

Critique of “The Marginalia of Codex Vaticanus: Putting the Distigmai in Their Place”  
presented by Peter M. Head to the NT Textual Criticism Seminar Nov. 21, 2009 in New Orleans

Philip B. Payne

© Payne Loving Trust 2010. All rights reserved. March 31, 2010.

## INTRODUCTION

Thank you, Dr. Wasserman, for the opportunity to address the thought-provoking paper of my esteemed friend, Peter Head, on the Codex Vaticanus distigmai. My response covers his paper (a copy of which Head kindly sent me on December 7, 2009), my recollections of its reading in New Orleans, and your comments. Our collective purpose is that the nature and dates of the various distigmai will be further clarified through our dialog. The consequences of getting this right could hardly be greater, since if fifty-one distigmai match the color of the original ink, as Paul Canart’s analysis concluded but Head’s paper dismisses, they provide remarkable evidence of the reliability of the transmission of the NT text and fresh insights into particular passages.<sup>1</sup>

I am gratified that Head’s examination of the distigmai supports the evidence that they mark places of textual variation between Vaticanus and other texts. This author’s new book and forthcoming article with Canart<sup>2</sup> provide statistical data confirming extraordinary correlation between distigmai and significant textual variations.

---

<sup>1</sup> Philip B. Payne, *Man and Woman, One in Christ: An Exegetical and Theological Study of Paul’s Letters* (Grand Rapids: Zondervan, 2009), 240–43, and Philip B. Payne and Paul Canart. “Distigmai Matching the Original Ink of *Codex Vaticanus*: Do they Mark the Location of Textual Variants?” pages 199–226 in Patrick Andrist, ed., *Le manuscrit B de la Bible (Vaticanus graecus 1209): Introduction au fac-similé, Actes du Colloque de Genève (11 juin 2001), Contributions supplémentaires*. Lausanne, Switzerland: Éditions du Zèbre, 2009, argue this. <http://www.pbpayne.com/?p=312> lists the fifty-one distigmai Canart judged to match the original ink color. *Man and Woman, One in Christ* is available at 41% off, \$17.75, at [www.pbpayne.com](http://www.pbpayne.com). This same site will offer *Le manuscrit B de la Bible* at a discount soon.

<sup>2</sup> Payne, *Man and Woman, One in Christ*, 241–42, and Payne and Canart. “Distigmai,” gives two chi-square probability test results showing the probability that the null hypothesis is correct, namely that the distigmai are unrelated to textual variants. The first chi-square test compares the frequency of significant textual variants, as judged by NA<sup>27</sup> textual variants, occurring in 27 lines preceded by a distigme adjacent to a paragraphos or a longer obelus bar underlining text at the left end of that line and extending into the margin toward the distigme, to the frequency of this in the following 20 lines (hence 540 comparison lines). The chi-square results show that the probability of such a high correlation of distigmai with significant textual variants happening in a random distribution is far less than one in 10,000. *Man and Woman*, pages 237–40, identifies a pattern in five of these 27 lines where a significant block of text is omitted in one of the manuscripts, that the bar is significantly longer than typical paragraphos bars: Matt 18:10; Luke 1:28; Acts 2:47 and at the end of Luke 14:24 and 1 Cor 14:33. Since such a long bar occurs only six times adjacent to a distigme in the entire Vaticanus NT (the sixth is Mark 5:40 where other

## FUNDAMENTAL MISCONCEPTIONS

Some of the responses to my critique of Head's thesis highlight three misconceptions that appear to predispose people to embrace Head's thesis that all distigmai were added to Vaticanus as a single process late in the history of its marginalia:

1. Scribes near the time of Vaticanus did not have the sophistication to be aware of textual variants.
2. Scribes near the time of Vaticanus did not have a system for noting variants.
3. The notation of textual variants is such a rare phenomenon that it could only have happened once.

## PROOF THAT DISTIGMAI MARKED THE LOCATION OF TEXTUAL VARIANTS NEAR THE TIME OF VATICANUS

The fourth or fifth century hexaplaric Codex Colberto-Sarravianus (LXX G)<sup>3</sup> contains distigmai in its margins that correspond closely to the shape and location of distigmai in Vaticanus.<sup>4</sup> These LXX G distigmai have corresponding colon-shaped markers in the midst of

---

MSS insert “but Jesus” in the middle of this line; the bar in Rom 16:5 is shorter), *Man and Woman*, pages 238–40 argues that these should be regarded as distigme-obelus symbols marking the location of interpolations. The second chi-square test compares the frequency of NA<sup>27</sup> textual variants occurring in the fifty-one lines preceded by a distigme that matches the apricot ink color of the original manuscript to the frequency of NA<sup>27</sup> textual variants in the 540 comparison lines. The chi-square results show that the probability of this happening in a random distribution is far less than one in 10,000. The odds of this happening in two successive tests, as it did these two chi-square tests, is infinitesimally small if distigmai are unrelated to textual variants. Hence, these chi-square results provide extraordinarily strong evidence that the null hypothesis (that distigmai are unrelated to textual variants) is incorrect.

<sup>3</sup> *Explanatio signorum, quae in Septuaginta, ed. A. Rahlfs, occurrunt.* (Stuttgart: Württ. Bibelanstalt, 1935). Daniel Buck noticed the distigmai of Codex Colberto-Sarravianus and wrote that they are “similar enough to Vaticanus that one wonders if they may have come from the same scriptorium ... three different sections are extant—130 leaves at Leiden, 22 at Paris, and 1 at Leningrad—the names of the first two are here combined. [It] is the most extensive Hexaplaric LXX to survive, and basically the oldest as well. It contains the Hexateuch, and appears to share an interesting tie-in with Codex Vaticanus” since both omit the last three words of Deut 9:22: τὸν θεὸν ὑμῶν. His comments were posted on Feb. 10 and 11, 2010 at [http://evangelicaltextualcriticism.blogspot.com/2010/02/putting-distigmai-in-their-place-payne\\_08.html](http://evangelicaltextualcriticism.blogspot.com/2010/02/putting-distigmai-in-their-place-payne_08.html).

<sup>4</sup> There is a photograph of a page of hexaplaric Codex Colberto-Sarravianus with distigmai highlighted in pink and labeled as “obelus (umlaut)” at <http://adultera.awardspace.com/TEXT/diacrit.html#02>. Under the image is a citation from Ernst Würthwein. *The Text of the Old Testament: An Introduction to the Biblia Hebraica*. Translated by Erroll F. Rhodes. (Grand Rapids: Eerdmans, 1995), “On the page shown an obelos [sic] marks the words: This indicates that Origen found these words in the LXX, but that they were NOT in the Hebrew text.” This website comments that Würthwein's “basic explanation is sound. ... What is

the adjacent text marking the end of the LXX text that is not in the Hebrew Scriptures. These distigmai and colons in LXX G are simplified forms of Aristarchus's obelus and metobelus as represented in what appears to be the earliest MS of Origen's Hexapla (LXX G). There is no dispute that these mark locations where the Hebrew Scriptures do not include the text in lines marked by distigmai. The conjunction of distigmai with metobeloi separating letters in the midst of LXX G body text proves that both were part of the original production of LXX G. Since LXX G is dated close to Vaticanus, this proves the very early use distigmai to mark the location of textual variants. Correspondingly, it disproves each of the three assumptions cited above that appear to motivate people to discredit the evidence that at least 50 distigmai go back to the original production of Vaticanus.

LXX G also provides a simple explanation for the origin of this particular symbol and this specific location for it in the margin, namely that it is a simplified form of Aristarchus's obelus as represented in what is believed to be the earliest surviving MS of Origen's Hexapla (LXX G). This early use of distigmai in Origen's Hexapla to mark the location of textual variants that depart from the Hebrew Scriptures provides a natural transition to its use to mark the location of Greek textual variants in the original production of Vaticanus. This is a straightforward logical expansion of the distigme's use and one that requires less sophistication and less linguistic skill than Origen's use.

This confirmation of the use of distigmai near the time of Vaticanus to mark the location of textual variants explains naturally why fifty-one of the distigmai in Vaticanus match the color of its original ink according to Canart's careful analysis. As the senior paleographer at the Vatican, who probably has spent more hours examining Codex Vaticanus than any other living scholar, his judgments should not be dismissed lightly. I have witnessed the care with which he examines the ink color of distigmai and unreinforced text on the same page both with the naked

---

of particular interest here however, is the actual form of the 'obelus'. It is in fact [a distigme]. There is no doubt in this case that the function is indeed that of 'obelus', at least according to Origen's version of that function. Here the 'obelus' (actually [a distigme], a sideways colon) marks a part of the Greek which is not found in the 2nd century A.D. (Massoretic) Hebrew text. When the passage extends beyond a single line, each new line that continues the reading is marked also at the beginning ([in] the margin) with the same sign (either Asterisk or Obelus). The most important thing about this particular example here, is that we can observe that these marks are indeed by the original scribe, since in many cases, the beginning and ending marks are actually IN THE MAIN TEXT. The text has not been erased and re-written to make room. Instead, obviously the original scribe was aware of the Hexapla markings and incorporated them into his text as he wrote. ... The method which Origen adopted is described by himself in his famous letter to Africanus (c. A.D. 240)... "I marked, for the sake of distinction, with the sign the Greeks call an [obelus] ... those passages in our copies which are not found in the Hebrew." ... It is not clear whether Aristarchus [of Samothrace (217–145 B.C.)] or his predecessor and teacher Aristophanes invented the original 'accents', or system of critical signs that Aristarchus used to correct and restore Homer's works. To correct the text, he marked with an obelus the lines he considered spurious... [Distigmai] in the NT here [in Vaticanus] are not really a surprise, or any different from the [distigmai] found in the Greek OT. There they were also used to indicate variants in the text."

eye and with a high powered internally lighted loupe. Consequently, I trust his judgment.

## THE KEY ISSUES

I agree with Head's fundamental principle: "When there is interference it is expected that the more ancient marginal material will preserve a more consistent pattern of its placement (due to freedom from interference), while the more recent marginal material will vary its placement as other things interfere with its normal location." Nevertheless, his paper has serious flaws that critically undermine its central thesis that de Sepúlveda penned all distigmai in the sixteenth century. Head states that "this date comports with all the evidence of the interference between marginal material," but much of the evidence suggests otherwise, as this critique demonstrates.

Head's paper fundamentally misrepresents my position and uses definitions of "distigmai" and "textual variant" that are far broader than virtually all previous studies of the distigmai. He changes the criteria of judgment on crucial issues. In particular, he appeals to "the colour and faded nature" of diplai<sup>5</sup> to "place these in the production stage of the codex," but then denies that "even indeed actual similarities of observed colour ... are a particularly good guide to the dating of dots," without stating any justification for this shift. His thesis does not provide a plausible explanation for the sharp difference in distigmai color.

Head's thesis seems to presuppose that all diplai, all distigmai, and all small numbers are, respectively, unified systems, each category of marginalia the product of a single process of approximately the same date. In fact, however, within each of these categories of marginalia there are significantly different symbol shapes and positions, and there is even evidence that their scribes wrote them at different times.

Significant differences from the typical features of distigmai provide evidence that some distigmai were written at different times from others, just as the re-written small numbers were. In a few instances, interaction with other marginalia adds to the evidence that specific distigmai were penned later. In other instances, interaction with other marginalia provides evidence that those distigmai were written earlier than the interacting marginalia. Just before its conclusion, this paper provides criteria for helping to establish which distigmai are part of the original production of the manuscript, which were re-inked in the Middle Ages, and which were added at some other point in its history.

Head mistakenly says I agree with him that "the system of distigmai is a unified system (all are the product of the same process and of approximately the same date even if they were not all applied at the same moment)." Not only have I never advocated this, quite to the contrary, I have clearly distinguished between distigmai that match the color of the original ink of the codex, which should be dated in the fourth century as part of the original production of the manuscript, and distigmai that match the color of the medieval reinforcement.

---

<sup>5</sup> Although Head used "diple" for both singular and plural, this paper uses the correct form, "diplai," for the plural. T. A. E. Brown and C. E. Hill (citing Diogenes Laertius, *Lives of Eminent Philosophers*, 3.65 and Isidore of Seville, *Etymologies* 1.21.13) confirmed this.

Head draws attention to the fact that I do not regard the number of distigmai to be as large as Wieland Willker does, or nearly as large as he does. If all occurrences of dots in the margins are called distigmai, it will be difficult to draw valid conclusions about them without multiple qualifications such as “when there are two dots in horizontal alignment in the margin next to a line of text.” Consequently, I argue that the term distigme should designate pairs of dots that fit within or approximate the parameters of dot size, location adjacent to a line of text, and roughly horizontal orientation found in the fifty-one distigmai Canart has judged to match the original ink color of Vaticanus. These characteristics are quantified just before the conclusion to this paper. Furthermore, as far as can be clearly determined, “distigme” should not designate any mere offset, that is, the mirror impression of a distigme on the facing page, since an offset is merely the inadvertent transfer of ink and has no text-critical significance.<sup>6</sup>

A crucial weakness of Head’s paper is its apparent presupposition that “all [distigmai] are the product of the same process and of approximately the same date.” This presupposition is essential for Head to conclude from evidence that a few distigmai are late, that all distigmai must be late. Nevertheless, this presupposition that all dot pairs to the same process and date is particularly surprising in light of Head’s acknowledgment of “the different colours and weight of ink,” and his acknowledgment that “especially variations from the normal placement of the distigmai may be significant.”

For Head’s view, the problem of the variety of dots in Vaticanus is particularly acute since his paper includes in the category of distigmai “perhaps 825,” many more than either Willker or I classify as distigmai.<sup>7</sup> The broader the category one identifies as distigmai, the harder it will be to defend that they “all are the product of the same process and of approximately the same date.”

I regret that because of the delay in the publication of my forthcoming article with Canart and since its publisher did not grant permission to provide Head an advance copy of it, among other issues, Head was not familiar with my arguments that a scribe wrote the diplai prior to the distigmai. In that article, I provide conclusive evidence that a scribe wrote some distigmai after the binding of the codex.<sup>8</sup>

One part of Head’s paper is, as far as I know, completely original, namely his assertion

---

<sup>6</sup> To include them in any generalizations about distigmai would tend to dilute the data pool and reduce the reliability of any statistical analysis of it.

<sup>7</sup> Although I argue against Head’s broad definition of distigme, since I am interacting with his paper, there are times when in order to cite him accurately, I of necessity repeat his usage.

<sup>8</sup> On Sept. 21, 2009, however, I did email to Head a synopsis of that essay and offered him any assistance I could in preparing or reviewing his paper to make sure it was up to date. Two days before his presentation, on Nov. 19, he emailed me a preliminary yet almost complete version of his paper, but it arrived after I had already left for New Orleans, so I did not see it until after the paper. In addition, I met with Head hours before he presented the paper. When he outlined what he would say, I told him it would be a dereliction of duty not to acknowledge that his thesis provides no explanation for the differences in ink color.

that “92% of all the distigmai in the Gospels match passages of variation between that exact line of Vaticanus and the Greek and/or Latin text of Erasmus. If we further take account of variant readings noted by Erasmus in his Annotations (again offering contemporary manuscript evidence) this rate extends to 98%.” I address this issue at the end of this critique.

The ultimate question is, given their variety in color, location, orientation, shape, and apparently even purpose (discussed below), whether Head’s view is even plausible that “the system of distigmai is a unified system ... all are the product of the same process and of approximately the same date.” How can they all be the product of the same process and of approximately the same date in cases where there seems to be obvious re-inking? Re-inking is a very different process than the noting of the location of textual variants, one that would only be reasonable after the original ink had faded, which is a process that takes considerable time. Particularly problematic for Head’s view is the distigme at 1409 B 25 (Acts 18:16), where the left dot appears to be re-inked but the right dot is not re-inked and still displays what Canart classifies as “probable” to be the original ink of the codex.<sup>9</sup> Canart also discerned traces of the original ink color of the codex protruding from the distigme at 1469 A 3 (1 Cor 9:22), which is also clearly visible in the new facsimile,<sup>10</sup> and from the distigme at 1501 B 42 (Phil 3:16–17). NA<sup>27</sup> notes early variants in all three of these distigme locations. Consequently, these distigmai displaying two colors, both the original ink color and the reinforcement ink color, support the view that a scribe wrote them during the original production of this codex to mark the location of significant textual variants. Head must provide an explanation of these variations in ink color in order to make his thesis plausible. Willker, in contrast, acknowledges, “This is a good argument,” for the Payne-Canart thesis.<sup>11</sup>

Similarly problematic to Head’s thesis are Willker’s observations: “In some cases the reinforcer interpreted an imprint as a true [distigme] and reinforced both!”<sup>12</sup> “At least in one instance the reinforcer reinforced [a distigme] which shows through the page from the verso.”<sup>13</sup> This indicates that a scribe wrote these distigmai, at least, prior to the medieval reinforcement and, consequently, long before de Sepúlveda.

Also against Head’s contention regarding distigmai that “all are the product of the same process” is evidence that in various instances the reinforcer associated distigmai with spelling corrections marked in Vaticanus. There are two dark chocolate brown dots before six lines where

---

<sup>9</sup> A photograph of this is in the forthcoming Payne and Canart, “Distigmai.”

<sup>10</sup> *Biblorum sacrorum graecorum Codex Vaticanus B: Bibliothecae Apostolicae Vaticanae Codex Vaticanus Graecus 1209*. Rome: Istituto Poligrafico e Zecca dello Stato, 1999. The remaining copies are available below list price at <http://www.linguistsoftware.com/codexvat.htm>.

<sup>11</sup> Willker, “Codex Vaticanus Graece 1209, B/03: Umlauts: Dating” at <http://www-user.uni-bremen.de/~wie/Vaticanus/umlauts.html>.

<sup>12</sup> Wieland Willker, “Codex Vaticanus Graece 1209, B/03: The Umlauts: Imprints” at <http://www-user.uni-bremen.de/~wie/Vaticanus/imprints.html> cites 1334 B 23 R, 1396 B 39 R, and 1506 A 28 L as re-inked. Similarly, both the distigme and its mirror impression at 1310 C 39 L and 1311 A 39 R match the color of the re-inked text.

<sup>13</sup> Willker, “Codex Vaticanus Graece 1209, B/03: The Umlauts: Imprints” at <http://www-user.uni-bremen.de/~wie/Vaticanus/imprints.html> cites 1383 A 4 R.

the reinforcer corrected spelling over an unreinforced letter: 1281 A 26, 1361 C 1, 1423 A 14, 1479 A 12, 1481 C 21, 1501 B 42. The reinforcer in an seventh such instance may have regarded his change of **H** into **€I** in 1262 A 2 also as a spelling correction. The best evidences of the re-inking scribe's association of distigmai with spelling corrections are instances where corrected spelling is marked in the margin by a symbol that is similar to a distigme, but is shaped and positioned differently. In 1468 A 26 the dots are vertically aligned with only the top dot in a normal distigme position. In two cases the marks are positioned lower than typical distigmai and are not two dots but rather two short slanted strokes somewhat like grave accents: 1409 A 23–24 (pointed out by codicologist Patrick Andrist) and 1423 A 14. The distinctive marks at 1409 A 23–24 are half way between two lines, unlike any original distigme, presumably because the name “Titius” begins on line 23 and wraps onto line 24. “Titius” is unreinforced, which effectively changes the name to “Justice.” These distinctive features indicate that the scribe did not trace over original distigmai in these three instances but created these marks. Similarly, the two dots before the spelling correction in 1281 A 26 are lower than typical distigmai, almost on the baseline; the left dot is noticeably higher than the right one, rather than being positioned in the typical horizontal alignment; they are closer to text than most distigmai; and there is a small dark chocolate color dot between them.

Apparently, then, a scribe misunderstood the original purpose of the distigmai to mark the location of textual variants and, instead, added marks similar to them, but in some cases noticeably different in both shape and location, in order to mark the location of spelling corrections made in Vaticanus. This illustrates the value of limiting the definition of distigmai to dot pairs that, though they may be re-inked, have characteristics falling within the apricot color distigmai's range of size, shape, and location relative to text.

Head states that his paper focuses “on an area which Payne and others have not worked on, the relative chronology of the dots in relation to the other marginal material.” In fact, Willker and I had already taken into consideration most of the categories of marginalia raised in Head's paper, yet their data has not convinced either of us that all the distigmai are a unified system or the product of the same process and of approximately the same date. Since Head cites Willker's website, he should be aware that a section of that site addresses the chronological order of the distigmai in relation to diplai and section numbers.<sup>14</sup> Furthermore, Head's paper acknowledges, “Payne actually suggested this for 1245 B 6 (Matt 9.13).” I explained this displacement as follows (the first explanation considering the possibility of a chronological sequence where the small number was written prior to this distigme): “either or both of two factors appear to have caused this. First, another symbol, **NS**,<sup>15</sup> already occupies that location. If the [distigme] were put on the left as it usually is in column B, it would have overlapped this other symbol. Second, the text that is omitted is on the right side of the line, which makes the [distigme] on the right of the line particularly appropriate.”<sup>16</sup> As detailed in the section below about diplai and distigmai,

---

<sup>14</sup> Willker, “Codex Vaticanus Graece 1209, B/03: Umlauts: Chronological order” at <http://www-user.uni-bremen.de/~wie/Vaticanus/umlauts.html#chr>.

<sup>15</sup> The VaticanusLS font from [www.linguistsoftware.com/ntmss.htm](http://www.linguistsoftware.com/ntmss.htm) is used throughout this paper to represent Vaticanus text.

<sup>16</sup> Philip B. Payne, “Fuldensis, Sigla for Variants in Vaticanus, and 1 Cor 14.34–5,” *NTS* 41 (1995): 256, note 58. This article is available for free download from [www.pbpayne.com](http://www.pbpayne.com).

my 2001 paper that Canart presented to the Geneva Colloquium on Codex Vaticanus argued the relative chronology of the diplai as prior to the distigmai.

Head also mistakenly writes that he agrees with Payne that, “the different colours and weight of ink suggest more than one comparative movement through the NT.” I do agree with Head that the distigmai note variant readings in multiple manuscripts, but on different grounds. Differences in ink color are not a conclusive argument for multiple manuscript comparisons since other factors could explain this. In particular, the dark chocolate brown color matching the medieval reinforcement is most naturally explained as reinforcement of faded ink at that time, just as it is for the text. This, the most common difference in color, was probably not the result of comparison to additional manuscripts. These differences in color are, instead, strong evidence that the medieval scribe who penned these dark chocolate brown distigmai probably was reinforcing fading ink. All instances where distigmai match the color of medieval reinforcement and all instances of distigmai significantly faded relative to others nearby undermine Head’s thesis that de Sepúlveda penned all the distigmai in the sixteenth century.

The fifty-one distigmai that Canart has judged to match the color of the original ink range all over the page from the top to the bottom of the manuscript and are associated with each column of the open codex: 8 before the first column, 9 between the first and second columns, 7 between the second and third columns, 7 before the fourth column, 9 between the fourth and fifth columns, 2 between the fifth and sixth columns (since this is the usual position for either of these columns), and 10 after the sixth column.<sup>17</sup> Consequently, they defy any explanation for their apricot color based on their position on the page.<sup>18</sup>

The strongest evidence that Vaticanus was compared to multiple manuscripts is that a good number of the distigmai that have been offset (leaving a mirror image on the facing page in ink matching the codex’s original ink color) are followed by other distigmai on the same page that did not offset ink. If all of these distigmai were penned at the same time, noting variants in only one manuscript, then the following distigmai should also have been offset, since in a sequential comparison they would have been penned later and their ink would also have been wet enough to offset on the facing page.<sup>19</sup> Additional evidence Vaticanus was compared to multiple manuscripts is that the known significant textual variants at the distigmai locations

---

<sup>17</sup> These add up to 52 since both 1380 A 26 and 1381 C 26 are included, although one is a mirror impression.

<sup>18</sup> Curt Niccum’s statement at [http://evangelicaltextualcriticism.blogspot.com/2009/11/sbl-new-orleans-2009-i-peter-head\\_22.html](http://evangelicaltextualcriticism.blogspot.com/2009/11/sbl-new-orleans-2009-i-peter-head_22.html) baffles me: “when Payne first presented his argument for an underlying level of apricot-colored ink, every example came from interior margins where abrasion would be most severe. In fact, none of the original eleven distigmai that Canart identified were on the interior margins between columns three and four. All are listed in Philip B. Payne and Paul Canart, “The Originality of Text-Critical Symbols in Codex Vaticanus,” *NovT* 42 (2000): 108, which is available for free download from [www.pbpayne.com](http://www.pbpayne.com). Of these, 2 are before the first column, 3 are between the first and second columns, 2 are between the fourth and fifth columns, 1 is between the fifth and sixth columns, and 3 are after the sixth column. They, too, are distributed in various parts of the Vaticanus pages, from top to bottom and left to right.

<sup>19</sup> I explained this in an email to Head on Sept. 21, 2009.



come from diverse manuscript traditions that could not reasonably have come from a single manuscript, as Willker also argues.<sup>20</sup>

Nevertheless, Head is making a valuable contribution by pointing out that color differences may be evidence for multiple manuscript comparisons. Still, I argue that the color differences also support manuscript comparisons at different times. So, while dark chocolate brown distigmai matching the ink color of re-inked text are most naturally explained as mere reinforcements of the original notation, it is entirely possible that the reinforcer or one or more later scribes may have noted new textual variants or may have used pairs of dots for some other purpose. If the ink color of other distigmai is confirmed to differ from both the original apricot color ink of the manuscript and the dark chocolate brown ink of the re-inking, it makes sense to date them at different times. This also applies to other marginalia. Differing ink color supports rather than undermines Canart's and my arguments that the apricot-colored distigmai matching original text date to the original production of the manuscript.

Following is an assessment of the evidence Head presents for dating distigmai later than diplai, small numbers, large numbers, and other marginalia:

#### DIPLAI

Head argues for the originality of diplai as follows: "The consistent and careful placement, the colour and faded nature, and the consensus of observers place these in the production stage of the codex." Since Head regards the color and faded nature of the diplai as important evidence for placing the diplai in the production stage of the codex, it is inconsistent for him to dismiss the possibility that the color and fading of some distigmai may provide evidence that they were added in the production stage of this codex as well.<sup>21</sup> The proposition that at least some of the distigmai are original is based on Paul Canart's careful documentation of fifty-one distigmai matching the color of the original ink. One of them (1309 A 23) appears to match the color of a dipole less than 2 mm from it. Willker asks appropriately, "why should some [distigmai] fade and the neighbouring text not? ... The different colour is a serious objection [to late dating of distigmai]."<sup>22</sup>

Since 2001 I have argued publicly, just as Head does, that most diplai were added to Codex Vaticanus prior to the original distigmai. I still argue that offset distigmai matching the color of the original ink demonstrate that a scribe penned the distigmai after the leaves were folded and gathered into quires.<sup>23</sup> Even my first *NTS* article on the distigmai in 1995 (p. 256, note 58) pointed out that the distigme matching the original ink color of the codex at 1309 A 23

---

<sup>20</sup> Willker, "Codex Vaticanus Graece 1209, B/03: Umlauts: Distribution of the Umlauts" at <http://www-user.uni-bremen.de/~wie/Vaticanus/umlauts.html#dis>.

<sup>21</sup> "Needless to say, I am not persuaded that purported similarities of colour (even indeed actual similarities of observed colour) are a particularly good guide to the dating of dots."

<sup>22</sup> Willker, "Codex Vaticanus Graece 1209, B/03: Umlauts: Dating." Though Willker was objecting to dating apricot color distigmai to the Middle Ages, the objection would apply even more strongly to dating them to the 16<sup>th</sup> century.

<sup>23</sup> I argue this in "Distigmai" and in *Man and Woman, One in Christ*, page 242.

lies to the left of a diplo (ϥ) identifying an OT quotation and that this distigme's unusually far left position is evidence that diplai marking OT quotations on this page may have been written prior to it. Furthermore, unlike diplai, distigmai are usually placed in the far right margin of the sixth column of the open codex. On the basis of these differences, I have argued that, for the most part, the addition of the distigmai and diplai were separate steps in the original production of the manuscript. Head apparently thought he was undermining my position with this evidence, when in fact he was confirming my judgment.

I agree that Head provides convincing evidence that diplai were penned prior to distigmai in three instances, and in each of these three instances other factors indicate that the distigmai may be a later addition. The distigme at 1238 B 27 is in darker ink than both the apricot color diplo whose point it obscures and the surrounding chocolate brown re-inked text. Furthermore, NA<sup>27</sup> lists no textual variant here. It is unlikely the original scribe would partially obscure his own diplo, or that an already re-inked distigme would be re-inked again. Similarly, the distigme at 1255 A 39 is in darker ink than both the apricot color diplo whose point it obscures and the surrounding chocolate brown re-inked text. Furthermore, its dots are not circular, its left dot being particularly elongated, and its left dot is noticeably higher than its right dot. Consequently, I believe that neither of these distigmai should be attributed to the original scribe nor to the medieval reinforcement. Similarly, the distigme at 1255 B 3 significantly obscures a diplo, its dots are not circular, nor do they match the apricot color of the original ink, and NA<sup>27</sup> lists no variant on the line. Consequently, I agree with Head that it, too, should not be attributed to the original production of Vaticanus.

I also agree that Head's evidence is compelling that diplai must have been present prior to the small sectional numbers where  $\overline{\Pi} \overline{\Theta} \overline{\text{H}}$  occurs at 1252 C 13 and where small numbers overlap a diplo at 1249 C 36, 1379 B 18, and probably 1274 B 27. These, however, have no bearing on the dating of any distigme.

Nevertheless, Head's assertion that there are "sixteen places of interference between diplo and distigme" is clearly an overstatement. Three of Head's sixteen examples have no diplo.<sup>24</sup> One has no distigme.<sup>25</sup> Eight<sup>26</sup> lie within the normal range for distigme separation from adjacent text, and so should not be regarded as "accommodating to the prior existence of the diplo." Furthermore, even positioning to the left of a diplo is not particularly surprising since there is significant variation in the separation of apricot color distigmai from text even without competition for space.<sup>27</sup> In any event, we are agreed that diplai were generally written prior to distigmai, so in such cases, where both precede the same line of text, of course the distigme is

---

<sup>24</sup> 1402 A 38 (perhaps Head misinterpreted the dots that show through from 1401 C 38 as a diplo), 1459 A 28, and 1514 A 10 (which shows through from the other side of the vellum).

<sup>25</sup> 1518 A 33. Perhaps Head meant 1518 A 37, but it is in a normal distigme position and so does not evidence interference.

<sup>26</sup> Only four of the nine he lists as "inside dipl[ai]" are between a diplo and Vaticanus text: 1237 A 1, 1386 A 35, 1449 A 17, 1459 A 26. The eighth, 1455 B 31 L is not inside a diplo but outside. Three: 1402 A 38, 1459 A 28, and 1514 A 10 33 have no diplo, and one, 1518 A 33, has no distigme. 1518 A 37, which Head may have intended, is also in a normal distigme position.

<sup>27</sup> Documented on pages 26–27 and in footnote 81 below.

written either the outside or the inside of the diplo. The only clear instances of interference are the three cases where a distigma partially obscures a diplo, and, as noted above, each of these bears the characteristics of a later hand, so should not be regarded as evidence of a late date for all distigmai, and certainly not for the fifty-one distigmai that match the apricot color of the original ink.

Head affirms “The consistent and careful placement” of the diplai and says that “the placement of the dipl[ai] are quite consistent.” I found 123 isolated diplai or sets of diplai on consecutive lines in the Vaticanus NT where each diplo is aligned with the others in a remarkably straight line and all have comparable shape, size, apricot color, and intensity of ink. There are also, however, 22 sets of diplai where there is a pronounced difference among consecutive diplai in shape, size, color, and/or intensity of ink.<sup>28</sup>

Even among diplai, there are demonstrable differences not only of position, shape, size, ink color and intensity, but also of the time of their writing. For instance, the diplo at 1387 B 30 is a lighter color, shows through the page less than the previous seven diplai, has a more open angle, and is farther left than the previous seven diplai. What is most instructive, however, is that this diplo at 1387 B 30 is farther left apparently in order to avoid the  $\omega$  that shows through from the opposite side of the leaf. There is even more pronounced showing through of ink from the  $\Upsilon$  at 1388 B 28 below the sixth diplo at 1387 B 28, but that diplo overlaps the ink that shows through and is exactly in line with the other seven original diplai. These factors together constitute evidence that the sixth diplo, and presumably each of the first seven, was written before page 1388 was written, but the eighth diplo was evidently written after page 1388 was written and positioned farther left to avoid the ink that shows through. In spite of the differences, and especially the different position of the eighth diplo at 1387 B 30, its apricot color and the artistic diplo shape characteristic of the original hand is evidence that it was penned by the same scribe as the ones above it, but after writing the text on the other side of the vellum. The calligraphic beauty of the text of Vaticanus still visible in apricot color ink (e.g. at 1479 B 33–36) and of most of the apricot color diplai, supports the view that the same scribe who wrote the

---

<sup>28</sup> Size and intensity of ink: 1435 B 13, 1456 B 38–42. The last diplo is farther left: 1447 C 30. The last diplo is farther left and has a different shape: 1387 B 30, 1454 C 18, 1463 A 8. The last diplo is farther left and has a different size: 1311 A 39. The last diplo is farther left and has a different shape and size: 1310 C 9. The last diplo is farther left and has a different shape, size, and intensity of ink: 1311 A 4. The last diplo is farther right: 1341 A 12, 1392 A 26. The last diplo is farther right and has a different shape, size, and intensity of ink: 1491 C 4. Instances where all the diplai have an atypical shape, vary in intensity of ink, and are also unusually close to text: 1455 C 27–32, 1455 C 34–42 and 1456 A 1, 1456 C 1–2. Instances where the color of the ink approaches more closely the dark chocolate brown color of the ink used in the medieval reinforcement: 1352 A 8–9 (contrast the original ink apricot color at 1352 A 19); 1358 C 31 (if this is a diplo), 1361 A 31–34 (probable), 1361 B 8–9 (ambiguous), 1455 B 31 (not completely clear), 1455 C 38 (probable). In one instance, 1455 C 30, a diplo may even point backwards, but since the lower stroke aligns with an acute accent, it seems more likely that it shows through from the reverse side of the vellum. In that case, the two dots at the top of the other stroke are all that remains of the top stroke of this diplo. The diplai at 1455 C 28, C 29, and C 37 looks similar to this one but without ink from the reverse showing through.

text also wrote most of the diplai. The evidence that at least the diple at 1387 B36 was written prior to the text on the reverse side of this page leaves little doubt that the same skilled scribe who penned the NT text also penned at least some of the diplai concurrently with the text.

The diplai that differ significantly from standard diplai are the most likely to have been added later. Some diplai are so different in shape and position from all of the original diplai that it is virtually certain that they are by a different scribe, including all of the diplai at 1455 C 27–32, 1455 C 34–42, 1456 A 1, and 1456 C 1–2, each of which is far closer to text than any of the original diplai. Each of these is smaller and lacks the calligraphic quality of the original diplai. Based on the close correlation between diplai of all shapes and OT citations, the function of diplai appears to be consistent, which is not surprising since many of the citations are explicitly introduced as such.

Head asserts: “the small numbers are also secondary to the dipl[ai].” While this is true as a generalization,<sup>29</sup> there is significant evidence that some diplai were penned after a small number, as the following three examples demonstrate.

Of the three diplai Head cites on the outside of a small number, the one at 1311 A 4 is noticeably farther left than the preceding two diplai at 1311 A 2–3, apparently because the small number  $\overline{\text{KH}}$  occupies the position below the other two diplai. This diple was probably penned after the  $\overline{\text{KH}}$  and is placed farther left to avoid overlapping it. Compared to the previous two diplai, the diple at 1311 A 4 is also much smaller, lacks the graceful curves of the previous ones, and has a wider angle, which give further evidence that it was penned separately. The shape of the  $\text{K}$  in this rubricated section number is remarkably similar to the fourth letter in the text to its right,  $\text{K}$ .

Similarly, in 1310 C 7–9, two diplai in normal position are followed by a third at 1310 C 9 that is smaller, simpler, and farther left than the first two diplai, apparently in order not to be too close to the small number  $\overline{\text{KS}}$ .

Of the two diplai Head cites on the inside of a small number the one at 1244 A 20 is noticeably farther right than each of the three immediately preceding diplai. If it were in line with the preceding three diplai, it would overlap the small number  $\overline{\text{NA}}$ . The unusual shape of the diple, its almost horizontal top stroke, its bottom stroke curving the opposite direction from typical diplai, its lack of a top hook, its simpler, less calligraphic, style, and its darker ink all add evidence that it was added at a different time. Its position favors a time after  $\overline{\text{NA}}$  was written.

Surprisingly, Head cites all three of these instances to show that “the numbers are secondary in relation to the dipl[ai] ... at moments of interference,” which is the opposite of what these examples indicate. His questionable judgment comes from treating diplai like he does distigmai, namely as a unified system: “all are the product of the same process and of approximately the same date.” These examples, however, give evidence that a scribe wrote at least some of the smaller, simpler diplai after small section numbers were in the text.

---

<sup>29</sup> Small numbers that overlap diplai prove this, e.g. 1249 C 36, 1379 B 18, and probably 1274 B 27, as does one number written around a diple at 1252 C 13, as Head correctly observes.

These examples show that evidence some diplai were written later than other diplai should not be interpreted as evidence that all diplai were written at a later time. Since this appears to be true even of diplai, which display far more consistency in positioning than distigmai, it should not be surprising that some distigmai were also written later than others.

## SMALL NUMBERS

Head argues that on “at least five occasions we find that the presence of the small numbers seems to have caused a displacement” of distigmai from their normal position on the left side of one of the first five columns of the open codex to a position on its right side. For various reasons listed just before the conclusion of this paper, including four apricot color distigmai on the right of columns where there are no other marginalia on the left, I argue that simply being on the right is not a clear indication of displacement.

Head’s first instance regards the  $\overline{\Lambda\Gamma}$  at 1240 C 23 (Matt 6:1). NA<sup>27</sup> notes that the last five letters on this line,  $\Delta\text{I}\text{K}\Delta\text{I}$ , are replaced by  $\epsilon\lambda\epsilon\eta\mu$  in manuscripts L W Z  $\Theta$   $f^{13}$  33  $\mathfrak{M}$   $f$   $k$   $sy^{p,h}$  mae. The endings of both words are identical with the letters beginning the next line,  $\text{O}\text{C}\text{Y}\text{N}\text{H}\text{N}$ , so the difference is clearly at the end of the line. This explains the position of this distigme on the right side of this line.

Head’s second instance regards the  $\overline{\Lambda\Delta}$  at 1241 A 7 (Matt 6:5). NA<sup>27</sup> notes that the last three letters of this line,  $\text{C}\text{O}\epsilon$ , are omitted in manuscripts  $\aleph^*$  D L W  $\Theta$   $f^{13}$  33  $\mathfrak{M}$   $k$   $q$   $sy^{s,p,h}$ , so the distigme on the right side is ideally positioned to indicate this textual variant.

Head’s third instance regards the  $\overline{\aleph\varsigma}$  (Head calls the stigma<sup>30</sup> a digamma) at 1245 B 6 (Matt 9:13). NA<sup>27</sup> notes that just before the last short word in this line ( $\overline{\Gamma\text{O}\Gamma\epsilon}$ ) manuscripts C L  $\Theta$  0281  $f^{13}$   $\mathfrak{M}$   $g^1$   $sy^{s,hmg}$  sa mae bo<sup>pt</sup> add after “I came not to call the righteous, but sinners” the words “to repentance.” Again, the known variant is near the right hand side of this line, which explains the position of the distigme on the right of this line.

Not only do all three of these examples have a significant variant at or very near the right hand side of the line, manuscripts L  $\Theta$   $f^{13}$   $\mathfrak{M}$  include all three of these variants, and all three

---

<sup>30</sup> Many people do not realize how far back the use of stigma goes. Chris Hopkins, Nusmismatica Font Project, <http://www.parthia.com/fonts/stigma.htm> includes photographs of four coins embossed with a stigma from the time of Christ. He states, “G. F. Hill differentiates Digamma  $\text{Ϝ}$  and Stigma  $\varsigma$ , and tells us the  $\varsigma$  was used only as a numeral... The terminological confusion between Digamma  $\text{Ϝ}$  and Stigma  $\varsigma$  appears to be caused by their common numeric value and that  $\varsigma$  supplanted  $\text{Ϝ}$ . Digamma  $\text{Ϝ}$  was used as both letter and number until its eventual disappearance. I have not seen Digamma  $\text{Ϝ}$  used on coins in its numeric sense.” Cf. George Francis Hill, *Ancient Greek and Roman Coins: A Handbook* (Chicago: Argonaut, 1964; first published in 1899 as *A Handbook of Greek and Roman Coins*), 215. Herbert Weir Smyth, *Greek Grammar* (Rev. by Gordon M. Messing; Cambridge: Harvard University Press, 1956), 8 notes that the digamma presumably fell into disuse about the time Athens adopted the Ionic alphabet in 403 B.C., but it disappeared gradually, and was used in Boeotia as late as 200 B.C.

occur between Matt 6:1 and 9:13, within six pages, so might easily have come from a single manuscript. Their common sources and corresponding notation on the right of each line, where these variants occur, further support that these are the textual variants noted by these distigmai. Furthermore, a single scribe noting variants in the same manuscript all in this short span of text is more likely to place distigmai on the right side of each of these lines of text than if the variants had been from different manuscripts compared at different times from different parts of the NT. Consequently, none of these three either in isolation or together constitute clear evidence that the small numbers affected the position of any of these distigmai.

Head's fourth example is the  $\overline{\text{NΘ}}$  at 1274 C 41. Since there is no distigme anywhere near  $\overline{\text{NΘ}}$ , I presume Head refers to the distigme on the right hand side of 1273 B 41 (at Matt 12:59) as being placed there to avoid overlapping the bleed through of this number. There is, however, room for a distigme on the left side of this line without touching this number even if the distigme is given the same generous separation from the text that it now has on the right hand side of the line. NA<sup>27</sup> lists no variant on this line, either on the left or the right side of it, so gives no guidance in this instance.

Head's final example regards the  $\overline{\text{OΓ}}$  at 1496 B 10 (Eph 4:17). If the scribe who penned this distigme had positioned it the same distance from the text on the left side of column B as it is currently on the right, there would have been more space between it and the small number than between it and the text. Consequently, it cannot be safely assumed that it was positioned on the right side of the column in order to avoid interference with the small number. The more likely reason for its position on the right side of the column, then, is that the variant being noted was on the right side of the line, just as extant variants show to be likely in the three other comparable instances just noted. NA<sup>27</sup> lists no variant on this line, either on the left or the right side of it, so gives no guidance in this instance.

Head asserts that "there is no evidence for the distigmai interfering with any" small number. His assertion is undermined by the position of the  $\overline{\text{E}}$  at 1278 B 12, which is significantly farther left than any of the small numbers between two columns of text throughout Matthew or Mark. The apparent reason for this is to avoid overlapping the distigme<sup>31</sup> to its right. This is evidence that a distigme affected the position of a small section number. Head has not identified corresponding evidence of a small number affecting the position of a distigme. Since Head dates the small numbers early, "perhaps fifth century," unless this is not a distigme, it is evidence against Head's thesis that all distigmai were written in the sixteenth century.

Because of their sequential nature, the small numbers should be regarded as a unified system, even though at least twenty-two of them were rewritten later after a large number

---

<sup>31</sup> Head's expansive understanding of distigme would seem to require that it be regarded as a distigme, but its orientation is not as horizontal as most distigmai, though only slightly more than the apricot color distigme at 1351 A 6 (cf. below, note 79), and its dots are closer together than most, though not as close as the apricot color distigme at 1308 B 27.

partially obscured the original small number.<sup>32</sup> This rewriting of so many small numbers around large numbers proves that these repositioned small numbers were written after the large numbers, which Head states were “added at a much later date.” This means that many small numbers were penned in a later era. Since this is indisputable even within such a unified system of sequential numbers, it is not reasonable to dismiss this possibility among distigmai, which lack any analogous cohesion.

## LARGE NUMBERS

Head alleges “that the large numbers are earlier than the distigmai ... because distigmai appear sometimes inside and sometimes outside the large numbers.” Whenever distigmai appear inside large numbers, however, they are in their normal distigme position, so this does not demonstrate interference.<sup>33</sup> The only instance Head cites of a distigme on the outside of a large number, 1455 B 31, also shares other signs of not being original.<sup>34</sup> It is above the top of the following text line, which is highly unusual.<sup>35</sup> It is farther from text than usual. The left dot is higher than the right dot, which in itself would not be conclusive, but it is paired with two dots also above the top of that line of text but on its right side, over a square with a dot on each side, which is without parallel regarding any apricot-colored distigme, and, as far as I have observed, with any distigme. Consequently, I agree with Head that this distigme should be dated after the large numbers.

Head also alleges “that the large numbers are earlier than the distigmai ... because on two occasions distigmai are placed in the right hand margin at places where large numbers occupy their normal location in the left hand margin.” His second example, however, is not valid since the distigme is on 1482 C 10, the line above the large number. Furthermore, there is also a distigme in its normal position on the left side of 1482 C 10, proving that its position does not interfere with this large number. Head’s other example, the placement of the distigme on the right side of 1407 B 20 is not conclusive for three reasons. First, if it were on the left side of the text the same distance from the text that it currently has on the right, it would not touch the large number, so the large number does not necessitate this position on the right. Second, if it indicates

---

<sup>32</sup> 1387 C 13–14, 1388 B 18, 1394 B 37, 1399 B 18, 1401 A 18–19, 1414 A 27, 1418 B 13, 1424 C 2–3, 1427 C 40, 1431 C 25, 1433 C 11, 1457 C 1, 1465 B 19, 1466 A 28, 1467 C 6, 1471 B 20, 1474 B 5, 1478 C 10, 1495 C 20, 1508 C 3, 1511 B 21, 1513 C 10.

<sup>33</sup> 1426 B 38 (contrast the farther extension of the overbar in  $\bar{\text{B}}$  at 1438 C 10 and 1442 C 18, which would have interfered with the distigme at 1426 B 38 if that overbar had extended to the right similarly), 1486 C 20, 1508 C 5 (only the tail [ſ] of the large number at 1508 C 3 extends as far as the distigme, but even it does not come close to the distigme), 1449 A 35 (its position might be construed as affected by the large number, but it is clearly separated from the large number, and this distigme is the same distance from text as the next distigme at 1449 B 11. Furthermore, if the scribe had written this distigme at 1449 B 11 after the large number, one would expect it to be at the more usual mid-character height, since that position would have provided more separation from the bar under the number.

<sup>34</sup> Cf. the criteria listed just before the conclusion of this paper.

<sup>35</sup> Only one apricot color distigme has both dots above the line of text. Cf. below, page 27, item 6, and note 80 in the description of the range of apricot color distigmai characteristics.

the textual variant noted in NA<sup>27</sup> of the  $\mathfrak{M}$  reading that substitutes  $\pi\rho\omicron\sigma\lambda\alpha\beta\acute{o}\mu\epsilon\nu\omicron\iota$ <sup>36</sup> for  $\zeta\eta\lambda\acute{o}\sigma\alpha\nu\tau\epsilon\varsigma$  on 1407 B 20 it may be on the right since  $\pi\rho\omicron\sigma\lambda\alpha\beta\omicron\mu\epsilon$  is at the end of the immediately following line B 21, so the position on the right helps to identify the variant. Third, four apricot color distigmai are on the right side of a column without any interference from another symbol, so positioning on the right is always a weak indicator of secondary influence.<sup>37</sup>

Head asserts that “there is no evidence for the distigmai interfering with any [large number].” There is, however, evidence that distigmai interfere with large numbers. The large number  $\mathbf{I}\Theta$  at 1486 C 20 (2 Cor 12:11) is positioned to the left of the distigme, which is in a normal distigme location,<sup>38</sup> even though the over-bar, which identifies it as a number, extends over the distigme. In every other instance of a large number from the beginning of 1 Corinthians all the way to the end of the surviving uncial text in Hebrews, the overbar is always directly over each large number, never extending out beyond the number like this. The only other instance of a bar extending to the right of a large number  $\Theta$ <sup>39</sup> like this, 1416 C 17 at Acts 23:1, also extends over an addition in the margin, suggesting that in both instances the additional material in the margin attracted the extension of the bar beyond the large number theta. This is evidence that the distigme at 1486 C 22 affected the positioning of this large number.

Another instance where a large number’s position appears to accommodate for the presence of a distigme is at 1508 C 5, where the tail of the large number at 1508 C 3 is unusually far left, apparently to avoid intruding on the distigme in a normal distigme position to its right.<sup>40</sup>

Consequently, although there is one instance (1455 B 31) where a variety of evidence points to a distigme being written after a large number, in other cases a large number appears to have been written so as to avoid overlapping an already existing distigme. Thus, as regards large numbers, Head has overstated the evidence for and has not acknowledged evidence against his generalization that “the distigmai appear secondary.”

## RE-INKING IN THE MIDDLE AGES

---

<sup>36</sup> This OdysseaUBSU font is available at [www.linguistsoftware.com/lgku.htm](http://www.linguistsoftware.com/lgku.htm).

<sup>37</sup> Cf. the final paragraph before the conclusion of this paper.

<sup>38</sup> The large  $\Theta$  is 4 mm from the adjacent text. Some of the large Greek letters representing numbers come much nearer text than this one, e.g. the first one preceding it (1483 C 9, where  $\mathbf{Z}$  is within 2 mm of text) and the third one preceding it (1481 C 33, where  $\mathbf{E}$  is within 1.5 mm of text).

<sup>39</sup> The bars over every other large number (these begin in Acts) ending in  $\Theta$  are centered over the  $\Theta$ : 1386 B 23, 1380 C 40, 1397 A 6, 1403 A 5, 1410 A 10, 1424 C 3, 1474 B 5.

<sup>40</sup> Although a number tail is rarely almost this far left (e.g. 1501 B 21, but even it is primarily to the right of the vertical stem of the  $\mathbf{B}$ , whereas the tail at 1508 C 3 is primarily to the left of the vertical stem of the  $\mathbf{B}$ ), the tail at 1508 C 3 is much farther left than a typical large number tail, e.g. 1481 C 33, 1482 C 11, 1483 C 9, 1485 A 24, 1486 C 20, 1488 A 22, 1491 B 14–15, 1497 B 30, 1513 C 10, 1515 A 6.



Head asserts that distigmai “are later than the re-inking.” But if that is so, how is it that traces of the original ink color of the codex protrude from the apparently re-inked distigmai at 1469 A 3 and 1501 B 42? Why does the left dot in the distigmai at 1409 B 25 (Acts 18:16) appear to be re-inked but the right dot is not re-inked and still displays what Canart classifies as “probable” to be the original ink of the codex? Why on the same page as forty-five of the fifty-one distigmai that match the color of the original ink<sup>41</sup> is there at least one other distigme in a different color? Why do so many of the distigmai appear to have been re-inked along with the body text?

Willker argues that the distigme at 1498 C 3 preceded the medieval reinforcement: “When the enforcer reinforced the letters he also applied in certain cases a different syllable division. He did this by leaving the last letter of a line unenhanced and writing it anew in front of the first letter of the next line. Cases with Sigma and Nu are frequent. In a few cases a small section number is in the way. Here the enforcer squeezes the letter between the section number and the text. Now there is one case where there is [a distigme] in the way:

Page: 1498 C 3 L: A Sigma is squeezed between [distigme] and text.

Old division:

ΜΥΣΤΗΡΙΟΝ ΥΠΕΡ ΟΥ ΠΡΕC

ΒΕΥΩ ΕΝ ΑΛΥCΕΙ· ΙΝΑ ΑΥ

New division:

ΜΥΣΤΗΡΙΟΝ ΥΠΕΡ ΟΥ ΠΡΕ

CΒΕΥΩ ΕΝ ΑΛΥCΕΙ· ΙΝΑ ΑΥ

Consequence: The [distigmai] are older than the reinforcement. Since the reinforcement is dated by Tischendorf to the 10th or 11th CE, the [distigmai] must be earlier than this date.”<sup>42</sup>

This C inserted between a distigme and text is clearly the smallest syllable change letter I found added before a line of text from Romans through the end of the uncial text. Every other instance of C<sup>43</sup> or any other letter<sup>44</sup> was noticeably larger. Most would have overlapped the distigme, confirming Willker’s judgment. Willker’s judgment is incompatible with Head’s thesis that all distigmai are late, from the time of de Sepúlveda.

## OTHER MARGINALIA

<sup>41</sup> All except 1345 B11, 1346 B 40, 1346 B 19, 1350 B 18, 1356 B 24, 1370 A 32. Cf. note 1.

<sup>42</sup> Willker includes full sized and enlarged photographs of this at <http://www-user.uni-bremen.de/~wie/Vaticanus/squeezed.html>.

<sup>43</sup> 1449 B 7, 1456 A 10, 1456 B 36, 1458 B 3, 1465 B 5, 1473 B 17, 1481 A 21, 1493 C 32, 1514 B 16, 1517 A 17.

<sup>44</sup> 1451 B 31, 1452 C 7, 1463 B 42, 1465 A 49, 1469 B 30, 1499 C 14, 1504 A 3, 1506 A 20.

Head appeals to six other marginalia that he alleges to confirm “that the distigmai are late additions to the margins of Codex Vaticanus,” but none of them give clear support for this, whereas several provide evidence against his thesis. First, he states that the liturgical note symbolizing **A P X H**, at 1409 C 11 interferes with a distigme at 1409 C 10 and a similar symbol at 1471 A 6 interferes with the distigme at 1471 A 4. Both distigmai, however, are in locations typical to distigme. The distigme at 1471 A 4 is two lines above the liturgical note, which is too far away to affect the distigme position. The distigme at 1409 C 11 is actually farther left than the distigme just two lines above it, whereas if its scribe had positioned it to avoid interference with the liturgical note, it would have been farther right like the preceding distigme. Furthermore, the **X** at 1471 C 10 is positioned as usual<sup>45</sup> above the slanted **P** in **A P**, whereas the **X** at 1409 C 10 is midway between the two letters **A P**. This indicates that the **X** at 1409 C 10 was adjusted left to avoid overlapping the distigme. This provides evidence that it was written after the distigme, the opposite of Head’s contention. In this case, Head’s own evidence, carefully examined, undermines his thesis.

Second, Head appeals to “marginal notes normally taken to signal pious approval of the contents of the passage” as interfering with the position of a distigme at 1408 B 9,<sup>46</sup> 1416 C,<sup>47</sup> and 1426 C.<sup>48</sup> Each of these distigmai, however, is in a customary distigme location. To support Head’s thesis, they would have to have been displaced in some way. Consequently, they provide no clear support for Head’s thesis. To the contrary, if the position of the distigme at 1408 B 9 had been influenced by this pious symbol, it should be farther right, like the other two distigmai on this page. Its position this far left is more natural if it was written prior to the adjacent pious symbol rather than after it. Consequently, it is evidence against any thesis that all distigmai are later than these pious symbols.

Third, Head states, “In one significant passage, a dittography has resulted in the same passage being copied out twice. The distigme is placed only against the second, re-inked passage, suggesting the distigme was placed after the re-inking (dated by Tischendorf to the tenth or eleventh century).” It appears, however, that this dittography was noted at the time of the original production, since each line of the duplicated text is surrounded by deletion hooks that appear from the 1999 facsimile to match the apricot color of the original ink. These marks clearly guided the reinforcer to retrace only over the text not marked as deleted text. The scribe

---

<sup>45</sup> The **X** is also positioned predominantly over the slanted **P** at 1404 A 18, 1405 A 35, 1406 A 28, 1407 A 39, 1408 A 26 and even on the right side of it at 1388 C 19. Only when the **P** is vertical, as at 1393 A 27 (right side) and 1394 B 31 is the **X** more likely to be centered between the **A P**, and even when it is vertical it may be more over the **P**, as at 1384 C 39 and 1393 C 41, or directly over the **P** at 1396 B 8 (right side).

<sup>46</sup> If this distigme’s position had been influenced by this pious symbol, it should be farther right, like the other two distigmai on this page. Its position this far left is evidence that it was written prior to the adjacent pious symbol, not after it, so it is evidence against Head’s thesis.

<sup>47</sup> There are distigmai at lines 8 and 27, but neither is near another mark. Perhaps Head means 1416 B 16 or 25, but they both show through from the other side of the vellum.

<sup>48</sup> There are distigmai at lines 11 and 32, but both are in a normal distigme position. Perhaps Head refers to the overlapping of the distigme and the faint sweeping stroke at 1426 C 32, but since both are in their standard positions, it is unclear which was written first.

who added the distigme at 1479 B 39 naturally did the same. Therefore, this instance should not be appealed to as evidence that its distigme is late. Since, however, the dark chocolate brown color and intensity of the ink of the re-inking appears to be a perfect match for the adjacent distigme, this does constitute evidence that the distigme (to be more precise, probably its re-inking) should be dated at the same time as the re-inking, which is incompatible with any thesis that all Vaticanus distigmai were written after the Middle Ages.

Fourth, Head states, “the famous marginal comment at Heb 1.3 seems to have caused the displacement of [a] distigme to the right hand margin.” The position of this distigme on the right side of the margin of 1512 B 17 is naturally explained, however, by the textual variant noted in NA<sup>27</sup> of the insertion of  $\eta\mu\omega\nu$  just four letters from the end of this line in  $\aleph^2$ , D<sup>1</sup> H 33 1881. Not only is there room for a distigme on the left without interfering with the marginal comment, positioning of distigmai on the right side of a line is a weak indicator of non-originality in any case, as argued in the final paragraph before the conclusion of this paper. Willker, asking why this distigme is on the right side, judiciously states, “Nobody knows for sure.”<sup>49</sup> Head, however, draws a conclusion not only about this distigme, but that “the distigmai ... are later than a thirteen-century marginal comment.”

Fifth, Head states, “on one occasion [a] distigme seems to be placed in order to avoid interference with a large initial letter.” Presumably, he refers to the distigme on the right side of 1277 C 3 (Mark 1:1) or the right side of 1443 C 3 (Jude 1), but both are by the far right column of the open codex, where distigmai are normally on the right hand side. In any event, the last word of 1277 C 3,  $\Upsilon\omega$ , is replaced with  $\Upsilon\omicron\iota\varsigma$  in manuscripts A W f<sup>13</sup>  $\aleph$  vg<sup>ms</sup> sy<sup>h</sup> (bo<sup>mss</sup>); Ir<sup>lat</sup>, and the end of the last line of 1443 C 3,  $\text{H}\Gamma\text{A}[\text{ΠH}]$  is replaced in P  $\aleph$  with  $\text{H}\Gamma\text{I}\text{A}[\text{C}]$  according to NA<sup>27</sup>, so position after the end of the line should be expected. Head may, however, refer to two very faint dots at 1499 A 3, but Willker is probably right to regard this as offset from 1498 C 3.<sup>50</sup> Both pairs of dots have the same orientation, the outer dot being lower than the inner dot, the offset at 1499 A 3 is much lower than typical distigmai, their location on the page makes it clear that this is merely offset, and NA<sup>27</sup> lists no textual variant on this line. Or Head may refer to the mark on the right side of text between 1502 C 1–2 or the two faint dots at 1506 C 2, but both of these are merely ink that bleeds through<sup>51</sup> or shows through from the other side of the vellum. None of these provides any evidence of a distigme being placed to avoid interference with a large initial letter.

Sixth, following Curt Niccum,<sup>52</sup> Head states that the distigmai “are later than the fifteenth-century minuscule text of Hebrews” based on “the presence of at least one distigme on the fifteenth century minuscule page.” Skeat is probably correct, however, that the minuscule

---

<sup>49</sup> Wieland Willker, “Codex Vaticanus Graece 1209, B/03: The Umlauts: A textcritical complaint” at <http://www-user.uni-bremen.de/~wie/Vaticanus/note1512.html>.

<sup>50</sup> Willker, <http://www-user.uni-bremen.de/~wie/Vaticanus/imprints.html> calls it an “imprint.”

<sup>51</sup> The ink that bleeds through on the right side of 1502 C 1–2 comes from the hole in the vellum at the top of the large initial at 1409 A 1–2, through which both normal and red ink penetrated.

<sup>52</sup> Curt Niccum, “The Voice of the Manuscripts on the Silence of Women: The External Evidence for 1 Cor 14.34–5,” *NTS* 43 (1997): 242–55. For a detailed critique of Niccum’s argument, see Payne, *Man and Woman*, 235–40.

leaves appended to Vaticanus replaced damaged uncial leaves.<sup>53</sup> On the first page of the minuscule text there is only one distigme by its first column (1519 A 12 by Heb 9:18–19), two much smaller, non-horizontal, raised dots of undetermined purpose by its second column (1519 B 12 by Heb 10:1) and also a chapter break symbol<sup>54</sup> at the beginning of Hebrews 10 (1519 B 8). Both the distigme and chapter symbol mimic the form of these symbols in the preceding uncial text, and both occur in the minuscule text only here.

The simplest explanation for distigme and the chapter break symbol near the beginning of the first minuscule leaf is that, in order to preserve these markings, a scribe copied both of these symbols from the damaged uncial leaf into their corresponding positions in the first minuscule page that replaced it.<sup>55</sup> Niccum objects that if a scribe had copied these symbols from a torn leaf, he also would have copied other original markings such as paragraphoi.<sup>56</sup> He assumes that paragraphoi were on whatever then remained of this damaged uncial page. This is a precarious assumption since there is only one paragraphos in the previous complete uncial page, and all three distinctive features occur in a one-inch-by-four-inch portion of the first minuscule page (4 of the 110 square inches of a full page). It is also doubtful that someone like de Sepúlveda, with the scholarly care and observant eye necessary to document textual variants, would not only mark up this very ancient manuscript but would continue to note textual variants even after the change from uncial to the obviously different and later minuscule text. My explanation following Skeat, however, accounts for this naturally just as it explains the chapter symbol, namely that they were copied from the damaged uncial leaf into corresponding positions in the minuscule text. In contrast, Head's thesis provides no explanation for the chapter symbol.

Furthermore, the text where the only distigme occurs in the minuscule text was the standard reading at the time it was written and so probably would not have been marked as a variant reading at that time. My text of Erasmus's Greek NT has the identical text that is in the minuscule text of Vaticanus next to this distigme, so it appears that Erasmus's Greek text would not account for this distigme in any event.

In summary, none of the examples Head adduces from these six other categories of marginalia clearly support his thesis, but three undermine his thesis, several exemplify incorrect analysis of the data, and others raise questions that his thesis does not answer.

#### JUAN GINÉS DE SEPÚLVEDA (1494–1573)

When I heard Head's paper I received the impression he was asserting that 92% of the lines marked with a distigme in the Gospels of Vaticanus differ from the corresponding text in Erasmus's Greek NT. Apparently Wasserman received a similar impression, since he writes,

---

<sup>53</sup> T. C. Skeat, "The Codex Vaticanus in the Fifteenth Century," *JTS* 35 (1984): 454–65. Reprinted in *The Collected Biblical Writings of T. C. Skeat*, (ed. J. K. Elliott; NovTSup 113; Leiden: Brill, 2004) 122–34.

<sup>54</sup> Compare the chapter symbols at 1518 B 5 and C 20, 1517 A 40 and C 6, and 1516 B 30.

<sup>55</sup> This fits Skeat's understanding that the minuscule leaves appended to Vaticanus probably replaced damaged uncial leaves, "The Codex Vaticanus," 454–65.

<sup>56</sup> Niccum, "Voice," 245.

“Peter had compared the published text of Erasmus reflecting MSS available in his time and had found that in the gospels there was a 92% match between Erasmus[’s] edition and the distigmai. If one includes the notes in Erasmus the rate goes up to 98%! This supports Niccum[’s] thesis. ... Head thinks the 98% match with Erasmus is the death-knell of Payne’s theory.”<sup>57</sup> Even now that I have Head’s paper, I find several statements that seem to imply that Head was referring to variant readings between Erasmus’s Greek text and Vaticanus: “[de Sepúlveda’s] comparison between Erasmus’s edition and this most ancient manuscript ... Vaticanus. ... I believe this confirms the late date of the distigmai in the margins of Vaticanus, and even provides us with a name and setting of the person responsible.”

Listening to Head’s paper, I found this assertion of 92% / 98% correspondence between distigmai and variants in Erasmus’s text the most compelling part of his argument. I was puzzled, however, how a single Greek text (Erasmus’s) could have a higher percentage of significant variants<sup>58</sup> than all the variants in NA<sup>27</sup> combined. Since Head did not identify which edition of Erasmus’s Greek NT gave these percentages, I used what I have in my library, a reprint of Erasmus’s Greek NT with his Latin translation printed in Basil by Nicolaum Bryling in 1553, to check whether these percentages accurately represent the frequency of textual variants between Vaticanus and Erasmus’s Greek NT. As a test page I used the first Vaticanus page Head displayed in his talk, page 1428, containing nine distigmai. I found that Erasmus’s Greek text varies from Vaticanus in only four of the nine distigmai lines on that page, namely 44% of them.<sup>59</sup> This percentage is only slightly higher than the frequency of textual variants in random lines in Vaticanus and so provides very weak support for the idea that a comparison with Erasmus’s NT text explains the presence of distigmai. This percentage is far closer to the 35% of random lines<sup>60</sup> in Vaticanus that contain a significant variant<sup>61</sup> than it is to either the percentage of NA<sup>27</sup> variants in lines by a distigme adjacent to a bar/obelus (24 out of 28 lines<sup>62</sup> = 86%) or the percentage of NA variants in lines by an apricot color distigme (36<sup>63</sup> of 51 lines = 71%).

Since Head repeatedly associates Erasmus’s text with manuscripts of that period, I also compared how many of the fifty-one apricot color distigmai are by a line where NA<sup>27</sup> lists a variant in the Majority text (℞). In only 23 of these 51 lines does NA<sup>27</sup> list ℞,<sup>64</sup> so even if Erasmus’s text has a textual variant in every one of these, these would constitute only a 45%

---

<sup>57</sup> Cited from [http://evangelicaltextualcriticism.blogspot.com/2009/11/sbl-new-orleans-2009-i-peter-head\\_22.html](http://evangelicaltextualcriticism.blogspot.com/2009/11/sbl-new-orleans-2009-i-peter-head_22.html).

<sup>58</sup> As judged by the variants identified in the NA<sup>27</sup>.

<sup>59</sup> The four lines in Vaticanus with a different text in Erasmus’s text are in James 3:2–3, 5, 6, and 12b. The five without a variant are in James 3:7, 12a, 15, 17 and 4:4.

<sup>60</sup> Based on the 540 control lines identified in the table in Payne, “Fuldensis,” 253.

<sup>61</sup> As judged by the variants identified in the NA<sup>27</sup>.

<sup>62</sup> See the table in Payne, “Fuldensis,” 253 plus one I had missed, 1332 C 20 at Luke 14:24.

<sup>63</sup> The NA<sup>25</sup> lists a variant in two of these that are not listed in the NA<sup>27</sup>: 1277 C 19 (Mark 1:5) and 1356 B 24 (John 5:25). Cf. Payne and Canart, “Distigmai.”

<sup>64</sup> The 23 are listed in note 68. This takes into account NA<sup>27</sup> convention stated on page 12\* that “℞ has the status of a consistently cited witness of the first order. Consequently in instances of a negative apparatus, where support for the text is not given, the reading attested by ℞ may safely be inferred: if it is not otherwise explicitly cited, it agrees with *txt* (= the text).”

correlation, a very low correlation compared to my own tests demonstrating a statistically strong correlation between Vaticanus distigmai and significant textual variants as listed in the NA<sup>27</sup>. Similarly, Willker writes, “Did Peter say 92% are TR variants? Compared to what? Vaticanus? Vulgate? NA? — I would like to see a table. In my count only about 50% are Majority/TR variants (vs. NA).”<sup>65</sup> These comparisons indicate that there is a very weak correlation between distigmai and significant textual variants in Erasmus’s Greek NT text compared with a very strong correlation between distigmai and textual variants as listed in the NA<sup>27</sup>. I and, apparently, Wasserman misunderstood and, consequently, were misled by Head’s 92% and 98% figures into thinking that there is this incredibly high correlation between distigmai and variants in Erasmus’s Greek text, when in fact there is not.

Now that I have a copy of Head’s paper, however, I realize that he was not using the term “textual variant” as I was, to refer to different Greek texts, but to differences between Greek *and Latin* texts: “92% of all the distigmai in the Gospels match passages of variation between that exact line of Vaticanus and the Greek and/or Latin text of Erasmus. If we further take account of variant readings noted by Erasmus in his Annotations (again offering contemporary manuscript evidence) this rate extends to 98%.” Head’s conclusion further broadens the pool of comparison, “Sepulveda carefully compared Vaticanus with other manuscripts in Greek and Latin, and with Erasmus’s edition. Comparison with sixteenth-century witnesses accounts for 98% of the distigmai in the Gospels.”

It is inappropriate to include Erasmus’s *Latin* text as a basis for identifying *Greek* textual variants between Erasmus’s text and Vaticanus. Head states that he agrees with me that “the distigmai mark places of textual variation between Vaticanus and other texts known to the dotter.” I assumed when I heard this that Head, like me, was referring to textual variants that could help establish the original form of the Greek text or otherwise explain the development of the Greek NT text. Erasmus’s Latin text is not a reproduction of any other Latin text nor does it have any independent value in establishing the original form of the Greek NT text or its subsequent development. Consequently, if one is looking for textual variants between Vaticanus and Erasmus’s text, the only text of Erasmus that is relevant is his Greek text. It makes no sense to use Erasmus’s derivative translated Latin text as a basis for finding textual variants between Erasmus’s Greek text and Vaticanus since the directly comparable text is on the facing page.

How does one identify textual variants between a Greek text (Vaticanus) and a Latin text? The only way I can imagine is to look for Latin translations that do not accurately reflect the Greek text and to presume that a differing Greek text caused them. I recently had an experience that shows how unreliable a translation can be for making judgments regarding textual variants. I emailed the chairman of the NIV revision committee a document identifying over 100 instances where the NIV text does not accurately reflect the underlying Greek in passages in Paul’s letters related to the ministry of women in church. If I had concluded that all or most, or even some, of these translation errors indicated the NIV text was based on a Greek text other than the one I used for my critique, I would have been wrong, since the NIV translators used the same NA and UBS Greek texts that I used to make my criticisms. Consequently, one

---

<sup>65</sup> Cited from [http://evangelicaltextualcriticism.blogspot.com/2009/11/sbl-new-orleans-2009-i-peter-head\\_22.html](http://evangelicaltextualcriticism.blogspot.com/2009/11/sbl-new-orleans-2009-i-peter-head_22.html).

cannot assume that differences in translation, whether English or Latin, necessarily or even usually identify underlying Greek textual variants.

Since my initial comparisons of Erasmus's Greek text do not produce anywhere near a 92% correlation with textual variants in distigmai lines, I have serious doubts about Head's assertion, "A careful investigation of the Gospel text of Vaticanus with a [distigme] (in the Gospels) and the Latin and Greek texts of Erasmus by my colleague Leslie McFall resulted in a 92% match rate."

In order to be convincing, Head will need to establish that a very high percentage of the lines in Vaticanus next to a distigme have a significant textual variant in that same portion of text in Erasmus's *Greek* NT text. Such a tabulation should not include variations in spelling, since if de Sepúlveda were including things that minor, there would probably be thousands of distigmai in Vaticanus. If minor variants are included, the percentages could not be fairly compared with the percentages I have found of significant textual variants of the sort that the NA<sup>27</sup> identifies. Furthermore, since distigmai occur throughout Vaticanus, if de Sepúlveda is the source of all the Vaticanus distigmai as Head's thesis states, it would mean that de Sepúlveda probably compared the entire Greek NT text of Erasmus with Vaticanus. Do the distigmai mark all or virtually all of the locations where there are textual variants between Vaticanus and Erasmus's text? To the degree that differences between Erasmus's text and Vaticanus are not marked by distigmai, Head's thesis is weakened.

One should expect a significantly higher frequency of textual variants between Erasmus's Greek text and Vaticanus in distigme lines than elsewhere in Vaticanus. This is evident, for instance, in the far higher incidence of textual variants between  $\mathfrak{M}$  and Vaticanus in the lines of text by the fifty-one apricot color distigmai than in random Vaticanus lines. The NA<sup>27</sup> identifies a significant textual variant in  $\mathfrak{M}$  in 23<sup>66</sup> of the 51 lines of text by apricot color distigmai that Canart identifies as matching the color of the original ink, namely 45% of them. In contrast, the NA<sup>27</sup> identifies textual variants, including  $\mathfrak{M}$  ones, in only 35% of random text in Vaticanus.<sup>67</sup> If the variants in random lines were restricted to variants only in  $\mathfrak{M}$ , the percentage would be much lower than 35%. Thus, simply showing a higher incidence of textual variants in Erasmus's text where Vaticanus has distigmai than in random Vaticanus text does not indicate that Erasmus's text was the source of those distigmai, or for that matter, that any other  $\mathfrak{M}$  text was. If McFall and Head include as textual variants minor variants that the NA<sup>27</sup> does not list, the correspondence between distigmai text and Erasmus's Greek text would obviously exceed 45%. If they add to this anything they construe to be a textual variation between Erasmus's Latin text and Vaticanus, then, of course, that percentage could skyrocket and still not indicate that Erasmus's text was the source of those distigmai.

---

<sup>66</sup> 1243 B 21, 1277 C 19, 1279 B 1, 1279 C 41, 1287 C 29, 1296 A 14, 1300 A 39, 1308 B 27, 1309 A 23, 1332 B 15, 1339 A 42, 1342 C 41, 1345 B 11, 1346 B 40, 1349 B 19, 1351 A 6, 1357 C 1, 1370 A 32, 1382 C 39, 1401 C 41, 1459 C 41, 1466 B 6, 1499 C 42. Note 1 has a link to the list of all 51.

<sup>67</sup> Based on the 540 control lines identified in the table in Payne, "Fuldensis," 253.

Unless Head clearly defines what he means by “textual variant” his figures of 92% or 98% are meaningless. How minor can differences be and still fit his definition of “textual variant”? Does his definition include spelling variants? Does it include differences that do not affect the meaning or message of the text? Does his definition include textual variants in other Greek texts available in Erasmus’s time? Does his definition include differences in Erasmus’s Latin text? Does his definition include textual variants in other Latin texts available in Erasmus’s time? If so, what constitutes a Latin textual variant? What assurance can he provide that he is not including as textual variants the sorts of differences that I regard as errors in translation in the NIV but are not based on any Greek textual variant? I simply cannot believe that there are far more significant textual variants between the distigmai lines in Vaticanus and Erasmus’s Greek NT text than in all Greek manuscripts combined, which is what I originally thought Head meant by 92% and 98% and which is what it should mean if he defines “textual variant” as I and most others have in discussions of the Vaticanus distigmai up until now.

Furthermore, there must be a control group using the same definition of “textual variant” in order to assess the significance of percentages of correlation. If “textual variant” is defined so broadly that 92% of distigmai lines have one, but a high percentage of non-distigmai lines also have such a “textual variant,” the 92% is not convincing evidence. In light of the already established higher correspondence between Vaticanus distigmai lines and textual variants in the Majority text (M) than in random lines of Vaticanus and the relationship between Erasmus’s text and the Majority text, one should expect a higher percentage of textual variants in Erasmus’s Greek text corresponding to Vaticanus distigmai text than in random selections of his Greek text. One should take this into account in any conclusions drawn from comparisons of Erasmus’s “distigmai text” to Erasmus’s “control text.” My own use of a control group of 540 random lines in Vaticanus was essential for getting significant chi-square probability results<sup>68</sup> confirming the correlation between significant textual variations and lines marked by distigmai.

Head writes with apparent approval that “Niccum noted that in 1533 J.G. Sepulveda had written to Erasmus about the results of a comparison between Erasmus’s edition and this most ancient manuscript ‘most diligently and accurately copied out in uncials’. De Sepulveda had, according to this letter, been comparing the text of Vaticanus both with Greek and Latin manuscripts extant in his time and with Erasmus’s edition, and on the basis of this study sent Erasmus a list of 365 readings, apparently where Vaticanus and the Vulgate agreed against the Greek text published by Erasmus.” Head’s paper lacks documentation that de Sepulveda added distigmai to Vaticanus or actually sent such a list to Erasmus.<sup>69</sup> The most obvious way for Head

---

<sup>68</sup> My calculations include Yates’ correction for continuity. Cf. Payne, *Man and Woman*, 241–42, and forthcoming, Payne and Canart, “Distigmai.” Cf. the summary above in footnote 1.

<sup>69</sup> This is questioned by Carlo M. Martini, *Il problema della recensionalità del codice B alla luce del papiro Bodmer XIV* (Analecta biblica 26; Rome: Pontificium Inst. Bibl., 1966), 8, note 20; who suggests that the existence of these readings was mentioned to Erasmus but that the list was never actually sent to him, cf. Stephen Pisano, “III. The Text of the New Testament,” pages 27–41 in the *Prolegomena* volume to *Bibliorum sacrorum graecorum Codex Vaticanus B: Bibliothecae Apostolicae Vaticanae Codex Vaticanus Graecus 1209* (Rome: Istituto Poligrafico e Zecca dello Stato, 1999), 21. The remaining copies of this set of the *Codex Vaticanus B* facsimile and its *Prolegomena* are available at [www.linguistsoftware.com/codexvat.htm](http://www.linguistsoftware.com/codexvat.htm).



to establish the thesis that de Sepúlveda penned all the distigmai in the process of comparing Erasmus's edition to Vaticanus, would be to compare the Greek NT text of Erasmus to Vaticanus and demonstrate the following two statements to be true:

1. Wherever there is a textual variant between these two texts, there is a distigme.
2. Wherever there is a distigme, there is a textual variant between these two texts.

My own preliminary comparisons of Erasmus's Greek NT text to Vaticanus distigme lines shows that neither of these is true, nor is either anywhere close to being true.

Furthermore, if de Sepúlveda himself penned all the distigmai in order to identify locations that differed from Erasmus's Greek NT text, as Head's thesis seems to postulate ("a comparison between Erasmus's edition and this most ancient manuscript"), why when he wrote to Erasmus did he speak of only 365 variants instead of 825<sup>70</sup>? This leaves more than half the distigmai unexplained and seriously undermines Head's thesis that "all are the product of the same process and of approximately the same date." Furthermore, this thesis although purporting to account for all distigmai, does not account the distigmai in the Vaticanus OT. Willker noted, "Even with the limited Rahlfs apparatus ... of 14 safe [distigmai] I have found variants for 10 of them."<sup>71</sup>

More fundamentally, if de Sepúlveda's intention were to oppose Erasmus's Greek NT, wouldn't it make far more sense for him to mark up a copy of Erasmus's Greek NT for this purpose than to mark up irreplaceable manuscripts? If someone noted variants directly on multiple original manuscripts, he or she would have to go through each manuscript to tabulate a total. But if that person noted the variants directly in a copy of Erasmus's Greek NT, that single source would hold all the suspect readings and would permit that person to tabulate those with relative ease. Furthermore, since according to Head, de Sepúlveda's concern was to establish errors in Erasmus's Greek NT, that is not only the most logical place to note them, Erasmus's text is the only text that would include all the suspect readings.

By Head's view de Sepúlveda had the audacity to pen "perhaps 825" distigmai in Codex Vaticanus. Furthermore, Head's thesis requires that de Sepúlveda not only wrote on virtually every leaf of Vaticanus, he turned pages containing "more than fifty" of them while the ink was so wet these distigmai offset onto the facing page! It is hard to imagine someone in de Sepúlveda's position treating Vaticanus in such a careless manner to note variants with Erasmus's or other texts.

Nor is it likely that a sixteenth-century scribe would mark so many other Vaticanus readings as textual variants that were standard at that time. Nor does Head's conjecture explain the distigmai that occur where no known manuscript has a significant variant. Such occurrences are natural, however, if the original scribe was noting variants in the fourth century since most, if not all, of the manuscripts available to the scribe of Vaticanus are no longer extant. Furthermore, neither Niccum nor Head gives any evidence that fifteenth or sixteenth century scribes

---

<sup>70</sup> The number of distigmai by Head's reckoning.

<sup>71</sup> <http://www-user.uni-bremen.de/~wie/Vaticanus/observations-OT.html> confirmed 2/11/2010. Willker's list now cites 17 distigmai in the LXX of Codex Vaticanus at <http://www-user.uni-bremen.de/~wie/Vaticanus/umlauts-OT.txt>.

conventionally used distigmai to note textual variants or that de Sepúlveda was even aware of this use for distigmai.

Nor does Head explain what manuscript source at that time would account for the diversity of textual variants represented by the distigmai. Willker observes that: “In general there is **no CLEAR pattern** in the witness support for the various umlauts. We have support from - D only, - Byz only, - D + Byz, - P46 only, - some minuscule MSS only. IMHO this indicates that not one single MS has been used for comparison, but more than one.”<sup>72</sup> How can Erasmus’s text by itself or in combination with other sixteenth century texts account for variants that are attested in, e.g., D alone or  $\mathfrak{P}^{46}$  alone?

Furthermore, Curt Niccum has told me that he does not believe that de Sepúlveda penned the distigmai in Codex Vaticanus, in spite of his earlier statement, “Evidence suggests Sepulveda introduced these [distigmai]. ... Sepulveda must have shared ... the reading  $\kappa\alpha\tilde{\upsilon}\delta\alpha$  at Acts 27.16 ... attested only in Vaticanus and Sinaiticus<sup>corr</sup>.”<sup>73</sup> This reading, however, is also in  $\mathfrak{P}^{74}$ , 1174, it, etc., cf. UBS<sup>4</sup>. This error is pivotal since Niccum argued from this reading’s rarity that distigmai “originated with [de Sepúlveda].” Unless Niccum has changed his view again, it is incorrect to say that it is Niccum’s position that de Sepúlveda penned the distigmai.

## IDENTIFYING LATER DISTIGMAI

Head’s paper has raised a valuable question: What factors help to identify which distigmai are not original or re-inked? Eight factors offer the best evidence that a distigme did not originate at the time of the original production of Vaticanus, as judged by the standard of the fifty-one apricot color distigmai that Canart confirmed to match the ink color of unreinforced text on the same page:

1. Dot(s) that are not circular.<sup>74</sup>
2. Irregular size dot(s) in the distigme.<sup>75</sup>
3. Non-horizontal orientation of the dots.<sup>76</sup>
4. Irregular spacing between the dots.<sup>77</sup> All of the apricot distigmai are within 1 mm of each other.

---

<sup>72</sup> Willker, “Umlauts: Distribution of the Umlauts,” exactly reproducing Willker’s bold text.

<sup>73</sup> Niccum, “Voice,” 245, note 20. “One can only conclude that some scholar after 1400 compared Vaticanus with another text, noting places of variation and/or agreement in the margin.”

<sup>74</sup> The clearest exception to this among the apricot color distigmai is the slightly elongated right dot in the distigme at 1279 C 41.

<sup>75</sup> The clearest exceptions to this among the apricot color distigmai are the faint distigmai at 1264 C 29 and 1345 B 11, which may appear small due to the faded ink, and the enlarged left dot of 1261 A 21, which the scribe’s pen may have touched more than once or slid slightly.

<sup>76</sup> Slight variation is common, e.g. the right dot slightly higher in 1336 A 22, 1351 A 6, 1370 A 32, 1468 B 3, and 1475 B 11 and the left dot slightly higher in 1261 A 21, 1357 C 1, 1380 A 26, 1419 B 36. The greatest such divergence from horizontal in an apricot distigmai is 1351 A 6.

5. Irregular separation from the Greek text in the adjacent column. This is a fairly weak indicator since without any possible interference from other marks in the margin, apricot color distigmai range from to within 1 mm (1243 B 21) to 8.5 mm (1264 C 29).<sup>78</sup>
6. Irregular placement relative to the base line. Most apricot distigmai are at mid character height, but one (1380 A 26) is slightly higher than the letters in the adjacent line of text. Six are near the top of the letters in the adjacent line of text<sup>79</sup> and three are near the bottom.<sup>80</sup>
7. Juxtaposition next to more than one other dot or other marking.<sup>81</sup>
8. Distigme ink color that does not match either the original apricot ink color of the codex or, secondarily, the dark chocolate brown of the medieval reinforcement.<sup>82</sup>

---

<sup>77</sup> These are comparatively consistent. The apricot color distigmai with dots closest together is at 1308 B 27. Other close ones are 1243 B 21 and 1264 C 29, but none overlap. The farthest apart is 1261 A 21, but 1380 A 26, 1381 C 26, and 1473 A 6 are separated a similar distance.

<sup>78</sup> Three are 4 mm from text (1279 B 1, 1287 C 29, 1296 A 14), two are 4.5 mm from text (1332 B 10, 1457 B 24), two are 5 mm from text (1382 C 39, 1499 C 42), one is 5.5 mm from text (1401 C 41), two are 6 mm (1279 C 41, 1332 C 20), one is at 7 mm (1352 A 40), and one is at 8.5 mm (1264 C 29), all with no interference from other marginalia. One is at 9 mm with a diplai separating it from the text on 1309 A 23. This is not surprising in light of the evidence listed above that diplai were written concurrently with the text and prior to distigmai. This is the only distigme on its page so its positioning does not look out of place. One at 1277 C 19 is 9.5 mm from text and is above and to the right of a  $\bar{\Delta}$  that shows through from the reverse side of the vellum. This, however, may be just coincidence since the distigme closest to it, at 1277 C 3 also extends significantly into the margin (over 7 mm) with no interference from any other mark, and both it and the distigme at 1277 C 3 lie on a level with the very top of preceding text and so are in harmonious positions. Perhaps, however, Willker is correct that 1277 C 19 is an offset from 1276 A 19, which is 7.5 mm from text; see note 81. If so, then the original distigme at 1276 A 19 left an apricot color mirror impression at 1277 C 19, and only the original distigme at 1276 A 19 was re-inked with dark chocolate brown ink, not its mirror impression, which perhaps because of its faintness was missed by the reinforcer.  $\Theta$  has  $\theta\epsilon\omega\pi\omicron\upsilon\sigma\alpha\iota$  in the middle of 1276 A 19, before rather than after  $\acute{\alpha}\pi\omicron\ \mu\alpha\kappa\rho\acute{\theta}\epsilon\nu$  according to Reuben J. Swanson, *New Testament Greek Manuscripts: Variant Readings Arranged in Horizontal Lines against Codex Vaticanus* (Sheffield: Sheffield Academic Press, 1995), 288.

<sup>79</sup> 1264 C 29, 1296 A 14, 1345 B 11, 1351 A 6, either 1380 A 26 or 1381 C 26 (since one is an offset), and 1475 B 11. As note 81 discusses, Willker regards 1277 C 19 to be an offset, cf. <http://www-user.uni-bremen.de/~wie/Vaticanus/imprints.html>.

<sup>80</sup> 1300 A 37, 1300 A 39, and 1466 B 6.

<sup>81</sup> Although there are no clear examples of this among the apricot distigmai, there are four instances where it is possible that the pen slipped slightly or made double contact with the vellum: 1261 A 21, 1287 C 29, 1380 A 26, and 1401 C 41.

<sup>82</sup> The 1968 color reproduction of the NT of Vaticanus is not reliable for assessing ink color. Even different volumes of that edition vary dramatically. I confirmed one distigme that was red in one volume and brown in another. The 1999 edition is excellent, but only the original MS permits definitive judgments. Ink color that matches the re-inking argues against a date after the Middle Ages. In light of evidence above, page 6 and notes 9–13 that the re-inking included

Because this is a handwritten manuscript, some variation is inevitable. The fifty-one apricot color distigmai are only a small fraction of them all. Consequently, it should not be surprising if some distigmai originally in apricot color ink but later re-inked have characteristics that exceed the ranges of the characteristics above. Nevertheless, the sharper the contrast from the ordinary shape and position of distigmai and the more points of dissimilarity, the stronger is the case against a particular distigme going back to the original production of Vaticanus, especially when one or more characteristics lie outside the range of any of the apricot color distigmai. The few cases cited above with convincing evidence that the position of a distigme was changed in order to avoid interference with marginalia warrant regarding those distigmai as penned later than the interfering marginalia. The distigmai in these cases almost always have characteristics atypical of distigmai. This confirms the usefulness of these criteria for helping to judge which distigmai are not part of the original production of Vaticanus.

Though never determinative, lack of an NA<sup>27</sup> variant in the line adjacent to a distigme may add to other evidence that a distigme is not original. This can only be used as weak corroborating evidence, however, since approximately 35% of Vaticanus lines lacking distigme contain an NA<sup>27</sup> variant, and since approximately 29% of the lines adjacent to an apricot color distigmai contain no NA variant.

I have not included position on the “wrong” side of a column in the eight characteristics of non-original distigmai for three reasons:

1. There are four cases like this in apricot color ink where no other marginalia compete for space on the “correct” side.<sup>83</sup> Consequently, being in such a position does not put a distigme outside a fairly normal range of positions occupied by apricot color distigmai. One should not use any of the above criteria by itself to exclude the originality of a distigme, especially if four apricot-color distigmai share that characteristic. Most importantly, it is wrong to assume that just because distigmai are on the less common side of text, they were forced there by some other previously written marginalia, as Head repeatedly appears to do. To do this would be inconsistent with the application I recommend for each of the eight criteria for dating distigmai later than the original production of Vaticanus.
2. It is perfectly reasonable that a scribe might want to place a distigme on the side of a line closest to where the textual variant occurs, and this correlation does in fact repeatedly occur.<sup>84</sup>
3. Some lines have a distigme both on its right side and its left side. In one instance with no interference from other marginalia, 1339 C 42, the distigmai on each side of the line matches the color of the original ink of the manuscript. Whether this indicates two separate variants or

---

distigmai as well as text, it is perhaps most judicious to regard distigmai whose ink appears to match the adjacent re-inked text as having been re-inked as well, unless there is evidence that they are later. In cases where no apricot color ink is visible, confirmation awaits scientific testing, such as multi-spectral imaging or X-ray Fluorescence imaging. Perhaps such analysis will one day confirm which dark chocolate brown distigmai were traced over original apricot color distigmai and whether some were added later.

<sup>83</sup> 1243 B 21, 1339 C 42, 1350 B 18, and 1351 A 6.

<sup>84</sup> Cf. the examples listed above, pages 13–14.

draws special attention to one, it shows that the scribe inserting it believed that it is acceptable to place a distigme on either side of a line.

Referring to the “wrong” side is misleading since it implies that this position does not conform to a consistent standard. This can be avoided by referring to it as the “less common” side.<sup>85</sup> This is especially important for Head since his use of the “wrong” side of text where there is no interference from other marginalia undermines his assertion that all distigmai constitute a unified system, the product of the same process and of approximately the same date.

## CONCLUSION

Distigmai were penned in the fourth or fifth century in the margins of hexaplaric Codex Colberto-Sarravianus (LXX G) and correspond closely to the shape and location of distigmai in Vaticanus. There is no dispute that these mark locations where the Hebrew Scriptures do not include the text in lines marked by distigmai. The conjunction of distigmai with metobeloi in the shape of a colon between letters in the midst of the LXX G body text proves that both were part of the original production of LXX G. Consequently, they prove the very early use distigmai to mark the location of textual variants, albeit of a specific sort. Correspondingly, they disprove each of the three assumptions that appear to motivate people to doubt the idea that distigmai go back to the original production of Vaticanus, namely that:

1. Scribes near the time of Vaticanus did not have the sophistication to be aware of textual variants.
2. Scribes near the time of Vaticanus did not have a system for noting variants.
3. The notation of textual variants is such a rare phenomenon that it could only have happened once.

The LXX G distigmai provide a simple explanation for the origin of the distigme symbol, namely that it is a simplified form of Aristarchus’s obelus as represented in what appears to be the earliest MS of Origen’s Hexapla (LXX G). This early use of distigmai in Origen’s Hexapla to mark the location of textual variants that depart from the Hebrew Scriptures provides a natural transition to its use to mark the location of Greek textual variants as part of the original production of Vaticanus. This is a straightforward logical expansion of the distigme’s use and one that requires less sophistication and less linguistic skill than Origen’s use. Furthermore, it explains naturally why fifty-one of the distigmai match the color of its original ink.

Head provides convincing evidence that in three instances a dipole was partially obscured by a distigme, and in each of these three instances other factors give evidence that the distigmai were later additions (page 10), not part of the original production of Vaticanus. Head, however, provides no indisputable evidence that any distigme should be dated after any small number. The only instance Head cites of a distigme in a non-standard position relative to a large number, namely on the outside of it at 1455 B 31, also shares many other signs of not being part of the original production of Vaticanus (page 15). These, the only four instances where Head provides

---

<sup>85</sup> The fact that these instances are statistically less common can be helpful in cases of offset. Even though this factor is not decisive in itself, it can be a contributing factor in helping to assess which of two pairs of dots on exact opposite positions of facing pages is the deliberately penned distigme and which is just the accidental transfer of ink to the facing page.

compelling evidence of distigmai being late, demonstrate the usefulness of the criteria listed below for identifying which distigmai should not be dated at the time of the original production of Vaticanus. For his four astute observations and his calling attention to other evidence that might support a later dating of some distigmai Head deserves thanks.

The central error of Head's thesis is his apparent assumption that all distigmai were penned at the same time. By incorrectly stating that I agree with him on this point, he diverted attention from this highly improbable assumption. There is an abundance of evidence that all distigmai were not penned at the same time, including differences in ink color, as argued above on pages 4–9. Head conceals this by making a series of disputable assertions that give the false impression of a simple sequence of marginalia, each system of marginalia being completed before the next began. For example, Head asserts: “the small numbers are also secondary to the dipl[ai],” but although Head is correct that most diplai predate small numbers, there is evidence in three cases that a diple was penned after a small number (pages 12–13). Evidence that some diplai were penned after a small number certainly does not constitute proof that all diplai were written after all small numbers. Likewise, evidence that some distigmai were written later than some other category of marginalia does not prove that all distigmai were written later than every instance of that category of marginalia, and it certainly does not prove that all distigmai were written later than every other marginalia.

Similarly, the rewriting of so many small numbers around large numbers proves that these repositioned small numbers were written after the large numbers, which Head understandably regards as “added at a much later date.” Just because some small sectional numbers were written much later than others, does not constitute proof that all small numbers were written late, and certainly not that all small numbers were written at the same late time. Why, then, should one presume that all distigmai, which display far more diversity than diplai or small numbers, were written at the same time and, consequently, that all can be dated as late as the latest one?

Head changes the criteria of judgment on crucial issues, such as appealing to “the colour and faded nature” of diplai to “place these in the production stage of the codex,” but rejecting that “even indeed actual similarities of observed colour ... are a particularly good guide to the dating of dots.” In addition, Head vastly overstates the evidence for his thesis. Head asserts, for example that there are “sixteen places of interference between diple and distigme,” but of these, three have no diple, and in eight, the distigmai are in a typical distigme position (page 10).

Head asserts that the distigmai “are later than the two different types of chapter enumeration,” but he identifies no indisputable evidence of a small number affecting the position of a distigme. Head also asserts, “there is no evidence for the distigmai interfering with any” small number. There is, however, evidence that the distigme at 1278 B 12 affected the position of the small number  $\overline{\text{C}}$  (page 14). Head similarly asserts that “there is no evidence for the distigmai interfering with any” large number. Page 16, however, cites evidence that distigmai interfered with two large numbers. Head appeals to six other marginalia that he alleges to confirm “that the distigmai are late additions to the margins of Codex Vaticanus,” but none of them give indisputable support for this, whereas several undermine his thesis (pages 18–20).

Head's assertions about de Sepúlveda lack sufficient documentation, shift without clear definitions between Erasmus and other texts, and leave unexplained what Head means by Greek and Latin "textual variants." It is clear, however, that Head must not mean significant Greek textual variants of the sort I have identified from the NA<sup>27</sup> since they would not produce the 92% or 98% correspondence rate he claims. Head makes the bold proposal that de Sepúlveda, presumably in order to show errors in Erasmus's text, added "perhaps 825 distigmai" to the irreplaceable Codex Vaticanus. His thesis requires that de Sepúlveda not only wrote on virtually every leaf of Vaticanus, he carelessly turned pages containing "more than fifty" of them while the ink was so wet these distigmai offset onto the facing page of this manuscript that has a history of being jealously guarded. It also entails a man of de Sepúlveda's sophistication not noticing the change from uncial to minuscule text until after he had written at least one distigme in it. Furthermore, although purporting to account for all distigmai, this thesis does not account the distigmai in the Vaticanus OT. If de Sepúlveda's intention was to show where other manuscripts differ from Erasmus's Greek text, it would make far more sense for him to mark up a copy of Erasmus's Greek NT than to mark up irreplaceable manuscripts since only Erasmus's text would include all the readings he wished to challenge, and only then could he count the number of differences with relative ease.

In order to modify his thesis to account for all data that is incompatible with it, Head will have to abandon both his assertion that all distigmai were written at approximately the same time and that all distigmai were written late. Without these assertions, however, Head's thesis is stripped it of its power to deny the originality of distigmai written in ink that matches the color of the ink used in the original production of Codex Vaticanus.

In contrast, the Payne-Canart thesis is, first, that about fifty distigmai (excluding examples of offset<sup>86</sup>), namely the ones matching the apricot color of both the original text and original diplai, are part of the original production of Codex Vaticanus.<sup>87</sup> Second, it is that distigmai in ink that matches the medieval reinforcement should, unless there is evidence otherwise, be dated to that time. Whenever apricot color ink protrudes from under the edge of a distigme matching the color of the adjacent re-inked text, it can be reasonably regarded as part of the original production of Vaticanus that was re-inked later. The process of re-inking is abundantly attested for text and selectively attested for distigmai, namely where apricot color ink protrudes from under ink that matches the medieval reinforcement. Just as there are hundreds of

---

<sup>86</sup> Cf. above, note 81 regarding 1277 C 19, where the original may have been re-inked and the offset left to display apricot color ink. Distigmai with apricot color ink protruding from under a distigme should probably also be included, cf. above, notes 9 and 10.

<sup>87</sup> This has been accepted, e.g. by Christian-B. Amphoux, "Codex Vaticanus B: Les points diacritiques des marges de Marc," *JTS NS* 58 (2007): 447, 440-66. Unfortunately, Amphoux on page 445 appears to attribute to me the view that the distigme at the end of 1 Cor 14:33 marks the western text placement of these verses after 14:40. I argue, instead, that if the scribe's intention were to indicate the western variant that places verses 34-35 after 40, there should have been a corresponding distigme after 40 to identify the equally great difference in the text there. Since there is no distigme after 40, the distigme after 33 more naturally represents a text without verses 34-35. Cf. Payne and Canart, "The Originality of Text-Critical Symbols," 113 and Payne, *Man and Woman*, 233.

instances where portions of text or isolated letters were not re-inked and show the original ink of the codex, so also there are fifty-one instances where distigmai were not re-inked and whose ink matches the color of the original text. The vast majority, however, of both text and distigmai, were re-inked.

The abundant evidence of re-inking supports the working hypothesis that *unless there is evidence to the contrary*, including evidence from interaction with other marginalia and divergence from typical distigmai characteristics, the distigmai that match the color of the re-inking, even when no apricot ink is visible protruding from under them, may be tentatively regarded as from the original production of Vaticanus and later re-inked. This working hypothesis is distinct from the Payne-Canart primary and secondary theses and is perfectly open to any sort of contrary evidence that would support a different date for any of these dark chocolate brown distigmai. This includes, for instance, the evidence cited above that some distigmai may identify spelling changes made by a corrector of Vaticanus.<sup>88</sup> It is my hope that some sort of scientific analysis of the distigmai, such as multi-spectral imaging or X-ray Fluorescence imaging, may provide confirmation of the presence or absence of underlying apricot color ink.

Further investigation both as regards date and purpose is required regarding distigmai that do not correspond to either the original ink of Vaticanus or its medieval reinforcement. Finally, there are other dots that are not proper distigmai, such as vertical pairs or trios or strings or clusters of dots that do not fit the typical characteristics of distigmai. These deserve separate investigation and should not be assumed to have the same purpose as distigmai.

Head's paper attempts to repudiate the Payne-Canart thesis and the evidence we adduce for it from the matching apricot color of original text, most diplai, and fifty-one distigmai. Nevertheless, the Payne-Canart thesis is compatible with all the underlying data to which Head appeals. In contrast, the application of Head's principle of marginalia interference undermines his thesis in the many instances identified above. Head's paper also provides no explanation for the obvious differences in ink color among the distigmai and instances where distigmai appear to have been reinforced throughout Vaticanus and across its pages, including apricot color matching the original ink color and dark chocolate brown color matching the medieval reinforcement, or for why some distigmai have apricot color ink protruding from the edges of dark chocolate brown distigmai, or why one distigmai has one apricot color dot and one chocolate brown color dot (p. 6 and notes 9–10). Nor does it explain why there is a statistically overwhelming correlation between apricot color ink distigmai and significant textual variants of the sorts identified by the NA<sup>27</sup>. Nor does it explain the instances cited above where there is evidence that distigmai preceded other marginalia.

Thus, although Head's thesis that de Sepúlveda penned all the distigmai in Vaticanus is simple, it does not adequately account for the data. It is economical, but since much of the data contradict it, it is simplistic and should not stand. The famous aphorism attributed H. L. Mencken aptly describes Head's solution: "For every complex problem, there is a solution that is simple, neat, and wrong." The more comprehensive Payne-Canart thesis with its attention to

---

<sup>88</sup> Cf. above, pages 6–7.



variations in the marginalia, including variations in ink color, however, does justice to the data in all their variety and welcomes further insights.