MORE "SECOND THOUGHTS ON THE MAJORITY TEXT"
A Review Article
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Daniel Wallace, "Some Second Thoughts on the Majority Text,"
Bibliotheca Sacra, 1989, 146:270-290.

As president of the Majority Text Society [in 1990] I received a number of letters asking for a reply to this article. Although discussion of the Majority Text is certainly welcome, we do feel that Prof. Wallace's readers have not been well served. In a personal communication the author assured me that in writing his article it was his intention to be evenhanded. Granting his sincerity, we are then obliged to conclude that he has not really understood our position, so I venture a few remarks by way of clarification.

In the very second paragraph I am misrepresented. Chapter 6 of my book[1] makes clear that all of Burgon's "Notes of Truth" need to be applied, not just "majority" (Burgon's "Number"). Burgon himself never argued that a simple majority of MSS was sufficient, by itself, to determine the text. Let me illustrate with an example. In Luke 3:33 some 60% of the MSS (a clear majority, therefore) insert Joram between Aram and Hezron.[2] But, out of 27 extant uncials only nine read Joram; 18 do not, and they are supported by the three earliest Versions. The earliest MS to include Joram is from the VIII century—all earlier MSS lack it. In terms of Burgon's "Notes of Truth," Joram wins in "Number" but loses in "Antiquity", "Variety" and "Continuity". I think Burgon would agree that "Joram" should be regarded as an interpolation.

The supposed "Dallas" connection seems scarcely relevant. Every Greek course I took at Dallas Theological Seminary was based on the "critical" text. I derived the beginnings of my own position from reading Burgon, who obviously could owe nothing to "Dallas". Such contemporaries as James Borland, Peter Johnston, William Pierpont, Maurice Robinson and Jakob van Bruggen similarly owe nothing to "Dallas".

On page 274 Wallace correctly points out that neither Hodges nor van Bruggen espouse mere majority. In fact, I am not aware of any "Majority Text" theorist who does. In preparing their Greek text Hodges and Farstad followed Kurt Aland's lead in using the term "majority", but it must be understood that this refers to a large or "overwhelming" majority.

Footnote 20 faults Hodges for contributing to the misconception that Majority Text equals *Textus Receptus*. In the sixties it was natural to speak and write of the *Textus Receptus* because that was what we had to work with. In the seventies, when Hodges and Farstad began to seriously consider editing a Majority Text, it became feasible to start speaking and writing of such a text. In the eighties and nineties it is no longer acceptable for an informed person to confuse or equate the two texts.

"Text and Theory"

On page 276 Wallace misrepresents me again. He equates my "Original" with the Hodges-Farstad Majority Text, even though my book appeared five years before that text. By "Original" I mean the wording of the Autographs, which is certainly not identical with the H-F MT. In my opinion the H-F text has 16 errors in the Pericope (John 7:53-8:11) and possibly as many as 300 in Revelation, besides having insufficient basis for deviating from the TR in dozens of other places. On the basis of the evidence I have seen so far, once all the MSS have been collated and carefully analyzed I imagine that the TR will need correcting in about 1,500 places, most of them being minor differences. Footnote 25 is misleading. While the TR has some 1,500 errors, UBS[3] has some 6,500 errors, in my view. I repeat and

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[1] Wilbur Pickering, *The Identity of the New Testament Text* (Nashville: Thomas Nelson Publishers, revised ed., 1980).

[2] These statistics come from *The New Testament in Greek: The Gospel According to St. Luke*, Vol. I, ed. The International Greek New Testament Project (Oxford: Clarendon Press, 1984, p. 74).

insist that the errors in UBS[3] are more serious (some overtly damaging) than those in the TR, as well as being four times more numerous.

Wallace's discussion of the shorter reading canon (pp. 277-78) is useful. By way of clarifying my own position, I cheerfully affirm that the Byzantine textform has hundreds of "shorter readings". I would but insist that a "shorter reading" is to be adopted on the basis of objective evidence, not because it happens to be shorter. It is demonstrable that in transcribing texts accidental omission is much more frequent than accidental addition (about the only way of "adding" accidentally is to copy something twice over, but you have to be drowsy not to catch yourself at it). Further, I would argue that even when deliberate, omission should still be more frequent than addition. If there is something in the text that you don't like, it draws your attention and you are tempted to do something about it. Also, it requires more imagination and effort to create new material than to delete what is already there. (Material suggested by a parallel passage could be an exception.) Further, it is demonstrable that most scribes were careful and conscientious, avoiding even unintentional mistakes. Those who engaged in deliberate editorial activity were really rather few, but some were flagrant offenders (like Aleph in Revelation).

Footnote 39 calls for comment. Of course harmonization is a secondary feature, obviously, but it most certainly is not an "undeniable fact" that "harmonization occurs much more often in the majority text than in the Alexandrian text." I would say that allegations to that effect are mainly based on superficial observation and presuppositional bias against the Byzantine text. W. Wisselink's exhaustive study of assimilation[1] shows that Wallace's allegation is not demonstrable, let alone an "undeniable fact". I do indeed deny it! Wallace affirms the "fact" of "logical guidelines to determine whether or not a reading is a harmonization," which supposedly renders such determining "less subjective". The presuppositions that orient such "guidelines" will presumably be subjective, and their subsequent application does not make them objective. Wallace refers the reader to his unpublished paper, "Some Reflections on the *Majority Text*," wherein he has an equally unsatisfactory treatment of "conflation" (p. 17).

"Hodges versus Hodges"

On page 279 we come to the most crucial part of Wallace's article. "Hodges versus Hodges" discusses alleged "inherent contradictions" within Majority Text theory as practiced by Zane Hodges, the prime mover in the H-F Majority Greek Text. This section will receive close scrutiny.

First, defenders of the MT (or even the TR, for that matter) have never defended "shorter or longer readings *in toto*"—we do not determine the text on that basis. We have indeed challenged the inordinate preference for shorter readings espoused by W-H and their progeny.

Next, Wallace has seriously misunderstood the use that Hodges makes of the argument from statistical probability, in particular the exercise conducted by his brother, David Hodges. Wallace correctly quotes from the H-F preface: "Any reading overwhelmingly attested by the manuscript tradition is more likely to be original than its rival(s)." The word "overwhelming" is crucial. But Wallace makes an egregious mistake in interpreting David Hodges's "hypothetical genealogy". What Wallace calls the "fifth, sixth, and seventh generations" together make up the fifth generation, as the diagram clearly states (there wasn't enough room to put all 81 circles side by side on the page). His mistake leads Wallace to ascribe gratuitous motives to David Hodges, who simply conducted an impartial experiment, as anyone familiar with the science of statistical probability can verify.

Wallace's discussion of "reading" and "text" (pp. 280-81) misses the mark to a surprising extent (footnote 50 borders on the ridiculous). Hodges neither "confuses" nor "equates" the concepts "reading" and "text"—footnote 49 is irrelevant. To fully unscramble Wallace's misstatements would take too much space, so I will just try to sketch the true state of affairs, as I see it.

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[1] Willem Wisselink, *Assimilation as a Criterion for the Establishment of the Text* (4 vols., Kampen, Netherlands: Uitgeversmaatschappij J.H. Kok, 1989).

The Greek New Testament According to the Majority Text[1] has about 140,000 words. Probably less than half enjoy 100% attestation from the extant Greek MSS, but fully 80% of the words have at least 99% attestation. For less than 2% of the words does the attestation fall below 80% of the MSS—in every case I am referring to the attestation for the wording in the H-F *Majority Text*. By way of comparison, there is a discrepancy of about 8% between H-F and UBS[3], or over 11,000 words. For some 8,000 of those words the UBS editors followed less than 10% of the MSS. If we add the W-H text, the TR and every other printed edition of the Greek NT, we will probably find unanimous agreement on between 89-90% of the words—so the whole textual debate surrounds about 10% of the words of the NT. Now then, when H-F refer to "overwhelming" attestation they mean 85% or more of the MSS, or a ratio of over 5:1. One or more of the several printed Greek texts have challenged some 8-9% of the text (a. 12,000 words) even though that 8-9% has over 85% attestation. It is to that 8-9% that David Hodges's exercise is addressed.

David Hodges deliberately weighted his formula in favor of the "error". His experiment is purely hypothetical and should not be rigidly applied to any specific set of variants—it is ancillary to MT theory. David Hodges argues that it is "highly unlikely", statistically speaking, that an error should come to enjoy a 75% attestation (from the extant Greek MSS), let alone 85% or more. Of course it is theoretically possible that one or another did, here and there, but to claim this possibility thousands of times, for 8% of the total NT text, is indefensible.

Wallace's footnote 51 and comments about kinds of variation illustrate yet again his misunderstanding of Hodges's formula. An "error" refers to any kind of variant and of course there can be multiple variants in a set—the statistical presumption is that the more recently an error is introduced the lower are its chances of enjoying wide attestation. Evidently there may be exceptions.

The predictable ways that a text may be corrupted are not "countless" (fn. 52), and precisely because they are predictable, scribes and correctors have done their work down through the centuries with those "ways" in mind. Many actual errors have indeed been corrected to the true reading, while many supposed "errors" have been turned into actual ones. Although David Hodges's experiment was confessedly mechanical, of necessity, it was weighted in favor of the "error" because one can demonstrate that most scribes were careful, avoiding even unintentional errors, while others officiously tried to improve upon their exemplars. Wallace's criticism again misses the mark. His claim that "statistics and stemmatics tend to cancel each other out" (fn. 53) reflects another misunderstanding. The feasibility and procedure for stemmatic reconstruction deserves careful attention.

"Majority versus genealogy"

Evidently genealogical relationships of some sort must exist among the MSS. Due to the prevalence of "mixture" and the uneven sprinkling of survivors from the earlier centuries, the tracing of linear descent for individual MSS would appear to be beyond our reach. But the grouping of MSS on the basis of a shared "profile" of variants is both viable and legitimate—also necessary. And it is possible to posit stemmatic relationships between MS groups on the basis of objective criteria—in this way we can trace the history of individual readings, to a considerable extent. Which is why H-F say: "Final decisions about readings ought to be made on the basis of a reconstruction of their history in the manuscript tradition" (quoted by Wallace, p. 282).

Although on page 274 Wallace correctly noted that Hodges does not espouse mere majority, on page 282 he writes, "the distinct impression arises that he [Hodges] is convinced that a reconstructed family tree will vindicate majority rule." "Majority rule" is a straw man. On page 279 Wallace quoted from the H-F Preface: "Any reading overwhelmingly attested by the manuscript tradition is more likely to be original than its rival(s)" (p. xi). Notice "overwhelmingly" and "more likely"—Wallace has misrepresented Hodges; his idea that stemmatics should "validate" majority rule is gratuitous. This misconception colors Wallace's whole discussion in this section.

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[1] Zane Hodges and Arthur Farstad, *The Greek New Testament According to the Majority Text* (Nashville: Thomas Nelson Publishers, 2nd ed., 1985).

When Wallace turns to Zane Hodges's reconstruction of the text of John 7:53-8:11, wherein 16 of 30 contested readings (as registered in the critical apparatus) are only supported by a minority of MSS (sometimes less than 30%), he is "startled" (p. 283).[1] To my mind, this is the first of only two sections of Wallace's critique that have some validity. Hodges preferred the third largest MS group, M^6 (itself seriously divided), on the basis of internal considerations. In my view this is where he went astray. His reconstruction is at variance with the objective stemmatic criteria. I have a 13 page paper (unpublished) that expounds the problem thoroughly. I would say that M^5, M^6 and M^7 reflect three independent lines of transmission dating back at least to the III century. (The other four groups derive from a single source which is a mixture based on the three main groups: $M^{5,6,7}$.) M^7 is always joined by either M^5 or M^6 and thus is always in the clear majority, except for the one point where there is a four-way split in the MSS and there is no "majority" reading. When the stemmatic evidence is properly interpreted, with reference to this pericope, stemmatics and "majority" happen to agree—on the basis of both stemmatics and majority M^7 is vindicated as the correct text.

Although M^7 is always in the majority (with the one exception), it must candidly be stated that 5 times the majority is less than 60%, and 19 further times less than 70%. It thus appears that the pericope had an unusually troubled passage through the centuries of transmission. In fact, so far as I know, only one portion of the NT had a more difficult time of it—the entire book of Revelation. It is this circumstance that essentially invalidates Wallace's critique of MT theory *vis-a-vis* the H-F text of the Apocalypse. For some 150 variant sets (variation units) there simply is no majority reading, and for some 250 more the majority is less than 60%.

Wallace has been misled by his sources when he supposes that MT theory "makes by far its best case" in the Apocalypse. The opposite is true—Revelation is the only extended passage with dozens of variant sets where no single reading has a majority. A basic split in the Byzantine stream took place in the III century (if not before)—since Codex Aleph conflates both strands they must be earlier than it is. For all that, except for some 200 places the majority principle works even in Revelation. For those 200 places it is precisely stemmatics, properly applied, that will show us the correct readings.[2]

Wallace is correct when he says that we do not speak of a Byzantine text-type, as such (fn. 59); it is a wide stream containing a number of "text-types", if we must use the term. The work of F. Wisse is especially instructive here.[3] He collated 1,385 MSS in Luke 1, 10 and 20. Analyzing the results he distinguished 15 major groups of MSS plus 22 lesser groups. One of the 15 is the "Egyptian", made up of some 10 MSS. Most of the rest make up the Byzantine stream. In Luke we must speak of at least 15 "text-types", all but one of which really fall within the Byzantine tradition. In the Gospels the true text will be massively attested by both majority and stemmatics.

Next to Revelation, the canonical books experiencing the most turbulence appear to have been the General Epistles. Kurt Aland has rendered an important service by making available virtually complete collations for 98 variation units scattered throughout those seven books.[4] Two of the variant sets lack a clear majority reading (one has 50%); two others have less than 60%—the differences are quite minor. For the most part there is massive attestation; only seven fall below 70%. And yet, no two of the 98 variant sets display an identical distribution of MSS. It follows that there was no "stuffing the ballot box" and there must be a variety of MS groupings, much as Wisse found in Luke. Here too, the true text is massively attested by both majority and stemmatics.

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[1] Wallace goes on to make a seriously misleading statement: "In other words for the *pericope adulterae*, the *Majority Text*, in half its readings, is a minority text." Actually, 95% of the words in the H-F text (Jn. 7:53-8:11) have majority attestation, and for most of them it is a heavy majority.

[2] As in John 7:53-8:11, I take issue with Hodges's stemmatic reconstruction here too—his preference for M^a seems to me to be at variance with objective stemmatic criteria. However, M^a is easily the largest MS group in Revelation (though not a majority) and internal evidence played less of a part in Hodges's stemmatic reconstruction here.

[3] Frederik Wisse, *The Profile Method for Classifying and Evaluating Manuscript Evidence* (Grand Rapids: Wm. B. Eerdmans, 1982).

[4] Kurt Aland, ed., *Text und Textwert der Griechischen Handschriften des Neuen Testaments* (Berlin: Walter de Gruyter, 1987).

Wallace sets up a straw man when he changes Hodges's "reasonably regular fashion" into "normal rate" with reference to the copying process (pp. 285-86). Our claim has to do with manner, not rate. The copies were made with reasonable care and honesty, whether or not the activity was concentrated in particular places or times. For example, the copies of Revelation at the Athos monastery exhibit a remarkable variety of text, even though they were produced at about the same time. It seems evident that the scribes reproduced the exemplars faithfully, even though those exemplars were at variance. Hodges's simplified stemma obviously omits many intermediate nodes and most of the grass roots pairings and groupings, but in the end his M^a node, for example, does represent some 75 MSS; further, it is possible to posit the presumed precise wording of the single parent MS, and that MS must date back to at least the III century (since Aleph has conflations based upon it). Of course this is no different in essence from "Hort's argument that a single archetype stood behind the Byzantine text-type," but Wallace's conclusion that "it destroys any notion of a normal rate of copying" (p. 286) is plainly false, a non sequitur. I agree with Hort that a single archetype stands behind the Byzantine textform—the Autograph, not his "Lucianic recension".

"Majority versus majority"

This section (p. 286) puts considerable strain on our innate civility. First, the quote from Kilpatrick is infelicitous: there is evidence for the Byzantine text before the IV century, and the extant materials from the II and III centuries do not tell us what the dominant text may have been.

Next, the statement, "the Byzantine text-type, as far as the extant manuscripts demonstrate, did not become the majority until the **ninth** century" [emphasis his], is rather strange. Wallace was tripped up by K. Aland's misleading "category" system.[1] In fact, the uncials for which Aland offers statistics line up as follows.[2] The V century: in the Gospels A and Q are clearly Byzantine while W and C are marginally so, only 0274 is clearly Egyptian, while D is marginally "Western"; the Byzantine text is clearly ahead in the Gospels, while the Egyptian text wins in Acts, Paul and the General Epistles. The VI century: in the Gospels N, O, P, R, 042 and 043 are all Byzantine, only Z and 040 are Egyptian; the Byzantine text retains its hold on the Gospels and moves ahead in Acts. In the VII and VIII the Byzantine continues to lead, and becomes a tidal wave in the IX. The above statements relate only to extant MSS—in reality I assume that the Byzantine text was dominant for all parts of the NT in every century.

Consider the massive Byzantine majority in the IX century. Where did all those MSS come from? Out of 27 Byzantine MSS or content segments (Gospels, Pauline corpus, etc.), with reference to Aland's variation units, eight are over 95% 'pure' Byzantine, ten are over 90% pure, and another six are over 80% pure (compared to Codex B that is only 72% Egyptian in the Gospels). Where did these 24 MSS or segments get their Byzantine content? Since they are all distinct in content they were presumably copied from as many separate exemplars, exemplars of necessity earlier in date and also Byzantine.[3] (Surely Wallace will not attempt to argue that all those IX century MSS were not copied from anything, but were independently created from nothing by each scribe!) And what were those exemplars copied from? Evidently from still earlier Byzantine MSS, etc. It follows that a massive majority in the IX century presupposes a massive majority in the VIII, and so on. Which is at least partly why scholars from Hort to Aland have granted that the Byzantine tradition dominated the transmissional history of the NT text from the IV century on.

Then, Wallace's comments on the Latin Vulgate tax our credulity. Why don't we count the 8,000+ extant Vulgate MSS? For the same reason we don't count the multiplied millions of copies of the King James Version. All the King James Bibles in the world derive from a single seventeenth century source—taken together they represent a single witness from that century. All the Latin Vulgate MSS boil down to a single IV century witness—they are not witnesses to the original at all, only to whatever Greek wording

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[1] Kurt and Barbara Aland, *The Text of the New Testament* (Grand Rapids: Wm. B. Eerdmans, 1987), pp. 155-159.
[2] *Ibid*, pp. 106-125.
[3] The only IX century MSS that might be siblings are F and G (010 and 012), and they are marginally Egyptian—along with Θ they are the only IX century uncials that are not Byzantine.

Jerome chose to follow. Jerome's Greek text, as best we can identify it, is a significant IV century witness, but only one.[1]

Of course it is true that not every Greek MS is a witness to the original either. If we can accept Wisse's 15 major groups and 22 lesser ones as valid, for the sake of the argument, then we have reduced 1,296 MSS (1,385 minus 89 "mavericks") to 37 exemplars. These must tentatively be considered independent until we can objectively posit clusters of them as deriving from common earlier exemplars. Wallace's repeated mention of "majority rule" makes me suspect that he does not really want to understand our position. When Hodges speaks of an "overwhelming" majority (85% or more) he reflects our conviction that for a reading to command such attestation it must also control the genealogical tree—it dominated the stream of transmission. To follow such attestation is the best we can do until all the MSS are collated and analyzed—Wisse has provided us all with an excellent example in his work on three chapters of Luke.

Wisse's conclusions also illustrate why a "Greek Ebla" (fn. 77) would pose no particular threat to our view—such a cache of MSS would only shed further light on the history of the transmission of the text. Wisse's 37 groups would doubtless require some modification, but the general picture would probably not be greatly altered. As a matter of fact, one wonders why after fifteen years we still have been given no specifics about those MSS at St. Catherine's Monastery—could it be that they provide an inconvenient support for the Byzantine text?

Footnote 75 is a curiosity. As of May, 1988, Aland's Institute had assigned 70 MSS to the IX century, 145 to the X, 441 to the XI, 588 to the XII, 573 to the XIII, 539 to the XIV, 249 to the XV, and 140 to the XVI.[2] In the XII century Koine Greek would be even less of a living language than it was in the IX. And yet, almost 600 MSS survive from the XII. There must have been at least that many, and probably a lot more, produced in the IX. It follows that the vast majority were indeed worn out, but not before they were copied. Those that survived were probably not considered to be sufficiently pure for daily use, and were therefore relegated to the archives. The total lack of "Western" MSS after Codex D most probably means that it was neither used nor copied—there were no "Western" MSS to wear out. Indeed, Aland now refers to "the D text" rather than the "Western" text. The trifling number of MSS that may reasonably be called "Alexandrian" argues for a rather slim stream of copying within that tradition. Actually, who would use them? After the III century who in Egypt or Italy was fluent in Koine Greek? Supply and demand operates in the Church as much as elsewhere. And now we may move on to the next section.

"Genealogical method ultimately dependent on internal criteria"

Here we come to the second section that I consider to have some validity (pp. 287-89). Wallace correctly registers Hodges' earlier strictures against the use of internal criteria, but his comments about Hort *vis-a-vis* the genealogical approach miss the mark. Hort never really applied genealogy to the MSS—he just talked about it.

Then Wallace quotes Hodges's description of a "valid stemma", which ends with an appeal to internal criteria. I agree with Wallace that Hodges's "noticeably superior" differs little in essence from Hort's "morally certain or at least strongly preferred" (p. 288). It seems to me that Hodges has stumbled into an inconsistency, but if the appeal to internal evidence is excised from his stemmatic model the remainder is objectively defensible.

Wallace misses the point when he quotes from E.C. Colwell's important critique of Hort's "genealogical method". Hort's model dealt, theoretically, with the descent of manuscripts, and Colwell's critique also

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[1] Wallace's comment about the textual affinity of the Vulgate is also misleading. The Vulgate agrees with the Majority Text well over half the time, with reference to variation units, and has what might be called strictly "Western" readings less than 20% of the time.

[2] K. and B. Aland, *The Text*, 2nd ed., 1989, p. 82.

focuses on manuscripts. Hodges's model focuses on "the descent of the **readings**" [emphasis added].[1] Wallace has misunderstood our position, again.

"Summary"

In response to Wallace's "Summary" I will only address the five points under "Hodges versus Hodges." (a) Wallace completely misunderstood the nature and thrust of David Hodges's statistical demonstration; in consequence his discussion of it is hopelessly confused and irrelevant. (b) The main difficulty with his experiment in stemmatics is that Zane Hodges imported internal evidence into what should be an objective exercise; in Revelation there are some 150 variant sets with no majority reading. (c) Wallace's "normal rate of copying" is a straw man; Hodges nowhere incorporates "rate" in his model. (d) Wallace has seriously misunderstood the essence of Majority Text theory, which is not predicated on the mere counting of "noses". (e) Hort and Hodges both imported internal evidence into their practice of "genealogy", but not in the same way.[2] If Hodges would but remove internal evidence from his stemmatic model, I believe he could escape the only valid criticisms that Wallace has lodged against him.

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[1] Hodges and Farstad, p. xxv. In passing I venture the opinion that Colwell would probably take exception to the use that Wallace has made of his article. Colwell was a prime mover in the development of the "Claremont Profile Method"—I rather imagine that he would grant the validity of a stemma based on variant profiles.

[2] If I understand Hodges correctly, where there is "overwhelming" attestation (85% or more), internal evidence will rarely be impressive enough to challenge the external evidence. Since fully 95% of the total NT text enjoys such "overwhelming" attestation, and for less than 2% of the total text does the attestation fall below 80%, Hodges would appeal to internal evidence mainly where the MS evidence is seriously divided. Further, his "genealogy" is based on exact collations (Hoskier), or at least a sampling of a large number of MSS (von Soden). In contrast, Hort's "genealogy" was speculative and he used internal evidence to reject the witness of the MS consensus—for fully 6% of the total NT text he rejected majority attestation of over 85% and frequently rejected attestation of over 99%.