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found importance it had for Him. Schleiermacher thought there was such a thing as an impious laying claim to immortality. It may be that there is; but if we are in sympathy with Jesus, we will not agree with the extension of Schleiermacher's doctrine by a later theologian, viz., that there is such a thing as a pious resignation of immortality. It is enough for the disciple that he be as his Master.

JAMES DENNEY.

THE WESTERN TEXT OF THE GREEK  
TESTAMENT.

II.

IN dealing with the difficult *ἐπιβαλὼν ἔκλαιεν* of Mark xiv. 72, where D has *et coepit flere*, Mr. Harris suggests that the Latin is only intended to render *ἔκλαιεν*, and that this was turned back into Greek as *ἠρξάτο κλαιεν* in Δ, *ἐπιβαλῶν* being then extruded to keep up the symmetry. If this be the true explanation, it goes to show that not only the Latin versions but also the Syriac, the Theban and the Gothic have been derived from a source thus tampered with. In Acts xvii. 19 it is possible that *πυνηθόμενοι καὶ λέγοντες* may have come from *rogitantes et dicentes*, but there seems no reason to postulate a free rendering of the original *λέγοντες*, which surely needed no expansion, rather than an interpolation in the Greek. In any case we may notice (1) that the blunder *cogitantes* in D throws the supposed assimilation back a stage or two in the history of the text; (2) that an insertion just before of the words *μετα δε ημερας τινας* without any apparent motive shows that an interpolating hand has been at work on the passage. In Acts xxi. 39 Δ departs from all other MSS. by giving *συνχωρησαι* for *ἐπίτρεψον*; if this be a capricious variant, it may serve as a precedent for a good many more; there seems no reason to assume that it has come through *permitte*. The Latin

text seems too confused (*cuius rogo obsegro autem mihi*) to respond to Mr. Harris's treatment.

Luke xxii. 12 furnishes some curious phenomena, but I cannot altogether agree with Mr. Harris in the way in which he handles them. The text runs *κακεινος υμιν δεξει αναγαιον μεγα εστρωμενον*: D has *ille vobis ostendet superiorem domum stratum*; Δ *εκεινος υμειν δεξει αναγαιον οικον εστρωμενον*. That *superiorem domum* is an attempt to translate *αναγαιον* is probable enough; and *οικον* may well have come in from *domum*. But Mr. Harris goes on to argue that "the Latin translator rendered *αναγαιον* by *manianum*, a word understood in the vulgar Latin of the provinces and especially, it would seem, in Africa: this occurs in *a* as *medianum* both here and in Mark xiv. 15. There it is corrupted in *b* into *pede plano*, and as this was unintelligible by itself, in other copies *locum* was added. In *d* (our D) *medianum* is boldly corrected into *superiorem domum*." Now notice first that *manianum* is not at all a vulgar, still less an African word; it is used by Cicero, and denoted primarily certain structures in the Roman Forum. Secondly, *medianum* may be a corruption of *manianum*, as has been suggested also at a place in the Digest (ix. 3, 5, 7), where it likewise occurs; but the fact that it is found at least in three places points to its being a genuine form. Thirdly, the question whether there was a Latin translator, or more than one, is the very point at issue; if any one were asked whether *superiorem domum* came from *αναγαιον* directly, or through a *manianum* which has entirely disappeared, the answer could hardly be doubtful. In Mark xiv. 15 *αναγαιον οικον* has nothing answering to it in the Latin, whatever may be the explanation of the gap. On Acts xvi. 29 *φωτα δε αιτησας εισεπεδησεν*, Mr. Harris ingeniously suggests that the *petens* of the Latin may in some copies have given rise to *αιτων*, which then in the form of *απτων* produced the Syriac rendering "having kindled a light." His instances

of the substitution of an infinitive for *ὅτι* with the indicative, or of *ἵνα* with the subjunctive for an infinitive hardly go beyond the common variations of Greek MSS., though of course they admit of being explained by Latin influence. But in such cases, as in many others of the kind, it is not easy to see what would be the inducement to the supposed reviser. In Mark v. 17 *καὶ παρεκαλουν αυτον ινα απελθη* (Δ), it is assumed that *ινα απελθη* would have been as in other MSS. *ἀπελθεῖν*, if it had not been for the Latin *ut discederet*. But what should induce a copyist to introduce this verbal correspondence? We may notice that in the same verse Δ substitutes *παρεκαλουν* for the genuine *ἤρξαντο παρακαλεῖν*; in v. 18 where conversely it has *ἤρξατο παρακαλεῖν* where all other authorities have *παρεκαλει*, this is ascribed to the Latin rendering. Are we to suppose that in v. 18 Δ has alone escaped the Latinizing to which in v. 17 it alone fell a victim? Such a case is an unmistakable warning against overlooking the probability of capricious paraphrasing. In John xii. 25 *ἀπολλύει* is the reading of NBL 33 and is probably right, although in four parallel passages (Matt. x. 39, Mark viii. 35, Luke ix. 24, xvii. 33), *ἀπολέσει* is read without variation, and *φυλάξει* immediately follows. All other MSS. and all versions have fallen into the almost inevitable assimilation and read *ἀπολέσει*. Mr. Harris argues that they did so because they were misled by the Latin *perdet*, which they took for a future, though it was really a heteroclitite present. Do we need this assumption in view of the facts of the case, and is it a natural one to make? There is a very similar case in Matthew xvii. 15 where *καὶ κακῶς ἔχει* depends solely upon NBL (with Z doubtful); every other authority gives *καὶ κακῶς πάσχει*, and Mr. Harris assumes that they have all been corrupted by the Latin *et male patitur*. In Mark iv. 21 *μήτι ἔρχεται ὁ λύχνος*, if Δ, agreeing with some old Latin texts and some versions, gives *απτεται*, is it not more probable that this is

a correction to suit the sense [Mr. Harris says that it ought to be right] than that it is due to some confusion between *accendo* and *accedo*, which is not the most natural word for ἔρχεται. On the other hand the suggestion that in Matthew xv. 11 *κοινωνει* has come in (for *κοινοῖ*) through *communicat*, and similarly in Acts xxi. 28, is very plausible; for *communicare*, as in Tertullian's citation of this verse, does bear the meaning of "pollute," which of course *κοινωνεῖν* could not. In Mark viii. 3 a reading ἤκασιν is said to be due to the Latin which is found in  $\aleph$  A and every MS. and version but B L  $\Delta$  (Bas.) and Memph., and in viii. 13 there is almost the same authority for εἰς τὸ πλοῖον, which again is ascribed to Latinizing. I cannot see either in the nature of things or in the facts of textual criticism the slightest reason for supposing that it was more likely that *in navem* should have been added to *ascendit*, before εἰς τὸ πλοῖον was added to ἐμβάς. In Mark vi. 39 Mr. Harris speaks of the "idiomatic" *συμπόσια συμπόσια*. Would it have seemed idiomatic to the transcriber of  $\Delta$  or its parent MS., or to those for whom he wrote it; and would he have had to wait for the Latin *secundum contubernia* before he could give the natural equivalent *κατα την συνοσιαν*? In John vi. 23 there has doubtless been a misunderstanding of the original text, but not necessarily by the translator first; Tregelles takes the view (probably wrongly), which Mr. Harris ascribes to a blunder of the Latin translator, that *αλλα* is paroxytone, not oxytone. In Matthew v. 24 *προσφερεεις* for *προσφερε* seems hard to explain except as from *offeres*, a form by the way which the Clementine Vulgate has retained: yet we may suspect that *offeres* is itself corrupted from *offers*, the rendering of Am. In v. 40 we have *αφησεις*, and *dimittes* for ἄφες; in v. 42 *dat* for δός is more puzzling. Perhaps it may be a sign that the final *t* was already nearly or quite dropped. In v. 40 ὁ θελων . . . *αφησεις αυτω* (for τῷ θέλοντι) certainly looks much like a

rendering of *qui voluerit* by some one who had not looked on to the end of the sentence; but even here it is perhaps easier to suppose the alteration made by the hastiness of a careless transcriber than by any deliberate adaptation. In Matthew xiii. 48 the earlier reading ἦν ὅτε ἐπληρώθη ἀναβιβάσαντες appears in Δ as οτε δε ἐπληρωθη ανεβιβασαν αυτην: the deviation is noteworthy, but at least it is not a very close reproduction of the Latin *cum autem impleta fuerit educent eam*; if there was intentional assimilation, it is hard to see why it should have stopped where it did. In Matthew xv. 9 for πόρρω ἀπέχει Δ has πορρω εστιν, D *longe est*; unless we assume that Δ never capriciously deviates, there is nothing to determine which was the first to make the variation here. Of many more of Mr. Harris's instances in this section, I do not think it safe to say more than that Δ and D agree in a looseness of expression, which may have originated with either. Why, for instance, is it less likely that εἰρήνην ποιῆσαι came straight from εἰρήνην δοῦναι than *pacem facere*; or why in Acts xii. 15 is τυχὸν less likely to have been inserted than *forsitan*? So in Acts iii. 22 προφήτην . . . ἀναστήσει . . . ὡς ἐμέ αὐτοῦ ἀκούσασθε, if we find this corrupted into ὡς εμου αυτου ακουσεσθε, it will not occur to us that we must trace the corruption to a Latin translator first, in order to account for it. In Luke viii. 30 the right text is ὅτι εἰσῆλθεν δαιμόνια πολλὰ εἰς αὐτόν (where for a wonder Mr. Harris does not quote quite correctly), Δ has πολλα γαρ ησαν δαιμονια. Mr. Harris suggests that the Latin was *multa enim inierant demonia*, and the *enim inierant* easily became *enim erant*. This is so neat as to be irresistible; but it should be observed that the error is confined to Δ, and that it is no evidence of the wide extent of Latinizing which is postulated. Hence when in Matthew xxviii. 19 B and Δ agree in βαπτίζαντες, even if we do not attempt to defend this, we shall be slow to explain it by the influence of *baptizantes*, seeing that if any

MS. in existence has escaped Latinizing, that MS. is B. If one of the commonest of transcribers' errors needs to be specially accounted for, there is an aorist participle as well as an aorist imperative in close proximity, to which βαπτίζοντες might be conformed. In Mark viii. 36 κερδήσαι is found only in  $\aleph$  B, ἐὰν κερδήσῃ in every other authority; what probability is there that all must owe their corruption to *si lucratur*? The case is quite different with errors which are limited to  $\Delta$  and its Latin affinities. If for instance in Mark x. 16 for καὶ ἐναγκαλισάμενος αὐτὰ  $\Delta$  (and  $\Delta$  alone of Greek MSS.) gives καὶ προσκαλεσαμενος αὐτα, it is highly probable that this is due to *convocans eos*, originating in a misreading of the Greek, though of course it is not impossible that ENANKAL should have been miswritten ENKAL, and then deliberately changed to προσκαλ. In Luke v. 8, if we are to take ποσίν as a Latinism for γόνασι, it is as likely perhaps to be due to the transcriber's greater familiarity with the Latin idiom, as to assimilation to the Latin version, which by the way in all other cases retains *genua*. In Acts iii. 24 for ὅσοι ἐλάλησαν  $\Delta$  has ο ελαλησεν: this is neatly explained by assuming that ὅσοι was rendered *quodquod* (for *quotquot* as often), and this gave rise to ὄ, which naturally suggested the singular verb, another proof by the way that the reviser has not handled the present text of  $\Delta$ , for D retains *locuti sunt*. In Acts xix. 29 it is pretty clear that the original reading was ἐπλήσθη ἡ πόλις τῆς συγχύσεως, reproduced pretty faithfully in the Latin *repleta est tota civitas confusionis*. The reading of  $\Delta$  συνεχύθη ὅλη ἡ πόλις αἰσχυνης is apparently a capricious variation: αἰσχύνης may have come in as an equivalent of συγχύσεως, and perhaps the most plausible suggestion would be that συνεχύθη was a gloss written over ἐπλήσθη, intended to explain the phrase ἐπλήσθη συγχύσεως, that this came into the text, giving συνεχύθη συγχύσεως, and that the latter was replaced by αἰσχύνης, to avoid the clumsiness of the

phrase. In any case the theory of Latinizing does not help us here; and it is not clear what Mr. Harris means by "the early attestation of both the suggested primitive forms." There is absolutely no support for *συνεχύθη* except in  $\Delta$ , not even in D. Nor do I find any good reason for his statement that "evidently *αἰσχύνης* has been put in to balance *confusionem*"; if we start with the reading of all other authorities but  $\Delta$ , no difficulty arises.

It has been impossible to examine here more than a small proportion of the instances which Mr. Harris adduces, though all have been carefully verified for the preparation of this paper; and those have naturally been selected which seemed either the most convincing on the one hand, or the most open to criticism on the other. The general impression left on my own mind, and I hope that even this selection of facts may have served to give grounds for it, is that Mr. Harris has made out his case, so far as to prove the existence of this Latinizing influence in the case of  $\Delta$ , or, to be more exact, in the case of a text from which  $\Delta$  has descended, but that many of his cases are doubtful, some highly improbable, and that among the improbable ones must be accounted all those which implicate A C (and *a fortiori*  $\aleph$  B) in the same charge of Latinizing. Further, the agreement of the great majority of the Latin texts in some of the most significant errors seems to show that we may look for some common source; and thus the problem becomes that of reconstructing a primitive Latin rendering, which will be the representative of a very early Greek MS.

Although this is not the place to discuss Mr. Harris's remarks on the phonetic peculiarities of the Greek of  $\Delta$ , I cannot forbear saying that while they show much careful observation they must be received with some caution. Nothing for instance can be more improbable than his suggestion that the many Ionisms and few Dorisms, which



he thinks he can detect, afford any evidence that the MS. was written in Gaul, and to draw an argument from the assumed connexion of *Rhodanus* and *Rhodus* is really absurd.

It is more to the point to consider what is the general character of the text of  $\Delta$ , and how it acquired this. Mr. Harris thinks that he can show that the interpolations in Luke and the Acts, which are said to amount to 600 in the latter book alone, are due, at least in some cases, to Montanist influences. His first argument is a weak one. When in the *Acta Perpetuæ* the martyrs are brought by four angels to the gates of Paradise, they are received and welcomed by four other angels, who cry, "ecce sunt, ecce sunt!" This might seem a fairly obvious form of welcome. But in  $\Delta$  of Luke xiii. 29, 30, we find

*και ηξουσιν απο ανατολων και δυσμων  
και βορρα και νοτου και ανακλιθησονται  
εν τη βασιλεια του θυ και ειδου εισιν  
εσχατοι οι εσονται πρωτοι κτλ.*

It is held that this arrangement of lines shows that the cry of the angels was "an early commentary on a badly divided text," and that this text was in the hands of the church at Carthage. This seems a good deal of stress to lay upon the occurrence of such common words.

Next it is pointed out that  $\Delta$  has in Acts ii. 17, *οι υιοι αυτων* for *οι υιοι υμων*: this reading is also found in the *Acta Perpetuæ*. "Is it unreasonable to suggest," says Mr. Harris, "that the change has been made by some one who was interested to prove that the gift of prophecy had passed over from the Jewish Church to the Christian?" But is it less reasonable to suggest that the change is merely a grammatical adaptation to the preceding words? If in a missing word competition there were given *εκειω απο του πνευματος μου επι πασαν σαρκα και προφητευσουσιν*

*οἱ νιοὶ*—, I fancy that more would supply *αὐτῶν* than *ὑμῶν*. The former is found in Tertullian, as well as in Hilarius, and this shows that it was widely current in the West.

Perhaps a more fruitful source of enquiry is furnished by the nature of the glosses in the Acts. The Montanists laid great stress on the work of the Holy Spirit, and some eight of these glosses do intrude a reference to this. On the other hand, in view of the great number of the glosses to which no such character can be assigned, too much stress must not be laid on this. And it is here of great importance to observe carefully the attestation of these glosses by other authorities. Mr. Harris's contention is that the Western text of Luke and Acts is a Montanist text, earlier in date than the time of Perpetua. Now the interpolations in xv. 20, 29, are found in  $\Delta$  and in the Theban and Ethiopic versions, in v. 39 there is no trace of the interpolation in any version, nor is there of that in xvi. 4 (except in Syr. Harkl. marg.), nor of that in vi. 10. These evidently stand on a very different footing as evidence of the diffusion of the reading. The strongest cases for Mr. Harris's theory are Acts xv. 20, 29, with the repeated addition of the words "and all things that ye would not should be done unto you, do them not unto others"; and those in which *μετὰ πάσης παρησίας* is added. But they can hardly be said to be distinctively Montanist. The question as to the nature and range of this influence deserves fuller examination, but it can hardly be said to be decided as yet.

But further, Mr. Harris claims that as he has shown Latinization of the Greek text at work, the Greek text can have no certain value, except where it differs from its own Latin, and must no longer be regarded as an independent authority. Here I fail to follow his argument. Admitted that there are unmistakable traces that at some stage in the history of the tradition, the Greek text was here and

there adapted to the Latin, there is no evidence whatever to show that this was done either systematically or completely. It has always been recognised by all the world (but Bornemann) that  $\Delta$  contained many serious depravations of the text; no one has accepted its testimony unless strongly supported; if we can discover the origin of the depravations, that does not give them more claim to our consideration, and they could hardly have less. Indeed the agreement of  $\Delta$  and D may be taken just as well as evidence of their original source as of harmonizing; and other considerations must be brought in to decide in each individual case.

Another line of evidence is drawn from the Latin translation of Irenæus. This confessedly agrees in some remarkable readings with D. Dr. Hort held this to be due to the fact that the translation of Irenæus was made in the fourth century, and that the Latin version which appears in D was familiar to him, so that he naturally adopted its language in translating the quotations which Irenæus had made in Greek. Mr. Harris's contention is (1) that some of the interpolations now found in quotations by Irenæus, though only preserved in the Latin version, belong to the Greek original; (2) but that they are due to the influence of a Latin version; (3) that therefore this version must have been made long enough before the time of Irenæus for its influence to affect the Greek text which that father used. There are three passages where *μετὰ παρρησίας* or *μετὰ πάσης παρρησίας* seems to have been inserted; one of these (Acts ix. 10) happens to be quoted by Irenæus with the suspected words. Now if this phrase were exclusively used in interpolated passages, this would be a very strong argument. But there are at least four other places where it is undoubtedly genuine; and it is not at all improbable that Irenæus used the words here carelessly by a slip of memory. It is unfortunately not certain, owing to a defect in the MS.,

that they appeared here at all in  $\Delta$ . But if they did, were they due to Latin influence? *i.e.* were they inserted first in the Latin version, and from this transferred to the Greek text which Irenæus used? The only reason for thinking so is that they seem to belong to a group which has a Montanist colouring. But nothing prevents a verse from being interpolated twice over at different stages. Another case quoted by Mr. Harris seems rather to tell against him. To Acts xv. 29  $\Delta$  adds *φερομενοι εν τω αγιω πνευματι*, *D ferentes in santo sp̄o*. Irenæus has *ambulantes in spiritu sancto*. Does this look like an independent translation from the Greek or a borrowing from the Latin? Of course, it may be contended that any interpolated reference to the Holy Spirit must be Montanist in origin, and that a Montanist interpolation must have been made in Latin; but to do so is begging the question. Similarly in Acts iii. 17,  $\Delta$  has *κατα αγνοιαν επραξατε πονηρον*, *D per ignorantiam egistis iniquitatem*, Irenæus *secundum ignorantiam egistis nequam*; it is easy to say that the primitive form in *D* was doubtless *nequam*. If so, why and how should it have been changed? The argument here is hard to follow. If the Latin translator of Irenæus was guided in translating the quotations by his knowledge of the version which we have in *D*, why does he often depart from it? If he was only translating from a Greek text, which had been assimilated to such a version, why should we assume that he would always hit upon the precise word which had originally been used? *e.g.* if *πονηρον* came from *D*, where we now find *iniquitatem*, and is rendered by *nequam*, why assume that *nequam* originally stood in *D*? The evidence that Tertullian used the Latin translation of Irenæus is very slight; and is not much strengthened by the contention that there is a "fair possibility" that Cyprian used it. So far Mr. Harris's statement that "the Greek of the Beza text owes the *greater part* of its textual and grammatical

peculiarities to the reflex action of its own Latin" (p. 171) seems to have been inadequately supported.

He next takes up the question of the relation of the Harmony of Tatian to  $\Delta$ , and adduces seven instances in which Tatian agrees with the Latinized text. If these stand examination, the conclusion is a most important one, as to the date and the distribution of this type of readings. It will follow that the Latin version must have been made long enough before the time of Tatian for it to have affected the Greek text, and that this text must have been sufficiently widely distributed to make it natural that Tatian should use it as his basis. Now mere coincidence in a reading is not enough to establish connexion, unless there is something striking about the reading. *E.g.* in John xiv. 9  $\tauοσοῦτον \chiρόνον μεθ' \acute{\upsilon}μῶν εἰμί, καὶ οὐκ ἔγνωκός με, Φίλιππε;$  the wonder is not that several Latin authorities have *cognovistis*, but that any have escaped the attraction of the plural. In John xiii. 14 the [ $\acute{\rho}\acute{o}\sigma\phi \acute{\mu}\acute{\alpha}\lambda\lambda\omicron\nu$ ]  $\kappaαὶ \acute{\upsilon}μῆῖς ὀφείλετε ἀλλήλων νίπτειν τοὺς πόδας$  is not more likely to have come in from a translator than to be introduced by a transcriber. In Mark i. 33 (for which, by the way, there is almost the sole misprint in Mr. Harris's numerous references) the multitude was gathered together  $\piρὸς τὴν θυρὰν αὐτοῦ$ , the last word was such a natural addition that Tatian's *ad ianuam Jesu* does not of necessity imply connexion. Nor does the reading in Luke v. 8. If a Latin translator could render  $\acute{\epsilon}\xiελθε$  by *rogo exi*, as Mr. Harris assumes him to have done, one does not see why a Syriac renderer should not have given the equivalent of *peto a te ut a me recedas*, especially as we have only a Latin version of an Arabic translation of the original rendering. In John xvi. 21, for  $\acute{\eta} \acute{\omega}\rhoα αὐτῆς \Delta$  has  $\eta \etaμερα αυτης$ , a very natural variation; it is worth noticing, however, that none of the versions show it, except the Peshitto; Tatian renders *adventus diei partus eius*. May not this have been a quite independent expla-

natory paraphrase by the translator? In Mark ix. 15 *προστρέχοντες* has got corrupted in  $\Delta$  into *προσχεροντες* (for *προσχειροντες*), a reading followed by several of the old Latin versions. It is quite clear that this corruption did not begin in Latin, but in some Greek ancestor of  $\Delta$ . If therefore Tatian's version is represented by *prae gaudio properantes*, this does not suggest the use of any Latinized text, but merely of one into which this error had crept. Mr. Harris says that "no other Greek traces of the reading are forthcoming than those in  $\Delta$ "; but as D has *gaudentes*, it is clear on his own theory that it did not arise in  $\Delta$ , and it is highly probable that other copies besides  $\Delta$  were taken from the text in which it did originate. In Luke xxi. 25, *καὶ ἐπὶ τῆς γῆς συνοχὴ ἐθνῶν*, D has *et super terram conflictio gentium*; other versions have different renderings, *compressio*, *occursus*, *pressura* (Vulg.); the Syriac (Cur. and Pesh.), have the equivalent of *complosio manuum gentium*. The version of Tatian has the conflate rendering *pressura gentium et frictio manuum*, which cannot be original. Mr. Harris is probably right in saying that *frictio manuum gentium* is the correct reading as supported by the Syriac. But is it so certain that *frictio manuum* must have come not from *συνοχή*, but from *conflictio*? In Luke xxiv. 29, *ὅτι πρὸς ἑσπέραν ἐστὶ καὶ κέκλικεν ἡ ἡμέρα*,  $\Delta$  omits *ἐστὶ καὶ* before *κέκλικεν*, written *καικλεικεν*. This is clearly a mistake which must have arisen in a Greek MS.; it is shared by the old Latin texts and the Peshitto, a strong proof that they used a text agreeing here with  $\Delta$ . But to say that this error could only have originated in a bilingual, because no trace of it is found in any Greek MS. but  $\Delta$ , seems to go far beyond what the evidence requires. There seems no reason why it should not have arisen in an ancestor of  $\Delta$ , as yet unaffected by a Latin version. Mr. Harris justly says that one instance will prove his case, but then that must be a demonstrative and irrefragable instance; and at most we

have possibilities which can be converted into probabilities only by evidence from other sources.

A further argument Mr. Harris draws from the nature of the glosses, trying to show that even when not Montanistic in character, they are obviously of Latin origin. For instance, in Acts iii. (misprinted ii.) 13 there is added in  $\Delta$  *εις κρισιν*, in *E* *εις κριτηριον*; but in *D* *in iudicio*, in *e* and in *Irenæus* as *in iudicium*, the variation in the Greek seeming to indicate an altered rendering of the same Latin gloss. But let us try to realize the process through which the scribe of *E* passed, according to this theory. Cod. *E* is "probably a direct descendant of Cod. Bezaë" (Harris); he thereupon had before him in  $\Delta$  *εις κρισιν*, in *D* *in iudicio*; this gloss had come in *at an earlier stage* in the Greek, from a still earlier insertion in the Latin. All trace of its being a gloss had therefore long been obliterated. Yet the scribe of *E* scents out its nature instinctively, and therefore feels at liberty to attempt another rendering of the original Latin! In precisely the same way he is supposed to have discovered that in Acts v. 38, although he has before him in  $\Delta$  *μη μιαναντες τας χειρας*, and in *D* *non coinquinatas manus*, these are but parts of a Montanist gloss, that the Latin was the earlier, and that therefore he is at liberty to attempt another rendering of it, by changing *μιαναντες* into *μολυναντες*, besides correcting the obvious error *coinquinatas* into *coinquinantes*.

On Acts xii. 10 Mr. Harris has an ingenious theory—they are all astonishingly and delightfully ingenious—to account for a puzzling gloss.  $\Delta$  has (of Peter and the angel) *και εξελθοντες κατεβησαν τους ζ βαθμους και προσηλθαν ρυμαν μιαν*, *D* *et cum exissent descenderunt septem grados et processerunt gradum unum*: the true text is *και εξελθόντες προήλθον ρύμην μίαν*. Where do these "seven steps" come from? Mr. Harris promptly tells us, from a Latin version of Homer, the glossete remembering how Poseidon came down from

the mountains of Thrace in three strides, reaching his goal with the fourth, and then turning three into seven for metrical reasons. The ἄγγελος κυρίου is supposed to have suggested Hermes, and Hermes to have recalled (heaven knows why!) the descent of Poseidon. A confirmation of this theory is sought in the fact that the glossete, thinking of the rod of Hermes, makes the angel thrust at Peter with a wand (νύξας), and not strike him with his hand (πατάξας), as in earlier authorities. Mr. Harris forgets II. xvi. 704, χείρεσσ' ἀθανάτησι φαινήν ἄσπιδα νύσσων, which shows that immortals were able to thrust with their hands. But where is the evidence for the Latinizing? Apparently only in the fact that there has been some slight probability that elsewhere a Latin Homeric cento has been employed. Here the Homerizing is much less evident. And if the Greek comes from the Latin, why τους ζ βαθμους, and whence the genuine ρυμην? Another point drawn from the κατέβη is as ingenious, but not less dubious. The gloss-writer "must have been in some city where people went up when they were committed to prison, and came down when they recovered their freedom." This was the case at Carthage, where the prison was on the Byrsa, hundreds of feet above the town. Hence perhaps the glossete was a Carthaginian. Yes, but it was also the case in almost every town which had an acropolis. It was the case at Jerusalem, whatever view we may take as to the exact spot of Peter's imprisonment. It was so at Rome, where a prisoner would certainly come *down* to the Forum. No weight can, I think, be attached to this.

The hypothesis of an African origin for these glosses is buttressed by cases of assumed African idiom, but these are not convincing. The instances quoted as accusatives absolute may be explained for the most part as mere negligences. In Acts xv. 11 *sileuitque* for ἐσιγήσεν is indicative of some omitted verb, which would have governed *desponentes pres-*



*byteros* : in xx. 12 *salutantes*—*adduxerunt*, where the Greek has a singular verb, certainly looks as if a nominative was intended ; in xiv. 19 the accusatives may well be governed by *supervenerunt*. Acts iii. 17 is perhaps doubtful, but the Latin seems to me the most natural and exact rendering of the Greek ; and so with Acts xvi. 37. The only plausible accusative absolute is in Acts v. 38, where *coinquinatas manus* may fairly be taken as a mere slip for *coinquinantes manus* ; and unless the Latin is older than the Greek, the point to be proved, it certainly is. In Matthew xvii. 19 we have, according to Mr. Harris, *descendentes* equivalent to *καταβαιόντων αὐτῶν*, but we look in vain in Africa or anywhere else for a parallel to such an accusative absolute of one term only. In Mark xi. 12 we find *ἐξελθόντα* (for *ἐξελθόντων αὐτῶν*), where the Latin is *cum exissent*. I do not know whether Mr. Harris would say that this answers to *exeuntem*, which had been intruded into the Latin, had displaced the original Greek, and had afterwards been corrected itself, still leaving the corruption in the Greek. I think it simpler to regard it as an assimilation by a nodding copyist to the preceding *ἐπαύριον*. But it is curious to notice how wide the traces of the blunder are.

The evidence for the *tumor Africanus* is not convincing. In Acts vii. 5 *possessionem hereditatis* is not an unnatural rendering of *κληρονομίαν*. In Acts vii. 46 the absurd reading of **N B H**, as well as **Δ** *ἤτήσαντο εὐρεῖν σκήνωμα τῷ οἴκῳ* (for τῷ Θεῷ) is explained by the fact that *οἶκον* occurs in the next line, and that the combination *οἶκος Ἰακώβ* (or rather *Ἰσραήλ*) was so common as to suggest itself at once. It is then a very simple case of parablepsy. Mr. Harris suggests that *σκήνωμα* may have been translated by *tabernaculum* and also by *sedes domui* (a very unlikely rendering), and that the latter may have given rise not only in **Δ**, but also in **N B**, to *οἴκῳ*. Other examples are more plausible, but none seem to go beyond the natural limits of a slightly periphrastic ren-

dering. In Mark vi. 43 does Mr. Harris think that the original reading was κλάσματα, that this was translated *reliquias fragmentorum*, that *reliquias* was then omitted, and κλάσματα changed into κλασμάτων to suit *fragmentorum*? So it would seem; but I suppose he would accept the πληρώματα of B and Δ, etc., as genuine; and this gives strong support to κλασμάτων (Ti., Tr.) as sound, though undoubtedly the text is hard to settle. In Luke xiii. 8 βάλω κόπρια appears in Δ as βαλω κοφινον κοπριων, while D has *mittam qualum stercoris*. Why should D here alone translate κοφινον by *qualum*? Mr. Harris very cleverly suggests that κόπρια was rendered by *squalem stercoris*, and that this early became corrupted into *qualum stercoris*, whence κοφινον κοπριων in Δ. But let us again try to realise the process. An early Greek text has κόπρια; this is rendered into Latin by a "tumid African" by *squalem stercoris*; and his version is set side by side with a Greek text, which is subsequently assimilated to it. We find no trace in the Greek of the original paraphrase, but at some later time, when *squalem* has become accidentally corrupted into *qualum*, then it causes the Greek to follow suit, and changes κόπρια into κοφινον κοπριων. Does this sound probable? That *qualum* was a natural translation of κόφινον is proved by Prudentius (Cath. ix. 60), who uses *qualus* in a reference to the feeding of the five thousand. There can be no objection to supposing that the paraphrase in the Greek is the earlier, except the assumption, by no means proved, that all such are of Latin origin. Mr. Harris does not fail to see that his theory of the origin of these glosses at Carthage requires us to assign them to a very early date in order to account for the wide diffusion of the text so expanded, which can only be accounted for if it was accepted at Rome. But he does not deal with the question why a text originating in Carthage should have been so accepted. There must have been by the middle of the second century a Latin version of the

Gospels current at Rome. How should this have been displaced by one of external origin? It is not sufficient to say that the Montanist glosses gave it currency, when Montanism was in the ascendant there. They are certainly not characteristic or obtrusive enough to warrant such a notion.

There is no part of his work in which Mr. Harris's method is more admirable than where he handles the glosses in the Western text of the Acts. He puts together 190 of the more important—there are more than 600 in all—and examines their character, with a view to provisional classification. He finds that C attests only four of them (the reference to Acts xv. 4 is an error, for the gloss occurs only in C<sup>3</sup>), and these show no marked Latinism. Hence he rightly concludes that these Western elements in C are antecedent to the Latin rendering, a point to be carefully borne in mind. Also of the glosses which seem to have a decided Montanist colouring, none appear in C or in the Syriac Peshitto. The argument which is drawn from Acts xii. 7, as to the date of the primitive Syriac, seems very precarious. But the Theban version is unquestionably after the Montanist glosses.

In dealing with the Gospels Mr. Harris finds no definite traces of Montanism, except the striking reading in Luke xi. 2, *αγιασθητω ονομα σου εφ ημας*, where the last two words may perhaps preserve a trace of the curious variant *ἐλθέτω τὸ ἅγιον πνεῦμα ἐφ' ἡμᾶς*. But he thinks that there are definite traces of Marcionite influence. The reading *ουδεις εγνω τον πατερα* (for *ἐπιγινώσκει*), common in early Fathers, may be due to a retranslation of *novit*. That *pater* is added in D at Luke xviii. 19, *nemo bonus nisi unus d's pater*, may be due to Marcionite tendencies; but it is to be noted that Origen uses the word without hesitation. Mr. Harris rather startles us by assuming that in Matthew xix. 16 *τί με ἐρωτᾷς περὶ τοῦ ἀγαθοῦ*, is a Gnostic depravation, derived from a Western bilinguist. If it was a deliberate corruption, how did the parallel passages in Mark and Luke escape?

And where are we to look for an uncorrupted tradition if  $\aleph$  B L fail us? Again, in Luke xxiii. 2 a Marcionite gloss, *καὶ καταλύοντα τὸν νόμον καὶ τοὺς προφήτας* was added, according to Epiphanius: it is found in several MSS. of the Old Latin versions, including one or two of the Vulgate; but there is not a trace of it in any Greek text. If, as Mr. Harris supposes, it stood in the ancient Roman bilinguals, why has it disappeared so completely even from MSS. so much influenced by these? Similarly, with the further addition, *καὶ ἀποστρέφοντα τὰς γυναῖκας καὶ τὰ τέκνα*, to which in *c* and *e* there is added *non enim baptizantur sicut et nos nec se mundant*. There is no doubt as to the Marcionite character of the gloss, the Gnostics wishing to represent the same charges as being brought against Christ which were directed against themselves. But again, why suppose that they ever stood in a Greek text which gives no trace of them? Surely it is possible to believe that one or two codices are interpolated without holding that this corruption ever extended to the Western text generally. To say that the primitive Western bilingual was Marcionized is to go beyond the evidence, except on the hypothesis that no Marcionite interpolations could creep into any of its descendants at a later date. The omission of *æternum* in *g*<sup>2</sup>. at Luke x. 25 is proof that the excisions of Marcion did leave traces in Latin versions, not that the general tradition was corrupted thereby.

Mr. Harris recognises the tentative character of much of his reconstruction of the history of the Western text, and offers suggestions as to the way in which the problem must be worked out. His remarks on the *κῶλα* of  $\Delta$  and D support, though they go but little way to demonstrate, the antiquity of the Latin version. Some curious cases of confusion between the abbreviations *dī* and *dñi* lead him to express himself definitely in favour of *ἐκκλησίαν τοῦ Θεοῦ* in Acts xx. 28, though against A and C, as well as D and E.

Finally, Mr. Harris closes his discussion with calling attention to a few isolated phenomena which seem to confirm his theory, and to special questions which still need investigation.

At the first reading it is almost impossible to resist the impression left by the learning, the ingenuity, the familiar acquaintance with the critical material, and the fresh insight into early Christian literature, which mark this noteworthy treatise. But the repeated study which it claims and repays leaves an uneasy feeling of an imposing edifice resting on weak and scanty foundations. The evidence, it is true, is cumulative, and great injustice has been done to the force of it by selecting, as has been imperative in this paper, only portions of it for examination. But many threads do not make a strong stay, if each has to stand separately a strain too great for its resisting powers. It is deeply to be regretted that the great master of textual criticism, whom we have lost in Dr. Hort, was not able, so far as I have learnt, to give his judgment on a theory which cut straight across some of his favourite notions, but which his candour would have led him to be the first to accept, if he had regarded it as established. Perhaps there is no one left whose verdict will weigh so heavily. The present paper is in no sense intended as a verdict. It is rather a plea in arrest of judgment until some of the difficulties have been removed which hang about an attractive theory, supported with conspicuous ability, but not yet, I venture to think, raised above the level of a possible hypothesis.

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