	30	85

These are all higher than those for any other MS, but their differences among themselves are also striking, while the difference between the lowest of the group and the individual members of the other group is not very great. The figures in this case are :

	With ζ .	Against ζ .	Total.
N	43	33	76
A	76	6	82
B	44	37	81
D	32	29	61

The combinations are more significant still. As will be seen from the foregoing table, there are only 18 readings supported by the whole family, and 16 of these are 'Byzantine'. The figures for most of the combinations are extremely small, and it will be observed that the largest are those of W alone, W famm. 1. 13. 28, and W fam. 13, while all the more important groups contain W fam. 1 or fam. 13. Of the agreements against the Textus Receptus, moreover, the majority are also supported by other important authorities: 2 by NABD, 8 by NBD, 15 by NB, 2 by ND, 1 by AB, 2 by BD, 2 by N, 2 by B, 1 by A, and 15 by D. It might seem, therefore, on a superficial view, that these could equally well be cited as evidence of the affinities of the papyrus with the Neutral or the Western text or with Codex A. Canon Streeter, however, in the *J.T.S.* xxxvi, 178, replied very effectually to a similar objection which is involved in the Rev. R. V. G. Tasker's argument with reference to Origen's quotations from Matthew and Luke, and shewed that the fam. @ text 'in twenty-six out of twenty-eight cases exhibits a mixture of Neutral, Western, and Alexandrian readings *in accordance with the same pattern* as that in the MS used by Origen'. In spite, however, of Streeter's triumphant note and its obvious justification, it is impossible to avoid the feeling that

	With ζ .	Against ζ .	Total.
W@ fam. 1	—	1	1
W@ 28	—	1	1
W@ 565	—	2	2
W famm. 1. 13	—	1	1
W 28. 565	—	1	1
W 28. 700	—	1	1
W fam. 13. 28	4	3	7
W fam. 1. 700	1	—	1
W fam. 1. 28	—	1	1
@ 565. 700	—	2	2
@ fam. 1. 28	—	1	1
Fam. 13. 565. 700	—	1	1
Famm. 1. 13. 28	1	1	2
W fam. 1	3	4	7
W fam. 13	2	8	10
W 28	1	1	2
W 565	—	1	1
@ 565	—	3	3
Fam. 1. 28	—	1	1
Fam. 13. 28	1	1	2
28. 565	1	—	1
28. 700	—	1	1
W	3	8	11
@	—	3	3
Fam. 1	—	1	1
Fam. 13	1	1	2
28	—	3	3
565	—	3	3
700	—	1	1
	—	—	—
Total agreements of group	86	92	178

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group, however, and for their various combinations, suggest grave doubts whether these aggregate figures have any real significance at all, and whether the evidence of the papyrus, which Sir Frederic Kenyon thinks goes 'far towards completing the disintegration of the so-called "Western" text', does not equally tend to disintegrate the W^⓪ family. The individual agreements are as follows :

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there was more substance in Tasker's contention than the reply seems to leave in it. Tasker might certainly have retaliated by quoting Streeter's remarks on p. 93 of *The Four Gospels* with reference to Origen's quotations from Mark in books i-x of the *Commentary on John*, where the non-Caesarean character of the earlier Origenic text is proved by the 31 agreements with B \aleph in Mark i 1-27 against 17 agreements with fam. Θ , and Streeter adds in a footnote: 'Of these readings 16 occur in B or \aleph and the remaining 1 in D; so there are *none* distinctive of fam. Θ .' He might further have pointed out that not only does only one of the eleven readings quoted as shewing Θ or 1 in opposition differ from the Textus Receptus, but that only one of the eleven opposition readings quoted as Neutral differs from that text. It is further interesting to note that P⁴⁵ supports the Textus Receptus four times out of five in the same passages, which Streeter can hardly have noticed when he called the column 'Members of Fam. Θ in opposition' 'not merely irrelevant' but 'positively misleading'.

However this may be, it must be frankly admitted that the papyrus falls very readily into Streeter's pattern, at least in so far as it has extremely few agreements with \aleph , B, or A, and none at all with D, which are not supported by some members of the W Θ group. This does not mean, however, that the P⁴⁵ text is identical with 'the text which results from the purely objective process of deducting Byzantine readings from members of Fam. Θ '. The text resulting from that process in Prof. Lake's tables (*Harvard Theological Review*, xxi, 216-246) is so uncertain that it is necessary in many cases to assume 2, 3, 4, and sometimes even 5 possible family readings, and, as we have seen, there are only two non-Byzantine readings in P⁴⁵ which do not find the family in conflict, nor are the opposing members of the family invariably in agreement with the Byzantine text.

In addition to the non-Byzantine readings agreeing with NABD, or some or one of them, there are 9 others supported by other documents, which leaves 33 sub-singular readings, in the sense of readings supported only by members of the W Θ group. These are distributed as follows: W Θ fam. 13. 28. 565. 700: 2; W famm. 1. 13. 28. 700: 1; W Θ fam. 13. 28. 565: 1; W Θ fam. 1. 565. 700: 2; Θ famm. 1. 13. 565. 700: 1; W famm. 1. 13. 28: 1; Θ 565. 700: 1; W fam. 13. 28: 1; famm. 1. 13. 28: 1; fam. 13. 565. 700: 1; W fam. 1: 2; W fam. 13: 5; W 565: 1; Θ 565: 2; W: 7; fam. 1: 1; fam. 13: 1; 565: 1; 700: 1.

It will be seen that 7 out of the 8 readings attested by W alone, and 5 out of the 8 attested by W fam. 13 are sub-singular. It is true that many of these readings are of a kind that might easily be repeated by different scribes at different times and in different places. Even the more important of them, such as $\tau\acute{\epsilon}\kappa\tau\omicron\nu\omicron\varsigma$ \acute{o} $\nu\acute{\iota}\omicron\varsigma$ in vi 3 and $\acute{\epsilon}\nu$ $\tau\eta$

προσεύχεται αὐτοῦς in ix 2, are of the harmonistic class, while the possible addition, also harmonistic, in vi 37 is only an *ut vid. propter spatium*. The preponderance of W and fam. 13 in these variants, as in the others, is nevertheless remarkable. It is with these MSS, and in a somewhat lesser degree with fam. 1 and 28, that the affinity of the papyrus is most clear, but the new evidence only emphasizes the slenderness and fragility of the links which bind these to the other members of the alleged family.

The unequal and capricious manner in which the members of the W@ family support each other has hitherto been explained by the supposition that each member of the group has been 'heavily revised by a Byzantine reviser', or, as Prof. Lake expresses it, 'all the Caesarean witnesses have been corrupted by the influence of the late Greek text'. When one examines some of the differences, 'corrupted' seems a somewhat fantastic term. If, for example, the Caesarean reading in Mark xiv 66 is that of Eusebius, supported by 565 69 Grg DI ἐν τῇ αὐλῇ, it is impossible not to wonder whether it is @, which reads *κάτω ἐν τῇ αὐλῇ* with **N**BCLU²X, or W famm. 1. 13. 28. 700, which read *ἐν τῇ αὐλῇ κάτω* with ANY and the majority of later uncials, which have been corrupted. This view, moreover, rather leaves out of sight the evidence which some of these MSS furnish as to their own character. The notes at the end of Mark and John in 1582 and 1, and the marginal notes in 565, do not suggest a scribe 'heavily revising' an old text to agree with the Byzantine standard, but rather a critical editor in the modern sense, comparing and selecting readings from different MSS. However this may be, the agreements of the papyrus with the Textus Receptus can hardly be due to Byzantine revision, and the frequent concurrence of the 'Caesarean' documents in such readings of **P**¹⁵ suggests the possibility, which Prof. Lake himself admitted in his study of Codex 1, that they may have come by them otherwise than by revision, in other words that they are independent witnesses to Byzantine readings. This would give a new colour to their testimony but would not strengthen the case for a Caesarean text. For in fact only 16 of the 86 agreements with the Textus Receptus are attested by all seven members of the group, all the others showing the Caesarean documents in conflict.

There is, moreover, no question in this case that agreement with the Textus Receptus means agreement with the Byzantine text. All the readings with which we are concerned are supported by the whole phalanx of the later uncials, with the solitary exception of the substitution of *καί* for *δέ* in Mark v 16, which is supported by **N**ABCGKLMII &c. against DEFHLSUV &c. We are clearly, therefore, not dealing here with 'infiltrations from earlier texts', but with genuine Byzantine readings. Again, while the readings supported by A are naturally almost all

Byzantine, the proportion of Byzantine to non-Byzantine readings among the variants supported by **N**, **B**, and **D** and the individual MSS of that group is distinctly higher than in fam. **W**⊙. On the other hand, **P**⁴⁵ has no Byzantine reading which is not supported by some members of the Caesarean group, and all but 14 of such readings are supported by fam. 13, and all but 17 of these by **W** also.

The problem would be simplified if it could be assumed that the Byzantine readings of the family which are supported by **P**⁴⁵ form part of the archetypal text, while those not so supported are due to revision or corruption. This, however, would involve the further assumption that the many disagreements of members of the family with Byzantine readings of the papyrus were due to non-Byzantine corruption. This, of course, is not impossible, but the accumulation of such assumptions hardly promises a solution.

As for the text of Origen, the papyrus is extant for so few of the passages quoted by him in his Caesarean period that it is impossible to reach any conclusion. It is against him in five of the readings cited by Prof. Lake, four times in agreement with the Textus Receptus and once with **W** only. It supports him five times, once in ix 1 with Codex 1 only; once in viii 38 with **W** only; once in viii 35 with **D** 28. 700; once in viii 37 with **NBLW**Δ 28. 565, and once in viii 34, in a reading which is not only supported by all the **W**⊙ family except fam. 13, but by **C*****D** and the whole group of later uncials (except **KLII**) and (according to Tischendorf) by more than 100 minuscules. This is clearly a 'Western and Syrian' reading which the Textus Receptus has not followed. It is possible to trace in Prof. Lake's tables at least five other cases in which Origen agrees with the Byzantine witnesses, in company with some members of the **W**⊙ family and against others.

In what I have so far written I have followed Canon Streeter's definition of the Caesarean text as 'the text which results from the purely objective process of deducting Byzantine readings from the members of Fam. ⊙', which agrees with Prof. Lake's statement that 'the true Caesarean text can be reached by comparing the various manuscripts, and accepting as the family reading that which is not found in the late text'. There is good reason to believe, however, that the complete reconstruction of the text which is promised by Prof. and Mrs Lake will place the matter in a somewhat new light. There are a number of remarks in Mrs Lake's study of 'Family II and the Codex Alexandrinus' (*Studies and Documents*, v) which shew that both she and Prof. Lake are fully alive to the disintegrating effect of **P**⁴⁵. On page 4 of this study she speaks in a footnote of the importance of reserving the term Caesarean text 'for readings which have a clear right to be considered as those used by both Origen and Eusebius. In passages, therefore, where

quotations from both these writers are lacking, it is very doubtful whether, at least in Mark, any variant should be accepted as "Caesarean" which is not found either in Θ or in 565.' Some readings found in Fam. 1, Fam. 13, 28, and W, but not in Θ or 565, she suggests might perhaps be classed as 'pre-Caesarean, together with the newly discovered Pap. 45'. Again, on p. 55 she says 'The Chester Beatty papyrus suggests what might be called a pre-Caesarean text (perhaps more correctly an Egyptian text) at the beginning of the third century'. In other passages she refers more than once to this 'Old Egyptian or pre-Caesarean text'. The most remarkable passage is that on pp. 61 ff., dealing with the '106 cases where A and Fam. II agree against the reconstructed Caesarean text' in Mark i and xi. 'The most striking attestation for these readings', she says, 'is that of Fam. 1 and Fam. 13, and to a lesser extent some other manuscripts belonging to the Caesarean group.' She then gives a table showing the number of times various Caesarean manuscripts agree with A and II against the reconstructed Caesarean text, which shews that the closest relationship is to W fam. 1 and fam. 13. She points out that W fam. 1 and fam. 13 more often agree with the Chester Beatty papyrus than do 565 and Θ , and suggests that the 'Caesarean' ancestor of fam. II was perhaps 'not Caesarean in the proper sense, but pre-Caesarean'. The remarkable thing is that the great majority of the readings in question are supported by the Textus Receptus or, if not by the Textus Receptus, then certainly by the Ecclesiastical Text. Mrs Lake here disregards Canon Streeter's principle that only variants differing from the Byzantine text are 'worth quoting at all' as completely as Mr Tasker did, and she obviously does so deliberately. She had previously said, on p. 60, that 'collation with the Textus Receptus can only give a partial view of the character of any text, since, inevitably, large portions of it coincide with the Textus Receptus', and on p. 56 that 'there are more points in which a great majority of texts agree with each other than points in which they differ'. Her figures here take account of the revisions of the reconstructed text which are still unpublished. She deals with 106 variants, whereas her appended collation, based on the earlier reconstruction, published in the *Harvard Theological Review*, Oct. 1928, shews 175, which means that the new reconstructed text will contain 69 more readings agreeing with A and II, and most probably also with the Ecclesiastical text, than the old one. On the earlier basis these 69 readings would have been assigned to the pre-Caesarean text.

These statistics are limited to chapters i and xi, for which the evidence of the papyrus is wanting. In chapter vi, the other chapter covered by the reconstructed text of 1928, \mathcal{P}^b supports A, II, and the Textus Receptus against the reconstructed text 8 times, 7 times with W, and 6

times with fam. 13. There is also one 'Byzantine' reading, not supported by A but attested by the papyrus in company with \aleph BWII and fam. 1 against Θ fam. 13. 28. 565, and 700.

It is clear, then, that the elements common to all these documents are not exclusively non-Byzantine, and it is not necessary to insist on the improbability of their being derived from the text of Caesarea. Not only does Mrs Lake obviously realize this, but Prof. Lake had long ago found reasons for thinking that Origen may have known the Fam. Θ text before he left Alexandria, and even that the text which he found in Caesarea was the Neutral text. It is curious that some recent students of the Codex Sinaiticus have been calling attention again to the reasons for associating that MS with Caesarea, and it will be remembered that Streeter, in his reply to Tasker, suggested that an ancestor of \aleph might have been 'crossed' with a MS of the Caesarean text. It is not without significance that one early MS, which there are positive grounds for assigning to Alexandria, is Codex A, with its 'fundamentally Syrian text'. That W comes from Egypt there is no doubt, and the local origin of \mathfrak{P}^{45} is equally certain. Even palaeographically \mathfrak{P}^{45} offers an explanation of the handwriting of W, so different from that of the other early uncials, and suggests the possibility that this MS may be as old as any of its fellows. If so, the last word has probably not yet been said on the origin of its 'patch-work' text. Moreover, it does not seem to have been observed how few of the Neutral or Alexandrian readings in Origen's earlier quotations are without support from some of the W Θ documents, at least if we include the Georgian versions and such subsidiary members of the family as 1071 and P13416. It may well be, therefore, that the explanation of the phenomena observed by Griesbach and Streeter is not that Origen changed his text but that his text agreed more closely with \aleph and B in some parts of the Gospel than in others. This would be one more reason against associating it with Caesarea rather than with any other region. Similarly, the many elements common to \mathfrak{P}^{45} and the Byzantine text, in agreement with W, Θ and the rest, or some or one of them, or with \aleph , B, or D, reduce the amount of that text which can possibly be the result of an eclectic Antiochian recension and even suggest a doubt whether current theories as to its nature and origin furnish the best possible explanation of the indisputable fact that 'large portions' of every text 'coincide with it', whether inevitably or not. There are unquestionably more points in which the majority of texts agree than points in which they differ, and of the points of difference attested by \mathfrak{P}^{45} it seems unwise to be more dogmatic than Sir Frederic Kenyon, who is content to assert that 'throughout the second and third centuries there was in existence a considerable variety of readings which had not yet crystallized into families'.

There is a further point, however, which the new evidence emphasizes. The variety of readings was considerable viewed horizontally, but far less considerable viewed vertically. That is to say, many passages exhibit variants, but very few exhibit more than two or three variants, mostly of a trivial character. The perpetual reappearance of the same small differences in MSS of the most varied ages and provenances makes the attempt to localize them baffling, but is at the same time a testimony to the 'substantial integrity of our textual tradition', to quote Sir Frederic Kenyon once more.

All these considerations point to the conclusion that what underlies \mathfrak{P}^{45} and its supporters is not a local text but rather such a text as Von Soden imagined under the name of IHK, containing the elements of all later texts 'in their pregnant causes mixed'. No doubt actual mixture of different lines of transmission took place at all periods of the MS tradition, but it is highly probable that the fundamental cause of the 'mixed' texts of which \mathfrak{P}^{45} is our earliest example is the fact that the original text itself was 'mixed' in this sense of combining elements found in all its descendants.

C. C. TARELLI.

A NOTE ON GALATIANS II 14

THE verb ὀρθοδοεῖν occurs for the first time in Greek in this verse (οὐκ ὀρθοδοοῦσι πρὸς τὴν ἀλήθειαν τοῦ εὐαγγελίου) and is subsequently found only in authors who are consciously borrowing St Paul's phrase. It may, therefore, be worth while to draw attention to a similar usage in a recently published papyrus (*Papiri della R. Università di Milano*, published under the general editorship of A. Vogliano, no. 24; the passage is left without comment by the editor). The text in question is a private letter, dated 7th December, A.D. 1117, written to a certain Paulus by his brother (it is hardly necessary to say that there is no reason to think that the writer was a Christian); ll. 6-12 run as follows: γράφεις μ[ο]ι λέγων "ἐὰν δυνηθῆς, ἀνάπλευσον" νῆ τὴν σὴν μοι σωτηρίαν καὶ τὴν τοῦ τεκνίου μου καὶ ὀρθοδοσίαν, ἧς πέπεισμαί σε κῆδεσθαι οὐκ ἔλασσον μου, ἤθελον μηδὲν πράσσειν (l. πράσσειν) ἄλλο εἰ μὴ τὴν ὄψιν σου πρὸς τὰ ἐδάφη σου προσκυνεῖν, ἀλλ' οὐ δεδύνημαι, οὐδὲ δύναμαι. ὀρθοδοσία is not recorded in any dictionary; but I think it is clear that it does not mean 'going straight' in any moral sense, but rather 'progress' and so 'success'. The former interpretation is ruled out both by the context and by the general tone of this and other similar letters; later on the writer explains that his business affairs have been going very badly (so badly, he says, that one friend of his ἐθε[ά]σατό με κλαίοντα πλείστοις δάκρυσιν) and promises to write again to let him know how things are getting on. We