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I.

THE MESSIANIC KINGDOM.

THE Greek term βασιλεία signifies *dominion, rule, reign*, the exercise of kingly power. It signifies also a *kingdom, dominion, realm*, a people and country under kingly rule. The Greek word answers to the Latin *regnum*, which is also equally adapted to express the English terms, *reign* and *kingdom*. The first relates to the time or duration of the sovereignty; the second, to the place or country over which it extends. Though it sometimes signifies, in the Gospels, *reign*, yet it is always translated *kingdom*.

Three forms of expression are employed in the New Testament to designate the reign and spiritual kingdom of the Messiah. These forms of expression are ἡ βασιλεία τῶν οὐρανῶν, ἡ βασιλεία τοῦ Θεοῦ, ἡ βασιλεία τοῦ χριστοῦ. All these forms of expression are considered to be synonymous. They all signify the kingdom of the Messiah.

The idea of the kingdom of God has its origin in the Old Testament, in which the coming of the Messiah and His triumphs are foretold. We cannot, therefore, adequately understand its import, unless we first trace its historical development in the Old Testament Scriptures.

The book of Genesis begins with the announcement that "In the beginning God created the heaven and the earth" (i. 1). It states that He made man in His image, and gave him dominion over all the earth (v. 26); and Abram calls God "the Lord, the most high God, the possessor of heaven and earth" (xiv. 22). God, by the act of creation, is possessor of all things and universal sovereign. Moses in his sublime song (Ex. xv. 18), says, "the Lord shall reign for ever and ever."

VI.

THE GREEK TESTAMENT OF WESTCOTT AND HORT.*

AFTER twenty-eight years of preparation, the text of Drs. Westcott and Hort's Greek Testament was at length given to the world in May last, followed in September by an Introduction discussing the principles of text criticism, and an Appendix comprising, among other important matter, a much needed series of notes on select readings. Long expected as it was, the reception which the published work has met with has been unprecedented among books of its class. It has not merely been greeted by critical journals, but it has been extravagantly lauded and extravagantly condemned by publications of purely popular character. So that, thus, a work which ordinarily would have passed silently to the shelves of specialists, has sprung suddenly into the notice of the general reader, and has, in this new sphere, made parties and raised wordy strife on subjects hitherto alien to its whole thought. This remarkable reception is due partly, doubtless, to the accident of the time of its appearance—the Text, just when men were looking eagerly for the publication of the Revised English New Testament,† and their minds were full of the textual problems necessarily brought before them in connection with that work, and the Introduction, just when the disputes concerning those problems and the proper methods of solving them were at white heat. It is due also partly, doubtless, to the excellent advertising which, prior to the publication, was given to the forthcoming work. Nearly every English writer on the subject has,

* THE NEW TESTAMENT IN THE ORIGINAL GREEK, the text revised by BROOKE FOSS WESTCOTT, D.D., and FENTON JOHN ANTHONY HORT, D.D. [Vol. I.] TEXT, [Vol. II.] INTRODUCTION, APPENDIX. Cambridge and London: Macmillan & Co., 1881. In the American edition (New York: Harper & Bros., 1881 and 1882) an "Introduction to the American edition, by Philip Schaff, D.D., LL.D." (pp. v.—lxxxvii.), is prefixed to the first volume.

† It was published five days before the Revision.

for a term of years, pointed to its coming as a boon in store for us; so that men's minds have been on a stretch with expectations which they were eager to see fulfilled. It is undeniable, however, that it is also due partly to the character of the text which has been found on publication to be contained in and defended by the new volumes. Naturally enough it has been looked upon as a gage thrown down in defence of the main principles adopted by the Revision Committee; and, naturally enough, it has, therefore, only poured oil on the already blazing controversy, and has called forth praise or condemnation according as it fell in with previously held principles or rubbed already abraded sores of prejudice. Thus, for instance, on the one hand, the *Quarterly Review*,* "with regret records its conviction that these accomplished scholars have succeeded in producing a text vastly more remote from the inspired autographs of the Evangelists than any which has appeared since the invention of printing"; while, on the other, the *Church Quarterly Review* thinks that "all students of the New Testament must hail with delight the appearance" of a text which, having been framed "with a splendid patience, which is at once an example and an encouragement to younger scholars," presents "the New Testament in the form most approaching the original autographs which is accessible." † Other journals range themselves on one or the other of these sides with more or less enthusiasm.

It is, therefore, clearly worth our while to turn aside for an hour from more attractive subjects to ask after the truth here, and seek to know just what the principles expressed by Dr. Hort ‡ are, and just what kind of text has been formed from them. It may affect the expectations with which we enter on this inquiry to know that, among previous inquirers, the opinions of those of critical judgment are pretty much all one way; § but this cannot exonerate us from

* October, 1881, p. 391 (supposed to be by Dean Burgon).

† July, 1881, pp. 514 and 519.

‡ The Introduction, though expressing the common views and conclusions of the editors, is yet from the pen of Dr. Hort.

§ Dr. Schaff (Introduction to American edition, p. viii.) thinks that this work presents a more ancient and purer text than any other edition. Dr. Ezra Abbot believes (*Sunday School Times*, Nov. 5, 1881) that it will mark an epoch in the history of New Testament criticism. Dr. William Sanday (*The Expositor*, October, November, December, 1881, and *Contemporary Review*, December, 1881) enthusiastically advocates it. Dr. William Milligan (*Catholic Presbyterian*, September, 1881) plainly likes it. From Germany we have seen but two brief statements: one from Hilgenfeld, who merely mentions it as a "noteworthy edition" (*Zeitsch. für Wissenschaftl. Theologie*, 25, II., p. 212), and the other from Dr. Von Gebhardt (*Novum Test. Grace*, etc., Tauchnitz, 1881, pp. vi. and vii.) who believes that the new edition "novum certo et inexpectatum his studiis emolumentum afferet," and "omnibus quotquot adhuc publicatæ sunt editionibus eo præstat quod ad testimonia in diversas quasi classes describenda et

the task, but rather renders it the more incumbent that the investigation shall be careful and the exposition clear.

THE HISTORY OF THE PRINTED TEXT.

Before, however, we enter upon this our proper task, it will be well to take a general review of the history of the printed text of the New Testament in order that we may see clearly just where the new editors take up the task,—with what basis of established fact behind them and with what unsettled problems before them. The printed text of any work which has been previously propagated for a considerable period in manuscript usually passes through three stages: an *editio princeps* is published,—then, some one edition acquires a circulation and acceptance which gives it the position and authority of a “*received text*,”—and then, critical editions are framed and published in the effort to amend the received text into nearer conformity with the autographs. This is the legitimate course of history. For, the first edition is naturally printed from whatever MSS. lie nearest at hand; and a text becomes the received text usually not from any peculiar purity that belongs to it, but from some commending external quality,—such as the beauty of its presswork or the convenience of its form,—which wins it popular favor. In a much-read work, this stage is, naturally, reached early in its printed history, before any important critical amendment has been undergone. Hence, as knowledge is acquired of older and better MSS. than those which accidentally fell into the way of the editor of the first edition, it becomes necessary to prepare critical editions. There must, therefore, result a striking peculiarity of procedure in the preparation of a pure edition of such a text, as distinguished from that of a work which was first published in a printed form: in the latter case the first edition is commonly the standard to which all others (*reprints*, therefore,) should conform,—in the other, as the representative, ordinarily of the latest and therefore presumptively the most corrupt MSS., it is the standard of that from which subsequent editions should diverge. This is peculiarly true of a work which has been very popular during a long period of existence in MS. and has lost none of its popularity by being put through the press. The one circumstance secures the rapid multiplication of MSS. and consequently rapidly growing corruption; the other, the early formation of favorite passing into received texts, fixing the early corruption.

acute dijudicanda certa cum ratione et tanta prolixitate quanta antehac a nemine, ibi adhibita est textus historia.” Journals for 1882 were received too late for mention here.

It is not strange, therefore, that just such a history has been wrought out by the text of the New Testament. Its *editio princeps* (Erasmus, 1516), hurried through the press at break-neck speed in the effort to forestall a rival edition (the "Complutensian Polyglot") known to be already printed and ready for distribution, was simply a printer's speculation and was taken from almost contemporary and utterly unsatisfactory MSS. without attempt at critical revision. It was doubtless only a printer's device that it bore on its fore-front, its boastful title-page; its editor was certainly free to confess in private that it was "precipitatum verius quam editum." Yet it was this text that was, without important alteration, gradually hardened into the Received Text, through the magnificence of Stephens' "*Editio Regia*" (1550) and the convenience of the small Elzevirs (1624-33). Though it reigned, therefore, as by prescriptive right for centuries, it is clear that the circumstances of its formation can lend it no authority; and even were we to frame, as our final text, one practically the same, it would necessarily be "non propter Receptum sed cum Recepto." After it had been once established, however, as the Received Text, men were a long time in learning this. Although preparations for critical editions began as early as 1657 (Walton's Polyglot), yet the bondage of the Recepta was not completely shaken off until the appearance of Lachmann's New Testament in 1831. The history from 1657, therefore, falls naturally into two periods: that of bondage to and that of emancipation from the Recepta, divided at 1831. Lachmann thus marks an epoch, and criticism owes him a debt which can be scarcely estimated, as the bold spirit who at last actually made the step so long prepared, of shaking off the shackles which so clogged it as to render a really critical edition impossible. The result of this step was to introduce the age of editions founded no longer on traditionary but rather on critical principles, so that, varying the phraseology, we may say that 1831 separates the periods of preparation for, and of publication of, critical editions. The text which Lachmann actually published, however, was unsatisfactory: it was intended by him as preliminary to further criticism, and the material for framing a satisfactory text was not yet in the hands of scholars. So then we may say with equal truth that the preparation for criticism really continued until the days of Tischendorf and Tregelles. And there is obvious propriety as well as convenience in considering the later editions of Tischendorf and the one great edition of Tregelles as marking the first issue of really critical editions,—and even in remembering that these (as combining with the text much valuable new

matter in prolegomena and digests) were preparations for future criticism as truly as critical editions themselves.

In this long-continued preparation was included the pressing of three separate lines of labor, issuing in: 1. The collection of documentary evidence for the text; 2. The classification of this increasing material; and 3. The formation of critical rules for the application of the evidence in the final reconstruction of the text. It is clear that no text at all worthy of the name of critical could be formed until the mass of evidence was collected; and just as clear that the value of the text actually framed would depend on the soundness of the work done in the other lines.

I. The work of collecting the material, heralded by Stephens and Beza, began in earnest with Walton's Polyglot (1657). The great names in this work are such as Archbishop Usher, John Fell, John Mill, in whose hands the collected various readings already amounted to 30,000, Bentley and his employés, Wetstein who made nearly as great an advance on Mill as he had done on his predecessors, especially in the matter of detailed accuracy and completeness, Matthæi, Alter, Birch and his compeers, Griesbach, Scholz, Tischendorf whose editions of MSS. "exceeded in number all that had been put forth before him," Tregelles and Scrivener. Until Tischendorf's labors were undertaken, from insufficient knowledge of material alone satisfactory editions of the Greek Testament were impossible. Now, however, we have, accessible to all, accurate editions or collations of a great number of documents, including all of great age that are known, and a sufficient number of all ages to furnish material for blocking out with accuracy the history of the text. The exceeding modernness of our accurate knowledge of the contents of even the most essential documents seems to be hardly realized by scholars at large; it is made plain to the eye by a table given at the end of § 18 of Dr. Hort's Introduction. Let us only remember that \aleph was not published until 1862, and B not adequately until 1868, while the present satisfactory editions of C. Q. D. D₂. N. P. R. Z. L. Ξ . E₂. P₂. have all been issued since 1843. One sixth century MS. of Matthew and Mark— Σ —was only discovered in 1879*; and thirty-four leaves (palimpsest) of an eighth or ninth century MS. of the Gospel were brought to light in 1881.† So that we do not even yet know all that may be in hiding for us. But we have at least reached this position: now, for the first time, we can feel sure that we have a sufficient body of evidence of all

* In Southern Italy, by Harnack and Von Gebhardt.

† In Great Britain, by Profs. Mahaffy and Abbott.

kinds before us to render possible the sketching of the history of the written text in a somewhat close and accurate manner, and to certify us that new discoveries can but enlighten dark places and not overturn the whole fabric.

2. It was inevitable in the first and earliest stage of the science, that all documents containing evidence for the text should be treated as of practically equal value. We can hardly blame Erasmus, that he set aside the readings of the only good MS. he possessed, because it differed from the rest. Nor is it difficult to understand why Stephens' collations rather ornamented his margin than emended his text; nor why the earlier editors printed the usual text unchanged, and relegated their MS. readings and their infirm conclusions from them alike to the Appendix or Prolegomena. By Mill's time, however, the mass of material was already too great to be manageable when treated in separate units, like a pile of sand; and his study of it was too intense and his mental vision too acute for him to fail to see signs of agglutination in the particles. Bentley seized these hints, and drawing a broad line between the old and the recent copies, proposed to set forth an edition framed out of the agreement between the ancient MSS. of the Greek original and those of the Latin Vulgate. The really telling work in this department was not, however, to be done on English soil. John Albert Bengel was the first who, with zeal and earnestness, set himself to the classification of documents according to their text-affinities. He saw clearly that if they could be arranged in affiliated classes, the science of textual criticism would be greatly simplified: the individual variations of document from document within the bounds of the same class would be convicted of an origin later than that of the class itself, and the class variations of family from family would alone deserve consideration. Thus a large number of variations would be eliminated at the outset, and the determination of the text be made comparatively easy. With no less of acumen than of patience, Bengel attacked his task. Collecting all the various readings of each document, he compared each of these lists with all the others, and thus sought to discover its relations, and so laboriously to construct his families. The result was to follow Bentley in drawing a broad line of demarcation between the ancient and the more modern copies under the names of the African and the Asiatic families, and to make the new step of dividing, in a more shadowy manner, the African family itself into two, represented respectively by A (which was practically the only purely Greek uncial at that time known) and the Old Latin version. In his opinion

also, as in Bentley's, the African class was of supreme value; and it was a critical rule with him that no Asiatic reading was likely to be genuine unless supported by some African document. Semler followed Bengel, and handed down his classification to Griesbach, who tested and modified it into harmony with the advancing knowledge of documents, and handed it on, commended anew by his genius and scholarship. According to their text Griesbach found the documents of the Gospels to fall into three classes, the first two of which, no matter when the documents themselves were written, presented a text which was at least as old as the third century, but the third of which contained a text not older than the fourth or fifth. He called these classes: 1. The Alexandrian, represented by B* C L, 1,33,69, Memphitic, etc. 2. The Western, represented by the Græco-Latin codices, the old Latin, etc.; and 3. The Constantinopolitan, represented by A E F G H S, cursives, etc. A somewhat different distribution of documents was necessary for the other portions of the New Testament; thus A rose to the more ancient classes after the Gospels. And a long list of intermediate texts was given; it was held, indeed, that no document preserved any one text uninjured. A misunderstanding—shared in part by Griesbach himself—of the bearing of these two facts (which simply proved that the typical texts had suffered severe admixtures with one another in framing our existing documents), went far in throwing doubt on the details of Griesbach's distribution, and thus in preventing an universal acceptance of it, although it could not hide its true character from the best scholars of the day, many of whom enthusiastically adopted it. Hug's vagaries, who sought to prove historically that three texts represented respectively by the groups B C L, E R cursives, and A K M were alike set recensions of a corrupt text (represented by D and the old Latin) universally current in the second century, still farther blinded men to the reality of the divergence, considered simply as a text phenomenon, between the three classes recognized by Griesbach and Hug alike, as well as to the truth of the important new fact brought out by Hug, viz: the early broad extension and popularity of Griesbach's Western text. Hug's publication had, however, the good effect of bringing Griesbach once more before the public on the subject (1811), to call attention to Hug's testimony to the correctness of the lines which he had drawn between his classes, prove the impossibility of raising Hug's fourth class (which he himself admitted was untraceable outside the Gospels) to the dignity of a co-ordinate division, and reiterate his mature con-

* Except in Matt., where he (wrongly) deemed it Western.

viction that the study of "recensions" was the hinge upon which all criticism of the text must turn. The follies of such writers as Dr. Nolan and the peculiarities of Scholz succeeded, however, not unnaturally, in throwing discredit on all recension theories, until they have fallen under something like a ban, and the prevalent idea seems to be that no classes can be distinguished of such sort as to be, at present at least, practically valuable in text reconstruction except the two broad ones—now universally recognized—of ancient and modern. At the same time it is generally practically acknowledged that the further facts of type-character as brought out by Griesbach, although not available in text-criticism, yet rest, in the main, on a basis of truth. Even Dr. Tregelles* would admit a genealogical descent, which he moreover practically acted upon in framing his text, which divides the MSS. into three classes corresponding with those of Griesbach. And, at the other extreme, Dr. Scrivener specifically allows a like trichotomy of documents capable of bearing like names.† It is furthermore admitted on all sides that the oldest documents are included in the first two classes; and, as a result of the process of comparative criticism introduced by Tregelles, that these documents are not only the oldest, but also the best, so that whenever they are fairly unanimous they must carry our suffrages with them. It is hardly less generally agreed that within the ancient division those documents which class with B—which itself is the best single MS.—are of greatly higher value than those which class with D. These conclusions—although not undisputed by some individuals—are accepted by the best writers of all schools, and may, therefore, be looked upon as well-proved and already settled facts.

3. Meanwhile, also, the continued efforts of many scholars toward forming a text out of the existing material were issuing in critical rules for applying the evidence to the text. We can pause only to point out the leaders in the work. Bentley first laid down the great principle that the whole text is to be formed apart from the influence of any edition, on evidence,—a principle which, obvious as it is, first succeeded in conquering its way to practical and universal adoption through the weight of Lachmann's example. It was due to Bengel that the value of transcriptional probability received early recognition through the rule: 'Proclivi scriptioni præstat ardua,' which undoubtedly he meant in this sense; after him it has been more fully defined

* "Horne's Introduction." Ed. 13, Vol. IV., p. 106.

† "Plain Introduction," etc. Ed. 2, p. 481 (Egyptian, Western, and Syro-Constantinopolitan classes). Yet compare p. 415.

and defended by many critics, especially by Griesbach, by Tischendorf (in the broad statement that the reading is to be preferred from which the origin of all the others can be explained), and by Ellicott (under the name of Paradiplomatic evidence). Internal evidence proper also,—the asking which reading it is most probable that the author would have written—has not lacked its full recognition, and has been pushed by some to the verge of subjecting the whole text to the personal idiosyncrasies of the editor. Since Tregelles the suffrages of students have been given to the doctrine that documentary evidence is decisive, if at all capable of sure interpretation,—so only the reading commended by it does not make nonsense. But the claims of paradiplomatic and internal evidence have never lacked defenders of excellent scholarship, and it cannot be said that any universally recognized rule has yet been formulated to guide in cases where documentary and internal considerations seem in conflict. While also the tendency has been more and more to rely on the *ancient* documentary evidence and its decisive authority where at all unanimous, is now universally (save by an erratic individual here and there) allowed, yet in those passages where this evidence is apparently somewhat divided the way has been open to a great variety of methods of procedure issuing sometimes in diametrically opposite conclusions even in readings of some interest.

A backward glance like this over the work that has been done, leaves standing clearly out in our consciousness the problems as yet unsettled. It was clearly not necessary for the new editors to seek to add to the mass of evidence before them; the day has now come when the true estimation of that evidence is the duty laid on the shoulders of scholars. Two great tasks lay before them: the investigation of the true extent and meaning of the affiliations of MSS., and the pointing out of the true method of applying the evidence when marshalled to the framing of the text. It was not enough to classify the MSS.; the true relations of the classes to one another needed study, and the true value of the evidence of each class. Therefore, here, not only was it necessary to re-examine the whole distribution of the MSS. into classes, but also the relations of the classes to one another had to be investigated with a view to accounting satisfactorily for the intermediate types on the one hand and to assigning its own value as evidence to each class and each combination of classes on the other. It was not enough to simply marshal the evidence—it was necessary to discover how to apply it when marshalled; with how much regard to each variety of evidence, documentary, paradiplomatic, and internal. With great sagacity,

Drs. Westcott and Hort recognized from the very first the true nature of their task, and devoted themselves to fulfil it. Our examination of their methods need take account, therefore, only of the results which they have reached in these two departments of labor.

THE GENEALOGIES OF DOCUMENTS.

We turn next, therefore, to an exposition of Dr. Hort's investigations in the great sphere of MS. classification. The obvious and universally accepted two-fold division of documents as to their text, represented by the ancient MSS. and the cursives respectively, is of course recognized by him at the outset. The important unsettled question of the relation of these two texts to one another is, therefore, faced immediately. It is first proved, from the citations of the fourth century fathers, that the cursive type of text existed fully formed in that century, *i. e.*, in MSS. contemporary with B and \aleph . Thus, the mere fact that our only extant fourth century MSS. represent the opposite forms of text is not at all conclusive as to the greater age of those forms. We can reconstruct from the cursives MSS. which beyond doubt existed, representing their type, in the fourth century; and the preservation of early documents representing the one class and not of those representing the other, is a pure accident. Thus far, therefore, nothing is determined concerning the comparative age or value of the two forms of text.* Going back beyond the fourth century, however, no

* It is worth our while at the outset of this discussion to guard against misconceptions as to the meaning of the phraseology used. We speak of different types of text, and the words have meaning in them. It is very important, however, that the reader should not exaggerate that meaning. The total difference is very small. What is very large when viewed from the point of view of the textual critic, is pitifully and meaninglessly small when viewed from the point of view of the dogmatic theologian or the general reader. The textual critic does not exaggerate the difference; but every letter omitted, every word misspelled, every synonym substituted is a difference to him, although the vast majority of them cause no change of sense in the passage by their presence or absence. They are nevertheless—though only *textual* phenomena—yet *textual phenomena*. And on their basis types and well-marked types of text may be recognized and described. To juggle with this, however, as the *Quarterly Reviewer* has done (p. 314), trying to shift it into another sphere and pouring into the terms totally alien concepts, is beneath the dignity of the scholarship which he undoubtedly possesses. Dr. Hort (§ 2) is careful to show how small a part of the N. T. is affected by various readings of any likelihood. And the statement of Bentley, as true now as in his day, is worth keeping constantly in mind: "The real text of the sacred writers . . . is competently exact in the worst MS. now extant; nor is one article of faith or moral precept either perverted or lost in them; choose as awkwardly as you will, choose the worst by design, out of the whole lump of readings." "But even put [the various readings] into the hands of a knave or a fool, and yet with the most sinister and absurd choice, he shall not extinguish the light of any one chapter, nor so disguise Christianity but that every feature of it will still be the same." Our whole discussion concerns—not *sense-ual*—but *textual* variations, and MSS. cannot be distributed into doctrinal or even *sense-ual* types, but only

trace of the cursive peculiarities can be found in the citations of the Ante-Nicene fathers; while on the other hand their citations, when critically obtained, all range with the opposite classes, and especially with that form of them which has been named the Western, and which was certainly the most broadly current text from the early part of the second century until the fourth. We have, therefore, to face this phenomenon: universal and, so far as evidence goes, sole currency of the ancient types of text, which Dr. Hort therefore calls the pre-Syrian, until the fourth century, with the sudden presence of the other, which Dr. Hort from the predominance of Syrian influences at this period calls the Syrian, in its full-formed state from the fourth century onward. Negative evidence cannot be demonstrative: but the presumption hence arises that the pre-Syrian texts are the oldest, and this in turn throws a presumption against the purity of the Syrian.

The next step is to compare the Syrian and pre-Syrian texts in their internal characteristics with a view to determining their relative values. If we collect two lists—one of all the readings which the Syrian text as a class offers in opposition to the pre-Syrian as a class, and the other of all the pre-Syrian readings where, as a class, they differ from the Syrian as a class, the two together thus forming the two sides of the same collection of various readings between the two classes—and then test the two lists separately by paradiplomatic and internal evidence, we shall reach this result: the pre-Syrian readings usually commend themselves as genuine; the Syrian readings usually present the appearance of corruptions. Hence, it doubly follows that the pre-Syrian text is certainly the better of the two; since it approves itself as such wherever it can be tested, the inference is strong that it is such also where the test cannot be applied. Thus we reach the same conclusion (and by largely the same methods) that Tregelles obtained by the application of what he happily called comparative criticism, but what Dr. Hort would call a combination of historical evidence and the internal evidence of documents. The result is sure, and the process by which it is obtained, in either case, trustworthy. But Dr. Hort's method has the advantage of being the more precise and methodical.

Although the Syrian text is thus presumptively the later, and certainly the less valuable, our problem is not yet solved, and cannot be until we answer the query: Whence came this Syrian text? It is still conceivable that it may preserve in itself an independent line of

into *text* types. So far as general sense is concerned, the New Testament is the same in all MSS.; and the dogmatic theologian or preacher of righteousness does not need to consider the variations save in determining *which* texts to use as proof-texts.

evidence, which ran underground during the early centuries and first came to light in the fourth, and which, though not so valuable as the pre-Syrian, cannot be safely neglected. The question so constantly put: "What right have we to pass over the testimony of this class as if it were *impossible* for it to contain independent evidence?" can never be answered without a very careful search into the origin of the class. Undertaking this work, Dr. Hort has instituted a very careful comparison between the Syrian and pre-Syrian texts, with this conclusion: the Syrian preserves nothing from antiquity not in the pre-Syrian—it was, in fact, bodily made out of the pre-Syrian forms. The proof of this is manifold and convincing. We need not stop here, however, to do more than point out one element of it—that derived from *conflate readings*. These arise from cases of ternary variation where the third reading is a combination of the other two. Now the Syrian text abounds in conflate readings, made by a more or less skilful combination of two pre-Syrian forms. One such reading might be accounted for as an accident, but the mass of them prove conclusively that the Syrian text in all these passages was derived from a combination of these earlier types. It becomes immediately (when the other phenomena are also taken into account) morally certain that other readings in the Syrian text, exactly the same as readings found in a pre-Syrian type, thus proved to have been used in its making, also came from this previous text. The inference cannot fail to extend further to those Syrian readings which, while not the same as those found in pre-Syrian texts, are yet declared by diplomatic evidence to be derived from them. The result, after careful investigation, is, that the Syrian text preserves nothing not in the pre-Syrian forms, out of which it was made; and, further, that it was made, not by accidental and slow growth, but intentionally, and by a set effort to frame a full, smooth, flowing, easy text out of the already existing abounding variations. It is, therefore, not only presumptively later than the pre-Syrian, but certainly; not only of less value as evidence, but of no value at all, where we have the pre-Syrian, out of which it was made. Its testimony is not to the original, but to the pre-Syrian texts, and it could be of value, in their presence, only if we could believe that it had been framed on critical principles, and so could guide us to a proper choice among pre-Syrian readings. But, to say nothing of what is otherwise known of the critical processes of the time, the internal evidence is decisive that the principles which guided its formation did not rise above the effort to obtain easy smoothness.* The presence of Syrian documents, therefore,

* We trust that we can count on the assent of the *Quarterly Reviewer* here, so soon

in attesting groups is simply confusing—multiplying variations or lending fictitious weight among early variations to this or that one which happened to find its way into it.

Two important rules of critical procedure may now be formulated: 1. All distinctively Syrian readings must be rejected, and: 2. All purely Syrian support to earlier readings must be neglected. Here, for the first time, is the practice of Tregelles in neglecting all late testimony fully vindicated. It is neglected, not because the evidence of this class is too small to be appreciably felt, but because it is not independent evidence but a mere repetition of that already in hand. All the evidence is certainly to be taken into account until the history of the text is recovered and the mutual relations of the witnesses determined. Then all purely derived evidence is to be sifted out. Here, too, a full answer emerges to the scoff, that, from the mass of the cursives, those which happen to agree with the old MSS. are arbitrarily selected, while the rest are as arbitrarily rejected. Of course, all those which prove to transmit the independent lines of evidence are justly selected, while, in like manner, those that betray themselves to be mere repeaters of the testimony already heard are as justly rejected. This is simply to protect the ballot-box; and it is certainly a great gain to criticism to be thus fully justified in setting aside the clamors of the mob and giving its attention to the trusty few alone.

Thrown back on pre-Syrian witness the difficult question is broached: How proceed when this witness is divided? Dr. Hort answers again, primarily by seeking the genealogical affiliations of the documents. The clear distinction between the groups headed by B and D respectively, is, of course, recognized and abundantly re-proved, and evidence is found of the existence of a third less strongly marked type, differing from the B group only by the presence of certain careful (grammatical, etc.) corrections. The three classes are called, respectively, Neutral, Western, and Alexandrian. So far was clear sailing. The difficulty arises when the relations of these groups to one another are considered—relations complicated most tryingly by the existence of intermediate types of almost every possible variety. Here the second great unsettled

as he examines the evidence adduced by Dr. Hort. Certainly he can have no *à priori* objection to the conclusion, since he writes (p. 321): "We know that Origen in Palestine, Lucian at Antioch, Hesychius in Egypt 'revised' the text of the New Testament. Unfortunately, they did their work in an age when such fatal misapprehension prevailed on the subject that each, in turn, will have inevitably imported a fresh assortment of *monstra* into the Sacred Writings." Just so. We call upon him to recognize just such a text as he describes in the class (Syrian) to which he has hitherto accorded mistaken suffrage, and to hold with Dr. Hort that it possibly represents the Lucianic revision.

problem appeared, which, however, like the first, seems to have been successfully solved. It is remarkable, indeed, that these intermediate texts should have so long disturbed scholars. Clearly their presence does not in any way lessen the actual divergence between, say B and the Old Latin version. The only problem is their origin. Explanations might be sought by considering them representatives of the links in the gradual chain of corruption from a type, say like B, to one like the Old Latin—or, in both directions, from a type intermediate between the two—or of mixture of the two diverging texts already formed. Undoubtedly all these causes may, and ought to, be called in to account for the phenomena. Corruption was clearly progressive—the result of a gradual growth—and the marks of the growth are preserved in the extant documents. But Dr. Hort has shown that much the largest portion of intermediate phenomena is due to *mixture* between two or more already existent types. There is no difficulty in accounting for mixture: it could arise in a variety of ways—sometimes from the scribe actually using two originals in making his copy; sometimes from the tricks of a memory full of the details of a different exemplar than that now before the eye; sometimes from the use, as exemplar, of a MS. which had been corrected in part, or throughout, from another of a different class. But, however produced, the existence of mixed texts can by no means throw doubt on the original diversity of the parts out of which they were made. They may, and sometimes do, render difficult or impossible the assignment of a simple genealogy to a given document, or, in cases where unmixed evidence is lacking, the definite assignment of given variations to their own proper classes; and thus, in some passages, they may render the application of genealogical evidence to the elucidation of the textual history and the formation of the text impracticable. But they most certainly do not affect either the reality of the groups or the surety with which we may assign the variations, for whose affinities there does not exist safe evidence, to their own proper classes. In a word, they do not affect the value of genealogical evidence wherever it can be applied.

Having thus determined the existence of three pre-Syrian groups, and assigned to each group its own proper contingent of the readings, the next step is to test the relative values of the pre-Syrian groups. The process by which this is done is altogether similar to that by which the pre-Syrian readings, as a class, were proved superior to the Syrian. Having made lists of the readings of each group, so far as mixture allows of their assignment, paradipto-

matic and internal evidence is appealed to to decide as to the value of each. They proclaim the Neutral readings generally right, and the Western and Alexandrian generally corruptions. Hence follow, as critical rules of ordinary validity: 1. The reading supported by the Neutral and Alexandrian groups against the Western is probably genuine; 2. That supported by the Neutral and the Western against the Alexandrian is probably genuine; 3. Where the pre-Syrian variation is ternary the Neutral is probably genuine, and is usually supported as such by paradiplomatic and internal evidence; 4. The reading supported by the union of the Western and Alexandrian groups should be preferred to the Neutral reading; but, as all existing Alexandrian documents contain Western corruptions, such apparent union is suspicious, and paradiplomatic and internal evidence generally decides here also in favor of the Neutral.

It is plain that we have here an exceedingly clear and trustworthy scheme, and it only remains for us to note the observed group-character of our best documents to enable us to apply the rules to a large number of readings. Examination shows that only five of our MSS. are purely pre-Syrian, viz: B, \aleph , D, D₂, G₃, although a considerable number of others, such as C, L, P, Q, R, T, Z, Δ (in Mark), Ξ , E₂, and some cursives, contain a pre-Syrian element of greater or less extent. D, D₂, and G₃ may be taken as representative Western documents, and seem to present that text unmixed, but in different stages of development. C and L, though with much mixture, possess the largest Alexandrian element. B is purely Neutral almost throughout (*i. e.*, except in Paul, where a limited Western element is found). \aleph is largely Neutral, but in admixture with a considerable Western and Alexandrian element. After \aleph , and with about as great an interval between them and it as between it and B, the largest Neutral element is found in Γ of Luke and John, Ξ of Luke, L, 33, Δ of Mark, C, Z in Matt., R in Luke, Q and P among MSS. of the Gospels. In Acts A, 13 and 61 come forward, and in Paul, A, P₂, 17 and 67**. Among the versions the Old Latin (not the Itala) is found to be purely, and the Curetonian Syriac probably predominately Western; the Memphitic was probably originally wholly pre-Syrian and predominately non-Western, but in its printed form it has a slight Syrian element also. The Thebaic is similar, except that its Western element is larger. The others present mixed texts with larger or smaller Syrian elements. Thus, it appears that the old verdict of scholars is confirmed, and the Memphitic is proved the best, followed next by the Thebaic, of all versions in

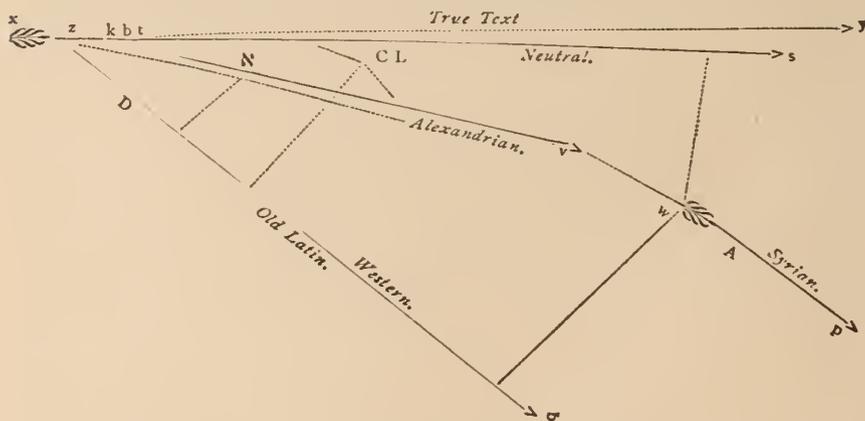
text-criticism. Among the Fathers the non-Western, pre-Syrian element is largest in Origen, Didymus, and Cyril Alexandrinus.

Of course, genealogical evidence will not settle everything; but by its systematization very much has been gained. The early history of the text has been recovered; a vast number of readings have been winnowed away with the Syrian text as not worthy of consideration; a large number have been rendered very improbable by their definite assignment to aberrant texts like the Western and Alexandrian; a large number—rivals of these—have been, therefore, shown to be probably parts of the original text; and thus a goodly portion of the text has been securely reconstructed, and the choice confined in a numerous class of other passages to narrow limits. A comparatively very small portion of the text is thus left in uncertainty.*

INTERNAL EVIDENCE OF GROUPS.

In order to determine the true reading in those cases where from whatever cause genealogical evidence is inapplicable or fails to be decisive, as well as to test the results obtained by that form of evidence, Dr. Hort calls in next another process which he appropriately names Internal Evidence of Groups. Internal evidence of readings

* Perhaps the genealogy of the text and the results which flow from its determination may be rendered easily comprehensible through a rough diagram, thus :



If x y represents the line of absolutely true descent, z q, along the course of which the various Western documents may be ranged in growing corruption, will roughly represent the Western divergence—k s the Neutral and t v the Alexandrian; w p represents the Syrian. Now it is evident that B, placed at a point between k and t, is the nearest to the originals of any MS. B N will carry us back to a point on k s, or to a point between z k or (when N is Western) to z, B D takes us to z. N D, on the one hand, may be equal to B D, and, on the other, may be equal to D alone, i. e., may take us to z or else somewhere amid the abounding corruption of z q, and so on.

is the evidence for itself yielded by each reading's own probability when tested by the combined use of paradiplomatic and internal evidence proper. Internal evidence of documents is the evidence which each document yields to its own value; and is elicited by noting what proportion of its readings approve themselves as probably genuine when tested by the combined use of paradiplomatic and internal evidence proper. If we take a list of all variations between two documents, and finding them to be eleven hundred in all, then discover that in a thousand of them all the probability is in favor of the correctness of one of the MSS., and only in a hundred of the other, we have thereby determined the probable comparative values of the MSS. The result is essentially altered neither where the contestants are one hundred instead of two, nor where the evidence applied is decisive in only a portion of the passages compared. Now we may carry this process one step higher until it becomes internal evidence of groups. If two MSS. agree in a reading, this is evidence, barring accidents, of community of origin in that reading. If they agree thus in a number of readings, accidents are barred, and their common origin in these portions—immediate or remote—is proved. It is immediately evident that by noting the readings in which two MSS. agree we are really constructing a list of readings from an older MS., the common parent of both in these portions. Nor does it introduce any new factor if we make the two MSS. a dozen or a hundred. And nothing prevents our testing through this list the comparative value of this lost MS. thus reconstructed, in relation to others reclaimed in like manner, just as if they were all extant and in our very hands. The compound of symbols (B \aleph , or \aleph D, or B C \aleph , etc.), the largest proportionate number of readings attested by which are approved by combined paradiplomatic and internal evidence, represents the best lost original, and should command our suffrages. This method enables us to deal with groups as units, and greatly simplifies the labor of criticism as well as adds, by freeing us from the old arithmetical balance of individuals and enabling us to assign a constant value to any given group, untold surety to its conclusions. It seems, on the face of it, to be impossible to doubt the legitimacy of the process or the surety of its results. But were doubt to arise, it should certainly be set aside on noting how fully these results confirm those reached by genealogical evidence, and are in turn confirmed by them. This is a veritable case of undesigned coincidence, and is entitled to all the force of that argument.

Tested after this fashion, the compound B \aleph is found to approve itself almost uniformly as genuine, and next to it B plus some other

primary uncial; while on the other hand compounds of \aleph and an uncial other than B generally fail to make good their claim. The only frequent exception to this law consists of compounds of B and a Western document *in the Pauline epistles*, which are usually discredited. We cannot resist the temptation to turn aside here long enough to call attention to the striking accordance of these results with the facts reached by the entirely different process of genealogical evidence. If B is the only document which (except in Paul) has no other than a Neutral element, its compounds will naturally (usually) present a combination of two independent groups; while all other documents (including \aleph) when conjoined, are apt to be so, only because they partake of common Western, Alexandrian, or Syrian corruption. The high comparative value assigned to compounds of B by the method being now considered, is thus just what should be expected. B plus only one or more secondary MSS., or B plus versions alone, or B plus fathers alone, commonly approves itself by the same test; whereas \aleph plus only such support (and much more any other uncial than \aleph) is almost uniformly condemned. Even individualisms of B when they cannot be ascribed to clerical errors of its scribe, quite frequently, and especially in ternary variations approve themselves; while individualisms of other MSS. are almost always condemned. After the Gospels, A rises to the value of a primary uncial, and in Paul no MS. is without some Western element. Consequently we are not surprised to find that such groups as "B D₂ G₃, \aleph D₂ G₃, A D₂ G₃, C D₂ G₃, and even A C D₂ G₃, and occasionally \aleph A C D₂ G₃" are condemned by internal evidence of groups. On the other hand the same test is usually favorable to the apparently non-western groups; and even, with rare exceptions, to \aleph B D₂ G₃, thus vindicating even here the combination B \aleph .* In the apocalypse \aleph falls to a perceptibly lower level than elsewhere, and the strongest combination is A C; and even A alone stands the test excellently.

The most striking results reached by this investigation are the high authority given to B and to the combination B \aleph . Dr. Hort proves the immediate independence of these MSS.,† and thus shows that the

* This simply amounts to an indication that \aleph and B gain their Western corruptions independently of one another (and forms another mark of the independence of the two MSS.), and hence do not usually partake of the *same* Western corruptions; and hence when combined B and \aleph agree even with typical Western documents, we are not to look on the Western line of corruption for the original parent of the groups, but on the original line of descent, that is, at z on the diagram. And this means, doubtless, in the first century.

† We content ourselves with this simple statement here, referring for proof to Dr. Hort's Introduction, §§ 237-304. The rash repetition by the *Quarterly Reviewer* of the old and worn-out charge: "Between B and \aleph there subsists an amount of sinister

combination represents a document of the early second century—if not a generation earlier; which itself represents seemingly the pure stock from which all others in existence appear to have diverged.* This high estimation of these documents has been even made the pretext of attack upon the system of criticism adopted by the whole school to which Dr. Hort belongs, and that although it is universally admitted that B is the best single MS. in existence.† The answer is

resemblance which proves that they must have been both derived at no very remote period from the same corrupt original" (p. 312), is there fully set aside, if indeed the Reviewer has not himself succeeded in destroying its meaning by his subsequent words: "It is easier to find two consecutive verses in which the two MSS. differ the one from the other, than two consecutive verses in which they entirely agree." (The italics are his). The fact that a small portion of \aleph is from the same hand that wrote B as much proves a community of text as the fact that Dr. Scrivener's Greek Testament and Westcott and Hort's came from the same press, proves that they present the same text.

* *Represents*, not *is* that pure stock. Such passages as Matt. xxvii. 49 (compare Dr. Hort, § 240), prove that B and \aleph possess exceedingly rarely a common corruption not shared by Western documents, so that B \aleph D represents the same stock at an earlier point. Thus B D or non-Western \aleph connected with D may differ in value from B \aleph , not in giving a less ancient or less pure reading, but only as giving so many fewer readings. B D when it does exist may be (save in Paul) equally as good as, or better than B \aleph .

† Of course it is not meant that no individual has ever disputed the supreme excellence of B; but only that all recognized authorities of whatever school are united at present on this point. The *Quarterly Reviewer* does not shrink from ranging himself against the consensus of critical opinion. With him B is not only a MS. of "bad character," and one that "exhibits a fabricated text" (p. 312), but one of the depraved trio (D \aleph B), which he can "venture to assure" his readers "are three of the most corrupt copies extant," and "have become by whatever process the depositories of the largest amount of fabricated readings which are anywhere to be met with" (p. 315). It is pleasant to learn that B is, however, even in the eyes of this critic, on the whole the least terrible of this terrible trio. The answer to all this is found in the statements of the text, supported as they are by all writers of repute on the subject. What confidence can be put in the Reviewer's broad statements on the subject may be not unjustly estimated by the aid of two circumstances: 1. He refers for detailed information on such points to Dr. Scrivener's "Plain Introduction," etc., as (and we take great pleasure in expressing our assent to the words) the work of a "judicious, impartial, and thoroughly competent guide" (p. 311)—of one even (he tells us) "vastly Tischendorf's superior in learning, accuracy, and judgment" (p. 318). And yet Dr. S. is explicit in his statement (p. 471) that B is "the most weighty single authority that we possess." 2. He allows himself (p. 321) in his zeal against B to quote Dr. Scrivener's description of the corrupt Western text (pp. 452-3), and apply it to B as one "of the class thus characterized by Dr. Scrivener"; and that although Dr. S. had carefully distinguished it from the class described (p. 452). It is true the Reviewer guards his statement somewhat by saying \aleph B C D are "*specimens—in vastly different degrees—of the class thus characterized,*" but this will not exonerate him from having printed a very misleading statement. For that the very small Western element in the Pauline portion of B will not be sufficient to justify the words used is apparent; and becomes still more so on remembering that the object of the passage is to exhibit the untrustworthiness of these MSS. *because* they class with D, whereas all the documents which the writer himself follows have a larger Western element than B.

The details which are given (as *e.g.*, p. 312) of the divergence of each of the great MSS. from a given standard are very interesting, but, as the Reviewer puts them, misleading in the extreme. When we read that "in the Gospels alone, B is found to omit at least

ready and complete ; only such authority is yielded to B or to B alone as that group or that MS. when tested by paradiplomatic and internal evidence vindicates for itself. The further scoff so often ventured, that the discovery of a fourth century MS. of Syrian type would revolutionize criticism and utterly change the balance of evidence, of course is equally meaningless. Such a discovery would have absolutely no effect on either. MSS. are to be valued, not counted ; and the age of the document is presumptive of value of text only prior to examination. Even though the Syrian text should be traced further back than now seems possible, nothing can alter the two facts : that it is inherently—paradiplomatic and internal evidence being judges—the inferior text, and that it was made out of the pre-Syrian. Nor will it do to raise objection to the reconstruction of lost MSS. from group—attestation as a chimera of the imagination furnishing only shadowy basis for farther inferences. If this be so, then any reconstruction of the New Testament text is *à fortiori* a dream. For internal evidence of groups only undertakes to do repeatedly and on a small scale what its opponents would attempt to do once for all on a large scale. The recovery of each lost MS. is only on narrower ground, and with more manageable and surer evidence, performing the task that all attempt in seeking the autographic text from documentary attestation. The only difference between the two methods is that one

2,877 words ; to add 536 ; to substitute 935 ; to transpose 2,098 ; to modify 1,132 (in all, 7,578)"—the thing looks alarming, and we feel a flesh-creeping all over. But when we revive sufficiently to ask : Omits from what ? adds to what ? etc., we discover, from a subsequent part of the article, that the *ordinarily printed text is meant*, and breathe freely again to know that this is but a list of divergences between B and the corrupt *Textus Receptus*, and therefore, roughly marks the corruption of that edition, not of B. It may be safely left to the public to decide on the fairness of quietly assuming that, in spite of the history of its formation, the *Textus Receptus* is all but perfect, and then before a popular audience quietly condemning the old MSS. for not agreeing with it, without a word of warning as to the exact nature of the question-begging which will alone give the words any sense or meaning. This quiet begging of the question—this quiet assuming the truth of a disproved fancy—is what gives at once the appearance of strength to the *Quarterly's* article and the reality of almost laughable weakness.

As to critical rules the Reviewer seems to have but two : 1. Witnesses must be counted, not weighed ; and 2. Internal probability consists in the pleasingness of a reading to us—with all our long use of a particular text and natural and ingrained love for its every detail. Perhaps we may be allowed to borrow a phrase which is most strangely strewed up and down the Reviewer's pages, and "venture to assure him" that the day is past when men can be allowed to mistake their personal preference for internal probability on the one hand, or on the other to give the inheritance of the lawful heir or two to the twenty children of the illegitimate son, just because they are more. And what is counting MSS. instead of weighing them but this ? Communism,—the theory that each individual, merely by right of his existence, can demand an equal share in all the rightful possessions of his neighbors,—seems to us inherently unlovely, whether among MSS. or men. Before we yield credit, let us by all means examine titles.

would wish to proceed slowly and surely—step by step—working its way from one fact to another by a strictly inductive method; and the other to jump at once crudely to its last conclusion. The difference, in a word, is the same as that between Bunsen's and Mackenzie's theories of Geysers—between the Baconian and the so-called Aristotelian methods of thought—between science and guessing.

THE APPLICATION OF THE EVIDENCE.

With the documentary evidence thus in hand, and thus estimated, how is it to be applied in reconstructing the text? From what has already been said, it goes without saying that the new editors do not apply it mechanically; and, on a moment's consideration, it must be seen that such a method of application would not be practicable. Clearly, even the purest line of transmission (say the Neutral) *may* contain errors introduced into that line subsequent to the divergence from it of a very corrupt line (say the Western), in which alone the true reading may thus be preserved, and the exceedingly early origin of Western divergence leaves it not *à priori* impossible that this may in certain instances be the case. In such instances the true reading would lie outside of the evidence usually considered conclusive in the formation of the text. The fact that such cases do occur, and the proof that any given asserted instance falls under this class, can only be sought through paradiplomatic and internal evidence. Other considerations of a somewhat like nature lead to the same conclusion. Hence, as the original, and not the best authenticated transmitted text is sought, it follows that the evidence cannot be applied mechanically. If, therefore, Dr. Hort's first great critical rule is: *Knowledge of documents must precede judgment on readings*; and his second: *Knowledge of genealogies must precede judgment on evidence*; his third, co-ordinate with these, even if not so formally stated, is: *No reading is to be finally accepted unless commended by internal evidence as well as documentary*—in internal evidence including both paradiplomatic and internal proper, under the names of *transcriptional* and *intrinsic* probabilities. Thus, to internal evidence is allowed a veto power, and its function is to a large degree analogous to the veto power the President of the United States has allowed him over bills of Congress. Recognizing the uncertainties and dangers that attend appeal to internal considerations, every attempt is made to guard against them. It is not in either sort to be primarily invoked; it has a right to be heard, indeed, but it must keep silence until the testimony of the documents has been sifted and thoroughly understood. Then, when

offered, it must be unanimous; both kinds must point in the same direction. Care must be taken that we try the readings intrinsically, not by our own notion of what should be read, but by an anxious attempt to reproduce the writer's own thought. Equal care must be taken that we judge the transcriptional probability by the actual mental tendencies of the scribes, and not by our own which may be opposite. And, still further, internal evidence must be allowed to override documentary probabilities only when, after repeated, and still again repeated testing, it persists in ranging its combined testimony in opposition. Often what is originally judged intrinsically probable is afterward seen to be untenable, and the reading at first imagined intrinsically improbable is seen, on repeated study, to be intrinsically certain. Often what is originally judged transcriptionally improbable is, on further study, seen to be transcriptionally certain. When, however, after this repeated testing and re-testing, the verdict is clear, that one reading is intrinsically best while apparently troublesome, while all others combine latent inferiority with open plausibility, then this combined testimony can never be safely disregarded, and, practically, is judged supreme. Thus, the clear united testimony of transcriptional and intrinsic evidence, though it is only secondary evidence in the sense that it must not be considered until the last word from the documents is in, is yet, in Dr. Hort's scheme, primary evidence, in that it is supreme and may override any and all documentary evidence.

No doubt it is easy to say that thus very great authority is assigned to a class of evidence which is peculiarly liable to mistake, and to be especially swayed by subjective feeling. True, we may answer, but how can we do otherwise? It may be admitted that it is easier to gather the external evidence, determine its meaning, and then apply it mechanically to the text. But will the result be truer? Royal roads to truth are not usually judged highly estimable, and the difficulty of a task is hardly sufficient reason for declining to undertake it altogether. It is undoubtedly difficult to abstract personal likes and dislikes, educational prejudices, the prescription of use and wont from our judgment of the bearing of internal probabilities; but these difficulties must be faced and laid, or, in ruling one half of the evidence out of court, we rule all hope of a perfect text out with it. At every step of a valid critical procedure we are forced to call in internal evidence to decide for us the relative value of rival documents or classes of documents: how can we refuse it, then, a final voice in deciding between rival readings? It may be open to question whether Drs. Westcott and Hort have not allowed it in

certain particular passages too much weight; or *vice versa* in certain passages, too little weight; but there can hardly be continued question but that the principle is correct, that no reading can be held to be absolutely certain unless it can be shown to be commended alike by documentary and both sorts of internal evidence. And the great merit of the scheme of criticism which Dr. Hort offers is just this: that it takes full account of every variety of testimony, and will not allow that its work is done until it has heard the united voice of the three great forms in which evidence reaches us. Certainly a text constructed thus is, above all others, a sure text.

CRITICAL CONJECTURE.

The high value thus assigned to internal evidence leads to the revival, as an adjunct in the settlement of the text, of the old method, once so popular, of critical conjecture. The vagaries of those who have most used this method long since brought it into not undeserved contempt. But *à priori* it will be difficult to see why it should be excluded from possible resort in reconstructing the text of the New Testament alone, of ancient books. The documentary evidence, mechanically applied, will take us here, too, only to the earliest transmitted text; and whether this be the autographic text as well, or a more or less corrupt descendant of it, can be learned only by an appeal to the two varieties of internal evidence. But the mere fact of questioning internal evidence on the subject implies that it may give its testimony against the transmitted text, and if so, in any passage, what is left us for the reconstruction of the text but pure or impure* conjecture? The very act of reconstructing the text on any other method than that of absolutely mechanically applying the documentary evidence admits the legitimacy of conjectural emendation. It may be said here, again, that thus a wide door is opened for the entrance of deceitful dealing with the Word of Life. The danger is apparent and imminent. But we cannot arbitrarily close the door lest we incur the same charge. It is true here, as elsewhere, that wicked men have it in their power to deal wickedly with God's Word, and that our only safeguards against it are piety and right

* By *pure* conjecture is meant conjecture unsupported by any external testimony; by *impure*, conjecture supported by documents of insufficient authority to of itself authenticate the text. Impure conjecture is, then, simply (in all ordinary cases) the adoption, by a modern editor, of a successful conjecture of an ancient scribe. It is worthy of note that every editor (most of all, those who retain the Syrian text) admits impure conjectures into his text, and those of such sort that their MS. attestation cannot be possibly accounted for by any theory of transmission; and it is difficult to see why sixth or tenth century scribes should be allowed the monopoly of conjectures.

reason. Two precautionary requirements are, however, in place: 1. First of all, it must be demanded that clear occasion for conjecture shall be required in each case where it is offered, and, unless not only its legitimacy can be proved, but in each case also its *necessity*, we shall allow none of it. It has, indeed, been often asserted that it has been already proven that there is no occasion for conjecture in the New Testament; but the assertion is certainly premature—the most careful writers feel it impossible to stand upon it—and we cannot afford to repeat it.* 2. And next, it must be demanded that, even if the necessity for conjecture be proved in a particular case, no emendation offered be accepted unless it perfectly fulfils the requirements of both varieties of internal evidence. With these demands we may occupy both safe and reasonable ground.

We do not wish to conceal our belief, moreover, that in the very large majority of the cases†—perhaps in all—where Dr. Hort or Dr. Westcott or both consider that primitive error exists in the reconstructed text which must be removed by conjecture, we cannot feel that the claim of necessity for it is even very plausible, much less made out. It is, therefore, a matter of deep congratulation that they have not deformed their text with conjectural emendations, but have in every

* Dr. Roberts ("Words of the New Testament, etc.," p. 24) and Mr. Hammond ("Outlines of Textual Criticism, etc.," p. 8) take broadly the ground that there is no need for conjecture in the New Testament. The *Quarterly Reviewer* simply says, with characteristic emphasis (the small capitals are his): "May we be allowed to assure him that IN BIBLICAL TEXTUAL CRITICISM 'CONJECTURAL EMENDATION' HAS NO PLACE" (p. 320). On the other hand, Dr. Tregelles (Horne's Introduction, Edition 13, vol. iv., p. 150) and Reuss (Geschichte d. heil. Schrift. d. N. T., p. 398) speak doubtfully; and Dr. Scrivener (p. 433 and note) admits the need, but would banish the method on the plea of expediency. At present there seems to be a pretty general reaction in favor of conjecture in progress. In Holland, indeed, the traditions of Valcknaer, kept alive in our own day by Cobet et al., never died out. The latest marks of the same spirit there may be found in Dr. Hartings' Essay, in 1879, and the Teyler Society's publications for 1880 (by Van Manem and Van de Sande Bakhuyzen). In England the conjectures printed in the *Cambridge Journal of Philology* have been straws showing the way the wind was blowing. Mr. Linwood's pamphlet (1873) is of less importance; but Dr. Lightfoot's proposals of conjectural emendation at Col. ii., 18; Phil. ii., 1, and Gal. ii., 12, have been of great influence. In Germany such hands have handled it, and in such a spirit as would keep it in disgrace. The just remarks of Von Gebhardt (Leipziger Theol. Literaturz. 6, 23, p. 540) show, however, the advent of fitter influences.

† Amounting in all to some 62, as follows: Matt. xv. 30, xxi. 28-31, xxviii. 7; Mark iv. 28; Luke xi. 35; John iv. 1, vi. 4 [viii. 9]; Acts iv. 25, vii. 46, xii. 25, xiii. 32, 42, xvi. 12, xix. 40, xx. 28, xxv. 13, xxvi. 28; 1 Pet. i. 7, iii. 21; 2 Pet. iii. 10, 12; 1 John v. 10; Jude i. 5 *bis.*, 22 *ff.*; Rom. i. 22, iv. 12, v. 6, viii. 2, xiii. 3, xv. 32; 1 Cor. xii. 2; 2 Cor. iii. 3, 17, vii. 8, xii. 7; Gal. iv. 31-v. 1; Col. ii. 2, 18, 23, *bis.*; 2 Thess. i. 10; Heb. iv. 2, x. 1, xi. 4, 37, xii. 11, xiii. 21; 1 Tim. iv. 3, vi. 7; 2 Tim. i. 13; Phil. 9; Apoc. i. 20, ix. 10, xi. 3, xiii. 10, 15, 16, xviii. 12, xix. 13. Van de Sande Bakhuyzen, on the other hand, wildly asserts that about 200 passages have been already successfully emended by conjecture!

case printed the best attested reading, and relegated their emendations to the Appendix. By this means they have left the question just where it should rest; admitting the legitimacy of the method and indicating the passages where, in their judgment, there is need for it, they leave as questions open for discussion in each case: Whether there be a real necessity for it, and whether their attempted emendation is successful.

THE NEW TEXT.

The question which next claims our attention is: What is the character of the text which the new editors have made, on these principles? At once the easiest, briefest, and most satisfactory way of answering it will be through a collation of the new text with the editions of Tregelles and Tischendorf (which alone are worthy of comparison with it) in some one portion of the New Testament—from which we may be able to learn immediately the relation of the three to one another, and hence the comparative value of each. We avoid the Gospels in making choice of a section, both because that portion of the new text has been already pretty fully examined by others,* and because Dr. Tregelles' text cannot be thought equal to itself in the Gospels,† and hence the comparison would not be fair to him. Outside of the Gospels we choose at random the Epistle to the Ephesians; and add to the compared texts that which underlies the Revised English New Testament, on account of its inherent interest to us all.

If we take no account of differences in mere spelling, whether due to itacism or elision, or in punctuation,—so that we consider only the real differences; and as well take no account of brackets or margins, so that we may deal with the *preferred* text in each instance:—we may count some one hundred and sixteen cases in which one or another of these four texts differs from Stephens' *Editio Regia*. In some seventy-five per cent. of these cases, however, they all four agree in the change made, leaving only some twenty-five per cent. of the changes from the *Receptus*, or about one per cent. of the whole epistle, about which there is any difference of opinion among competent editors as to the reading. This result is worth our consideration; it furnishes a sufficient answer to both the charge that textual criticism tends to unsettle the text, and the fear that we can never attain a really received text. At this early stage, at least ninety-nine per cent. of the Epistle to the Ephesians has reached the stage of a really received text,—receiv-

* *E. g.*, by Dr. Sanday in the *Expositor* (last three Nos. of 1881).

† Through the inaccessibility when it was made of both **N**, and good editions of **B**.

ed by all competent critics. And this understates the case, since the number of passages where difference of opinion exists, has been largely increased by the admission of the Revisers' text to comparison and its habit of retaining even confessedly false readings, provided that they are such as cause no difference in a translation. If we omit such cases, we have, instead of thirty-one, only 23 cases where the editors differ, *i. e.*, less than twenty per cent. of the divergences from the Receptus. What the character of these is may be seen at a glance from the following list:*

PASSAGES IN EPHESIANS IN WHICH P. W. T. AND Tr. DIFFER AMONG THEMSELVES.

1. Eph. i. 14 † ὁ. P. W. Tr-mg. with A B F G L P etc.—ὁς T. Tr. W-mg. with N D E. K.
2. Eph. i. 15. τὴν ἀγάπην T, Tr, P-mg with (N^c) D F G 47 etc.—omit, P. W. [Tr-mg] with N* A B (P). 17.
3. Eph. i. 20. ἐνίργησεν P. Tr. W-mg with N D F G K L P etc.—ἐνίργηκεν T. W. Tr-mg. with A B etc.
4. Eph. i. 20. αὐτόν after καθίσας T. with N A 17 Syrr. Memph, etc.—omit P. Tr. W. with B D E F G K L P etc.
5. Eph. iii. 1. X. Ἰησοῦν P. W. Tr. with N* A B C D^c etc—omit Ἰησοῦν T. with N* D* F G.
6. Eph. iii. 9. πάντας P. Tr. W-mg. with B N^c C D F G K L P. etc.—omit T. W. P-mg. with N* A. 67.**
7. Eph. iii. 18. ὑψος καὶ βάθος P. W. Tr with B C D F G, etc.—transpose T. Tr-mg. W-mg with N A K L etc.
8. Eph. iv. 2. πρῶτοντος P with A D F G L O^b etc.—πράττοντος T. Tr. W. with B N C. 17.
9. Eph. iv. 7. ἡ before χάρις P. T. [W]. with N A C D^c E K etc.—omit Tr. with B D* F G E P* etc.
10. Eph. iv. 8. καὶ before ἔδοκεν P. Tr. [W.] with B N^c C* D^c K L P. 37 47 etc.—omit T, with N* A C² D* F G. 17.
11. Eph. iv. 16. ἐαυτὸν P. W. Tr. with B A C D^c etc—αὐτὸν T with N D* F G etc.
12. Eph. iv. 18. ἐσκοτισμένοι P with D F (G) K L (P)—ἐσκοτωμένοι T. Tr. W. with N B A 17 etc.
13. Eph. iv. 28. τὸ ἀγαθὸν before ταῖς P. with L etc.—after χερσὶν T. Tr. W with B N A D F G etc.
14. Eph. iv. 28. ἰδαίς (before χερσὶν) T. Tr. W-mg with N* A D F G K.—omit P. W. with B, N^c L etc.
15. Eph. v. 2. ἡμῶν P. T. Tr. W-mg. with N A D F G K L P Memph. Vulg. Syrr. etc.—ὑμῶν W. P-mg. with B. 37. m. Theb. Aeth. Victorinus.
16. Eph. v. 4. καὶ (before ἠρωζ.) P. Tr. W. with B N* D^c K L 17. 37. 47. Memph. Aeth. Clement—ἡ T. with N* A D* F G P Theb. Vulg. etc.
17. Eph. v. 4. τὰ οὐκ ἀνίκοντα P. with D F G K L 37 47 etc.—ἂ οὐκ ἀνήκεν T. Tr. W with B N A P (17) etc.

* In this list P. (= Palmer) denotes the Revisers' text; W, that of Westcott & Hart; T that of Tischendorf; and Tr. that of Tregelles.

† Eph. i. 1. presents a peculiar case; the editors differ as to their actual treatment of it, but mean about the same thing by their very divergences. Hence it is omitted here.

18. Eph. v. 14. Ἐγχεῖται P. with 17. 37. Orig. etc.—ἐγχεῖτε T. Tr. W. with B \aleph A D F G K L P etc.
19. Eph. v. 15. πῶς after ἀκριβ. P. T. W. Tr-mg. with B \aleph^c 17. Memph. Orig.—before, Tr. with A. \aleph^c D rel.
20. Eph. v. 19. τῆ καρδία P. T. W. with B \aleph^c —ἐν τ. κ. Tr. with (A. \aleph^c D F G P 67) K L etc.
21. Eph. v. 22. ἰποτασσέσθωσαν Tr. [Tr-mg]. with \aleph A P 17. Memph. Vulg. Arm. Aeth. etc.—omit, P. T. W. with B. Clem. Hier.
22. Eph. v. 28. καὶ οἱ ἀνδ. P. Tr. [W] with B A D F G P 17. Memph. Vulg. etc.—omit καὶ. T. with \aleph K L 37. 47. Pst. Arm. Aeth. etc.
23. Eph. v. 31. τοῦ πατ. P. T. [W] with \aleph A D^c K L P. 17. 37. 47.—omit τοῦ. Tr. with B D* F G etc.
24. Eph. v. 31. τὴν μητ. P. T. [W] with \aleph A D^c K L P etc. as above—omit τὴν, Tr. with B D* F G etc.
25. Eph. v. 31. πρὸς τὴν γυναῖκα P. W. with B \aleph^c D^c K L (P) 47. Orig.—τῇ γυναίκε. T. Tr. W-mg. with \aleph^c A D* F G. 7. 37 etc.
26. Eph. v. 31. add αὐτοῦ P. W. Tr. with MSS. mss.—omit T with \aleph . Marcion.
27. Eph. vi. 5. τῆς (before καρδ.) Tr. W. P. with MSS. mss. etc.—omit. T. with \aleph . and 13 mss. Orig. Catena-Cr.
28. Eph. vi. 6. τοῦ (before χριστοῦ) P. with D^c L K 37.—omit T. Tr. W with \aleph B A D* F G etc.
29. Eph. vi. 8. ὃ ἔαν P with \aleph^c (L) 47 etc—ὃ ἂν Tr. with (A) D F. G. P. (37) Vulg. (Memph) etc.—ἔαν T. W. with B. L*. Peter of Alex.
30. Eph. vi. 8. κομίζεται P. with \aleph^c D^c K L 17. 37. 47.—κομίσεται T. Tr. W with B \aleph^c A D* F. G. etc.
31. Eph. vi. 21. καὶ ὑμεῖς after εἶδητε P. W. with B K L 37. 47. Syrr. Arm. Aeth. [Memph]—before, T. Tr. W-mg. with \aleph A D F G P etc.

The eight cases (8, 12, 13, 17, 18, 28, 29, 30) where P stands alone are all such that the variants would not be represented as variants in a translation, and, therefore, on which the Revision Committee refrained from expressing an opinion;* they ought to be omitted from our count. Of the rest, W stands alone in one case (15) and alone with the Revisers in five more (1, 2, 14, 25, 31); that is, T + Tr, stand against W in six cases. These will naturally be looked upon as *the* test-passages, and the question asked whether W is here justified in deserting the *consensus* of editors. Put the evidence for the rejected readings in these cases together, and we arrive at the principle of action. Here they stand in a column :

- No. (1). \aleph D E K, etc.
 " (2). (\aleph^c) D F G 47, etc.
 " (14). \aleph^c A D F G L P, etc.
 " (15). \aleph A D F G K L P.
 " (25). B D* F G.
 " (31). \aleph A D F G P, etc.

We see at a glance that the rejected reading—however much or however little is added to them—has yet always on its side the *dis-*

* Compare Revised N. T., Oxford ed., pica demy 8vo, p. xiii.; Palmer's edition of its text, p. 1; Scrivener's edition of its text, p. vi.

inctively Western documents; and moreover, that this is the only thing the rejected groups have in common. We see at once the principle involved, and the correctness of the procedure. How fully the new text is governed by the consistent application of this genealogical principle will appear on noting the authority which has swayed it in all the 31 cases. Except in such cases as 13, 18, 26, 27, 28, 30, where the Western text unites with the Neutral against Alexandrian or later corruption, and where, therefore, the accepted text rests on a peculiarly sound basis,—W rejects the reading supported by the Western documents throughout—with only three exceptions (4, 7, 22), and two of these (7, 22) it accepts with some doubt. The groups thus rejected include nearly every possible variety of further attestation, short of the union with the Western of the whole Neutral group, and agree only in this one particular,—that they all embody the specifically Western documents. On a calm consideration we can feel no doubt as to the correctness of the decision given; and indeed, can entertain doubt as to choice of the new editors only in one or two of the three exceptions they have made to their usual rule (4, 7, 22).

On the other hand, Tischendorf stands alone nine times (4, 5, 7, 10, 11, 16, 22, 26, 27), and Tregelles also nine times (3, 6, 9, 19, 20, 21, 23, 24, 29). A single glance at Tischendorf's peculiar readings shows that they are all probably due to overestimation of \aleph . The attestation for them runs all the way from \aleph alone among MSS. (as in 26; see also 27) up to \aleph A C² D F G (10); and in most cases it seems certain that W. has rightly sided with Tregelles in rejecting them (*e. g.* in 26, 16, 11, 10, and 5). Where Tregelles stands alone, it is always through following a numerical majority of old documents and a combination of Western documents with other primary uncials. Thus we have Western documents in 3 supported by \aleph ,—in 6 by B C,—in 9, 23, 24 by B,—in 19, 20 by A,—in 21 by \aleph A. This would have been sound procedure provided that these uncials were really independent of the Western group,—and it was on this assumption that Tregelles so proceeded,—but since they all have an element of Western corruption (in Paul), manifestly to act on such a rule is simply to betray the text into the hands of Western error. We cannot, therefore, fail to conclude that W. has rightly sided with Tischendorf against Tregelles in all these passages.

If the Epistle to the Ephesians is a fair sample of the new text,*

* That it is a fair sample of the text in Paul's epistles may be gathered from a com-

therefore, it is pretty evident that in the newly-published edition we have the best considered and most carefully framed—and therefore, also, the most perfect—text which has yet been given to the public. It is, consequently, a matter of deep gratulation, that the company of Revisers for our English New Testament not only had this text in their hands, but seem to have—not, indeed, mechanically, but with intelligent coincidence of judgment,—followed it pretty closely,—just how closely in this epistle is somewhat remarkable. A glance at the list given above will show that in only three cases would one have to alter Westcott and Hort's text to obtain the text which underlies the present Revised New Testament (3, 6, 15). It is right to mention, however, that if our sample were not Ephesians, but the Gospel of Matthew, this resemblance (unfortunately) would not be quite so striking. Dr. Sanday* compares the various editions in 195 selected passages out of Matthew, and finds that the Revisers agree with Westcott and Hort in 146 of these, in about 100 of which there is practical agreement among the editors. In other words, the Revisers agree with Westcott and Hort in 46, and disagree with them in 49 disputed cases. It will be well for us to note the MS. attestation of these readings. B and ∞ stand alone in 26 cases, and Westcott and Hort follow them each time, but never alone among editors; Tischendorf accepts them 23 times, Tregelles 13, Weiss 25, and even McClelland at least 12. So also the readings supported by B plus some other one MS. (not ∞) amount to some 14 in this list. Again Westcott and Hort in company with a larger or smaller combination of editors, but never alone,

parison of the following table of the passages in which the three great editions differ in the course of I Cor. i.-iv. :

No.	Passage.	Rejected by	Evidence for rejected reading.
1	i. 1	W	B D F G 37.
2	2	T. Tr-mg. W	A N D ^b L P 17, 37, 47.
3	4	[Tr-mg] W	A N ^a C D F G L P, etc.
4	14	T. W	A N ^c C D F G L P. 17, 37, 47, etc.
5	28	[W]	A N ^c C* D* F G, 17.
6	ii. 1	W	B N ^c D F G L P 17, 37, 47.
7	2	Tr. W	A N F G L 47.
8	9	Tr. W	N D F G L P 17, 37, 47.
9	10	Tr-mg. W	A N C D F G L P 17, 47.
10	15	[Tr] W	A C D* F G.
11	iii. 16	Tr-mg. W	B P 17, 37.
12	iv. 13	Tr-mg. T. W	B N ^c D F G L 37, 47.
13	14	T. Tr-mg. W	B D F G L 37, 47.
14	17	Tr. W	A N* P. 17.
15	17	T. [W]	A B D ^c L P 47.

* *The Expositor* for October, 1881.

accept them all; Tischendorf accepts 7 of them, Tregelles 9, Weiss 9, and McClelland 7. Singular readings of B amount to some 6; followed again each time by Westcott and Hort, and again always with the consent of some other editor, although in three cases with only one (*i. e.*, Lachmann once and Tregelles twice); in two cases they have the support of two (Tregelles and Weiss, and McClelland and Weiss); and in one case of the majority. From this it is evident that the new editors have not been absolutely singular in their devotion to their favorite documents.

This will appear in a still stronger light if we will try their work by another test. The passages from Matthew were *selected*, and it is mere accident that B is always followed in them. A very clear notion, both of the consistency with which the new editors follow the documentary attestation as interpreted by genealogical evidence and internal evidence of groups, and also of the readiness with which internal evidence is heard and permitted to overweigh all external testimony, may be gained by trying the new text by the list of *monstra* supported by B \aleph , or at least by B, given by Dr. Scrivener,* and constantly condemned by him with such epithets as "transparent (or 'frigid,' or 'feeble') gloss," 'intolerable,' "against common sense." In 27 (out of 38) of these cases Westcott and Hort print the reading in their text, although in five of them between brackets, and in three with the suspicion of its being a primitive error. In eleven cases they reject the reading, although in three of these (only one of them, however, in the Gospels) it is read by B \aleph , with more or less support (in one case by B \aleph C L U T, etc., and in another by B \aleph D H—this last obelized), in two by B D, and in five by B plus secondary authorities, and only in one by B alone. They have the support of either Lachmann, Tregelles, or Tischendorf, or of two or of all of them, in 14 out of the 27 cases, in which they accept the *monstra*, whereby it is again shown that their judgment is not so peculiar in such cases as we are sometimes invited to suspect. These facts are consistent with no other supposition than this: these editors follow the reading which the best MSS. commend, not because they are "worshippers" of B or of B \aleph , but only because they follow tested external evidence more consistently than any previous editor when it is not undoubtedly in conflict with internal evidence, and because they cannot in every case bring themselves to agree with the subjective school as to the true force and bearing of the internal evidence. Clearly, they would reject the 27 accepted *monstra* as

* Page 471.

readily as they have the 11 rejected ones, if only it could be proved to them that they were *monstra*. Until that is done they rightly judge that the best attested reading must stand. In other words, their practice gives us distinctly to understand that while they stand ready to set aside any external testimony on the clear demand of combined paradiplomatic and internal evidence, they do not stand ready to reject the guidance of all three forms of testimony, and subject the text to guess-work reconstruction, or to corruption by readings whose only support is that they "find us." The effort they make—and the tendency of their example—is to oppose the intrusion of arbitrary readings from whatever source they come, and to make up a text wholly on evidence, and not one variety of evidence solely, but with a wise and consistent regard to all the sources from which testimony comes to us.

After having given thus a calm review of the work of the new editors, we feel bound, in closing, to express our conviction of its great value very clearly. We cannot doubt but that the leading principles of method which they have laid down will meet with speedy universal acceptance. They furnish us for the first time with a really scientific method; they reduce guesswork reconstruction to the narrowest limits, and substitute for it a sound inductive procedure. The individuals who will feel called upon to oppose them will pass quietly away and leave no successors. And it is to be hoped that scholars will quickly recognize the lines of investigation which promise well for the advancement of the science, and abstracting themselves from all else, throw themselves with energy into the closer study of the relations between the documents which we already possess, or which may from time to time be dragged out of hiding and given to the public. Nor can we hesitate to say that the text which the new editors have given us is, in our judgment, the best and purest that has ever passed through the press, and, for the future, must be recognized as the best basis for further work. It pretends to be "no more than an approximation to the purest text that might be formed from existing materials"; much certainly "remains to be done for the perfection of the results now obtained"; and many readings now admitted which rank in probability in present light only a very slight shade above rejected readings given in the margin, the future must (in some few cases the present may) re-examine and attempt to point out more clearly the true place for. All this is not strange: practice must ever lag behind principle. But now, at last, the *truth has been touched*,

and although he—the consummate critic—is still hidden in the womb of the future, who can say :

“ Man clomb until he touched the truth, and I,
Even I, am he whom it was destined for,”

still the praise due to those who have first made it possible to look for his coming out of the immediate future is by no means small. And, in the meanwhile, the teacher and preacher alike may rest upon and use the text already in hand with the calm consciousness that substantially the autographic text is before him, and that probably all future criticism will not result in throwing doubt on more than one word of it in a thousand. If, as Bentley says, the true text is competently (for the ordinary purposes of life and teaching) exact in the worst copy extant, how much more is this true of the best edition yet framed? Let us all join heartily in the prayer with which Dr. Hort closes the Introduction: “that whatever labor” he and Dr. Westcott may “have been allowed to contribute toward the attainment of the truth of the letter, may also be allowed, in ways which must for the most part be invisible to them, to contribute toward strengthening, correcting, and extending human apprehension of the larger truth of the Spirit.”

BENJAMIN B. WARFIELD.