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(with some stylistic changes, mainly in the notes.)

## AN EIGHTEENTH-CENTURY MANUSCRIPT: CONTROL OF THE SCRIBAL HAND IN *CLEMENT'S LETTER TO THEODORE*\*

*This article discusses Morton Smith's role as a self-professed manuscript hunter in uncovering the only known copy of Clement's Letter to Theodore, and critically assesses the existing studies on its handwriting. We argue that Stephen C. Carlson's analysis is flawed due to its dependence on distorted images, that Agamemnon Tselikas's study has a number of problems due to the unsuitability of applying standard palaeographic practices to a case of suspected deception, and that Venetia Anastasopoulou has made a sustainable case by arguing that Smith could not have imitated the difficult eighteenth-century script—a qualitative verdict strengthened by our quantitative study of the lack of signs of control. We conclude that the handwriting is indistinguishable from authentic eighteenth-century handwriting.*

*Cet article discute le rôle de Morton Smith comme dénichéur de manuscrits en raison de sa découverte de la seule copie de la Lettre de Clément à Théodore, et évalue critiquement les études paléographiques menées sur cette copie. Nous estimons que l'analyse de Stephen C. Carlson est hypothéquée par la confiance excessive que ce paléographe accorde à des photographies médiocres, que l'étude d'Agamemnon Tselikas présente l'inconvénient de ne pas appliquer les critères paléographiques usuels dans le cas de faux, et que Venetia Anastasopoulou a produit une étude solide, à nos yeux, en argumentant que Smith ne pouvait pas avoir imité l'écriture difficile du XVIII<sup>e</sup> siècle – un verdict qualitatif renforcé par notre étude quantitative sur l'absence de signes de contrôle. Nous parvenons à la conclusion que l'écriture du manuscrit ne peut être distinguée d'une écriture authentique du XVIII<sup>e</sup> siècle.*

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\* We wish to thank Scott G. Brown, Allan J. Pantuck, and David Blocker for their extensive criticism and helpful suggestions.

[262] Morton Smith (1915–1991) took three sets of photographs of *Clement's Letter to Theodore*, and left the original document in the tower library of the monastery of Mar Saba. He could not take the document with him since it was the property of the Greek Orthodox Patriarchate.<sup>1</sup>

This statement is worth making, because it cuts to the heart of the pervasive myth that the late Columbia professor of ancient history behaved deviously in the way he presented this document to his peers. As a recent commentator noted, an “academic folklore” has grown up around this topic, which has been handed down from scholar to scholar “like an esoteric tradition.”<sup>2</sup> This folklore includes the charge that Smith failed to secure access to the manuscript so that other scholars could corroborate his findings.<sup>3</sup> This in turn has been used to support the suspicion that Smith himself forged the text.<sup>4</sup> Following the death of Smith in 1991, these voices have become increasingly insistent, and the folklore has become increasingly ingenious.<sup>5</sup> Instead of passing on this academic hearsay, we have opted for an introductory statement that is most likely to be true and will allow us to place Smith’s actions in a more defensible framework. This will be discussed in more detail below.

With such doubts in the air, some scholars have taken a suspicious stance towards anything connected with the manuscript of this Clementine letter. The interplay between the developing folklore and the known facts has allowed even the standard, mundane practices of modern academic manuscript hunters to be presented as evidence of foul play. Consider, for instance, how Smith’s addition of his name and a number (#65) to the front of the printed book in which this manuscript was written (a standard cataloguing procedure) has been used to argue that the book actually

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<sup>1</sup> Morton SMITH, *The Secret Gospel: The Discovery and Interpretation of the Secret Gospel According to Mark* (New York: Harper & Row, 1973), 13.

<sup>2</sup> Scott G. BROWN, “Factualizing the Folklore: Stephen Carlson’s Case against Morton Smith,” *HTR* 99 (2006): 291–327, at 291.

<sup>3</sup> Quentin QUESNELL, “The Mar Saba Clementine: A Question of Evidence,” *CBQ* 37 (1975): 48–67, at 49–50. Smith and many others interpreted Quesnell’s criticisms in this article as insinuations that Smith had created the letter himself. Cf. Scott G. BROWN, *Mark’s Other Gospel: Rethinking Morton Smith’s Controversial Discovery* (ESCJ 15; Waterloo, Ont.: Wilfred Laurier University Press, 2005), 12, 35–36, 73.

<sup>4</sup> E.g. Jacob NEUSNER, *Are There Really Tannaitic Parallels to the Gospels? A Refutation of Morton Smith* (SFSHJ 80; Atlanta: Scholars Press, 1993), 29; Donald Harman AKENSON, *Saint Saul: A Skeleton Key to the Historical Jesus* (Oxford: Oxford University Press, 2000), 85.

<sup>5</sup> BROWN, “Factualizing the Folklore,” 291–293.

belonged to Smith;<sup>6</sup> or how Smith's "sudden [263] mood swing from 'worst expectations' to 'walking on air'" when he came across the letter has been used to "raise doubts about his truthfulness" and to profile him as evincing a personality disorder;<sup>7</sup> or how Smith has been suspected to have been working as a secret agent for the United Kingdom or the United States, rather than having been genuinely interested in the (monastic) libraries for antiquarian reasons.<sup>8</sup> In recent years, some scholars have also presumed that Smith was so devious as to conceal cryptic clues in both the manuscript and his writings about it that disclose his identity as the true author of the letter.<sup>9</sup>

An endeavour to put Smith and his practices into proper context has barely begun, for scholars have only recently begun to delve into Smith's archival remains.<sup>10</sup> There is

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<sup>6</sup> When interviewed by Lee Strobel, Craig A. Evans said that he found it strange that "Smith 65" was penned on the front of the book containing the Clementine letter, since you would not write in books "if you were a guest in somebody's library"; Lee STROBEL, *The Case for the Real Jesus: A Journalist Investigates Current Attacks on the Identity of Christ* (Grand Rapids, MI: Zondervan, 2007), 50–51.

<sup>7</sup> Peter JEFFERY, *The Secret Gospel of Mark Unveiled: Imagined Rituals of Sex, Death, and Madness in a Biblical Forgery* (New Haven: Yale University Press, 2007), 10, 243; based on Jeffery's writings and the discovery story of the manuscript in SMITH, *Secret Gospel*, 10–11, 18, 93, Donald Capps even ventured to make a diagnosis: Smith suffered from narcissistic personality disorder; Donald CAPPS, "The Diagnostic Question" (paper presented at the annual meeting of the SBL, New Orleans, November 21, 2009).

<sup>8</sup> Charles W. HEDRICK, "Appendix: Interview with Agamemnon Tselikas," in *Ancient Gospel or Modern Forgery? The Secret Gospel of Mark in Debate: Proceedings from the 2011 York University Christian Apocrypha Symposium* (ed. Tony Burke; Eugene, Ore.: Cascade Books, 2013): 60–66, at 61, 64–66 (#4, #25, #32).

<sup>9</sup> E.g. Stephen C. CARLSON, *The Gospel Hoax: Morton Smith's Invention of Secret Mark* (Waco: Baylor University Press, 2005), 33, 42–47, 58–64; Francis WATSON, "Beyond Suspicion: On the Authorship of the Mar Saba Letter and the Secret Gospel of Mark," *JTS* 61 (2010): 128–170, at 152–155.

<sup>10</sup> Guy G. STROUMSA, "Comments on Charles Hedrick's Article: A Testimony," *J ECS* 11 (2003): 147–153; Allan J. PANTUCK and Scott G. BROWN, "Morton Smith as M. Madiotes: Stephen Carlson's Attribution of *Secret Mark* to a Bald Swindler," *JSHJ* 6 (2008): 106–125; *Morton Smith and Gershom Scholem, Correspondence 1945–1982* (ed. Guy G. STROUMSA; Leiden: Brill, 2008); Allan J. PANTUCK, "Response to Agamemnon Tselikas on Morton Smith and the Manuscripts from Cephalonia," *BAR* 37 (2011), accessed October 3, 2015, <http://dbcfaa79b34c8f5dffffa-7d3a62c63519b1618047ef2108473a39.r81.cf2.rackcdn.com/wp-content/uploads/secret-mark-handwriting-response-pantuck-2.pdf>; Allan J. PANTUCK, "Solving the Mysterion of Morton Smith and the Secret Gospel of Mark," *BAR* 37 (2011), accessed October 3, 2015, <http://dbcfaa79b34c8f5dffffa-7d3a62c63519b1618047ef2108473a39.r81.cf2.rackcdn.com/wp-content/uploads/secret-mark-handwriting-response-pantuck.pdf>; JEFFERY, *Unveiled*, 1–14, 149–184 offers the most comprehensive biographical treatment of Smith to date including his career as a manuscript hunter, but it exhibits hostility towards its subject. For the latter point, consult Scott G. BROWN, "An essay review of Peter Jeffery, *The Secret Gospel of Mark Unveiled: Imagined Rituals of Sex, Death, and Madness in a Biblical*

no doubt that Smith was a [264] self-professed “manuscript hunter.”<sup>11</sup> Historically, the birth of this noble profession has been traced to the Italian Renaissance, when the good Humanists of Florence and other cities of Northern Italy began to piece together the

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*Forgery*,” *RBL* (2007), accessed October 3, 2015, [http://www.bookreviews.org/pdf/5627\\_5944.pdf](http://www.bookreviews.org/pdf/5627_5944.pdf). It should be noted that the publication of Smith’s letters in *Correspondence* (ed. STROUMSA) has invited divergent interpretations from scholars. Stroumsa argues in his introduction that the letters evidence how Smith “developed his own view on the nature of Jesus’ rituals” based on the Clementine letter (Guy G. STROUMSA, “Introduction,” in *Morton Smith and Gershom Scholem, Correspondence 1945–1982* (ed. Guy G. STROUMSA; Leiden: Brill, 2008), vii–xxiv, at xiv), and that they also show the “evolution” (STROUMSA, “Introduction,” xvi) and “gestation of his interpretation” (STROUMSA, “Introduction,” xvii). We agree that, in Stroumsa’s words, this “strongly points to the total trustworthiness of Smith’s account of his important discovery” (STROUMSA, “Introduction,” xxi). However, Pierluigi Piovanelli has contested Stroumsa with an alternative interpretation. Piovanelli believes that the correspondence shows how “Smith was exposed to Scholem’s ... theories about Jewish mysticism ... and started thinking about the historical Jesus as a truly Jewish messiah à la Sabbatai Tzevi”, and—in a precarious situation with his career—“realiz[ed] that, in order to make a stronger proposal about the historical Jesus as a miracle worker/magician, he was in need of more consistent proof.” In Piovanelli’s interpretation, thereafter, Smith would have manufactured an “extremely sophisticated forgery ... as a tool for promoting ideas that existed beforehand in his own head”; Pierluigi PIOVANELLI, “Halfway Between Sabbatai Tzevi and Aleister Crowley: Morton Smith’s ‘Own Concept of What Jesus “Must” Have Been’ and, Once Again, the Questions of Evidence and Motive,” in *Ancient Gospel or Modern Forgery? The Secret Gospel of Mark in Debate: Proceedings from the 2011 York University Christian Apocrypha Symposium* (ed. Tony Burke; Eugene, Ore.: Cascade Books, 2013): 157–183, at 180–181. One example of his interpretative choices, cited in both his French and English articles on the correspondence, is the emphasis put on Smith’s statement in his letter to Gershom Scholem, dated October 6, 1962 (#76), in which Smith claims to “have the evidence” for his views on the historical Jesus, as an indication of Smith having manufactured that very evidence; Pierluigi PIOVANELLI, “Une certaine ‘Keckheit, Kühnheit und Grandiosität’ . . . La correspondance entre Morton Smith et Gershom Scholem (1945–1982): Notes critiques,” *RHR* 228 (2011) 403–429, at 413; PIOVANELLI, “Halfway,” 171–172; citing *Correspondence*, 132–133. Yet the contents of *Clement’s Letter to Theodore* function as “evidence” just as well whether Smith manufactured or genuinely discovered the manuscript (as long as the potential spuriousness remains undiscovered), *i.e.* Smith’s certainty for having the “evidence” remains the same in both cases. The unavoidable ambiguousness for assessing motives lies at the heart of Piovanelli’s interpretation, or as he himself notes, “Une telle reconstruction, basée sur une lecture aussi honnête et “sans malice” que possible des lettres de Smith et Scholem, est, selon la formule consacrée, sinon vraie, du moins vraisemblable”; PIOVANELLI, “Keckheit,” 423. We will return to the topic of motive in the Epilogue.

<sup>11</sup> SMITH, *Secret Gospel*, 8.

“lost knowledge” of the classical world.<sup>12</sup> The prevalent [265] attitude of the times held that “the rightful” owners were “too ignorant to be worthy of” the documents they might have in their possession.<sup>13</sup> For centuries afterwards, it was the primary goal of the manuscript hunter to locate and secure ancient documents, and to bring them back to “safety” (*i.e.* into the realm of the “civilized” Western world). As contemporary travelogues demonstrate, this behaviour of the Western adventurers had changed the Eastern attitudes toward them by the mid-nineteenth century, and made the book guardians unwilling to part with their treasures “on any terms whatever.”<sup>14</sup>

Technological innovation in the form of the camera brought forth a new paradigm for manuscript hunting. Leo Deuel attributes the shift to Agnes Smith Lewis and Margaret Dunlop Gibson at the turn of the twentieth century, when the two Semitic scholars let go of the desire to possess the manuscripts they wanted to study. The new breed of manuscript hunters wanted first and foremost to make previously unknown manuscripts available to the scholarly community. The libraries and archives, whether monastic or secular, could continue to tend to their priceless documents. The visiting academics were content on making catalogues and photographing the manuscripts for the purpose of further study.<sup>15</sup>

At this point, it is time to consider where Smith fits into all of this. At the turn of the 1950s, he was profoundly interested in manuscripts related to Isidore of Pelusium.<sup>16</sup> Having done charity work for the Greek Orthodox Patriarchate of Jerusalem,<sup>17</sup> Smith received letters of introduction to present at the monasteries. Between 1951 and 1952, he visited a number of private, public, and monastic libraries and succeeded in his effort to locate “all the major Isidore-related manuscripts in western Europe.”<sup>18</sup> Armed

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<sup>12</sup> Jocelyn HUNT, *The Renaissance: Questions and Analysis in History* (London: Routledge, 1999), 17–19. For an alternative narrative of the Renaissance-induced manuscript hunting in relation to *Clement's Letter to Theodore*, see Charles W. HEDRICK, “Secret Mark: Moving on from Stalemate,” in *Ancient Gospel or Modern Forgery? The Secret Gospel of Mark in Debate: Proceedings from the 2011 York University Christian Apocrypha Symposium* (ed. Tony Burke; Eugene, Ore.: Cascade Books, 2013): 30–66, at 39–41.

<sup>13</sup> Leo DEUEL, *Testaments of Time: The Search for Lost Manuscripts and Records* (New York: Knopf, 1965), 21–22.

<sup>14</sup> Henry Octavius COXE, *Report to Her Majesty's Government on the Greek Manuscripts Yet Remaining in Libraries of the Levant* (London, 1858), 10–11.

<sup>15</sup> DEUEL, *Testaments of Time*, 303–316.

<sup>16</sup> Smith's letter to Gershom Scholem, dated March 31, 1951, in *Correspondence*, 54–57 (#28).

<sup>17</sup> Smith's letter to Gershom Scholem, dated December 4, 1950, in *Correspondence*, 47–50 (#26).

<sup>18</sup> PANTUCK, “Response to Agamemnon Tselikas,” 2–3.

with a camera<sup>19</sup> and publishing his notes on the manuscripts,<sup>20</sup> Smith firmly belongs to the new manuscript [266] hunter archetype. On occasion, he also generously shared his findings with other scholars and encouraged them to publish them in his stead.<sup>21</sup>

Of the thousands of manuscripts that Smith encountered, photographed, and catalogued, only *Clement's Letter to Theodore* has evoked demands that he make the item available to his colleagues, as if the possibility of forgery were somehow more pertinent to this particular manuscript, as if the monastic library Smith found it in were incapable of keeping it safe, and as if *sans* theft he would have had the opportunity to take it with him.

We suspect that two reasons are responsible for the extraordinary reaction to this particular document. First, this Clementine letter contained quotations from a μυστικὸν εὐαγγέλιον (Theod. II.6, 12), allegedly composed by Mark (Smith called this text the *Secret Gospel of Mark*), which made it of interest not only to scholars of Clement but also to the larger field of Christian origins. Second, the theories about Jesus that Smith based on these quotations struck a nerve with his colleagues.<sup>22</sup> Smith argued that this text revealed Jesus to be a magician who offered his disciples a mystery rite by which they were “possessed by Jesus’ spirit” and “participated by hallucination in Jesus’ ascent into the heavens.”<sup>23</sup> What is more, on one occasion in each of his books on *Clement's Letter to Theodore* Smith suggested that symbolism related to this union

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<sup>19</sup> Smith took some five thousand photographs of manuscripts in this one trip alone; STROUMSA, “Comments,” 150. See also Smith’s letter to Gershom Scholem, dated January 26, 1953, in *Correspondence*, 62–63 (#31).

<sup>20</sup> Morton SMITH, “Σύμμεικτα: Notes on Collections of Manuscripts in Greece,” *Επετηρίς Εταιρείας Βυζαντιῶν Σπουδῶν* 26 (1956): 380–393. In Smith’s letter to Gershom Scholem, dated August 1, 1955, in *Correspondence*, 79–82 (#40), at 80, Smith notes that publishing catalogues of manuscripts is “a worthy cause” in itself.

<sup>21</sup> PANTUCK and BROWN, “Madiotes,” 110. In a letter to H. Dörries, dated April 25, 1959, Smith wrote a detailed description of the location of a manuscript of Macarius he had encountered, enclosed photographs he had taken, and instructed the well-known Macarius scholar on how to gain access to the original. We wish to thank Pantuck for bringing this letter to our attention.

<sup>22</sup> Cf. STROUMSA, “Introduction,” xiv: “The discovery itself seems to have deeply offended the religious sensibilities of many scholars, who could not conceive of such a picture of the Lord emerging from a credible ancient text.”

<sup>23</sup> SMITH, *Secret Gospel*, 113–114; Morton SMITH, *Clement of Alexandria and a Secret Gospel of Mark* (Cambridge: Harvard University Press, 1987), 251; Morton SMITH, “Clement of Alexandria and Secret Mark: The Score at the End of the First Decade,” *HTR* 75 (1982): 449–461, at 455.

could have gone as far as “physical union” between Jesus and the disciples.<sup>24</sup> These are extraordinary ideas, and the reactions they provoked among scholars ranged from incredulity to indignation.

[267] Smith narrated his discovery of *Clement's Letter to Theodore* in his book *The Secret Gospel*.<sup>25</sup> As part of an extended trip to the East encompassing libraries in Jordan, Israel, Turkey, and Greece,<sup>26</sup> Smith entered the ancient monastery of Mar Saba in the summer of 1958 with permission from His Beatitude Benedict of Jerusalem. Under the supervision of a monk, Smith had access to the Mar Saba tower library, where he combed through the printed volumes for manuscripts that had been left behind when the majority of them had been transferred to Jerusalem in the latter half of the 1800s.<sup>27</sup> At the end of his stay, he found a copy of a letter written on three of the end pages in a copy of Isaac Vossius's 1646 edition of Ignatius's letters, *Epistulae genuinae S. Ignatii Martyris*.<sup>28</sup> From the title of the manuscript alone (“From the letters of the most holy

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<sup>24</sup> “Freedom from the law may have resulted in completion of the spiritual union by physical union”; SMITH, *Secret Gospel*, 114; “a baptism administered by Jesus to chosen disciples, singly, and by night. In this baptism the disciple was united with Jesus. The union may have been physical (... there is no telling how far symbolism went in Jesus' rite), but the essential thing was that the disciple was possessed by Jesus' spirit”; SMITH, *Clement*, 251. See also Shawn EYER, “The Strange Case of the Secret Gospel According to Mark: How Morton Smith's Discovery of a Lost Letter of Clement of Alexandria Scandalized Biblical Scholarship,” *Alexandria* 3 (1995): 103–129 for the reception of Smith's writings.

<sup>25</sup> SMITH, *Secret Gospel*, 1–25.

<sup>26</sup> PANTUCK and BROWN, “Madiotes,” 107 n. 1.

<sup>27</sup> SMITH, *Clement*, ix. Agamemnon Tselikas offers three dates for major transfers of manuscripts: 1857, 1864, and 1887; Agamemnon TSELIKAS, “Agamemnon Tselikas' Handwriting Analysis Report,” *BAR* 37 (2011): I–XV, at VI, accessed October 3, 2015, <http://www.biblicalarchaeology.org/daily/biblical-topics/bible-interpretation/agamemnon-tselikas-handwriting-analysis-report>. An “Appendix: Summary Report of Agamemnon Tselikas” was published in Hershel SHANKS, “Was Morton Smith the Bernie Madoff of the Academy?” in *Ancient Gospel or Modern Forgery? The Secret Gospel of Mark in Debate: Proceedings from the 2011 York University Christian Apocrypha Symposium* (ed. Tony Burke; Eugene, Ore.: Cascade Books, 2013): 135–144, at 142–144. The same summary is also published under the heading “Agamemnon Tselikas' Summary” on the above *BAR* website.

<sup>28</sup> Recently, Piovanelli has brought up the unique nature of the discovery of *Clement's Letter to Theodore*, which he describes as “the only case in the history ... in which an important text by a major [ancient] author would have been found copied at the end of a European book”; PIOVANELLI, “Halfway,” 160–161. Though we believe him to be technically correct, however, by adding qualifications such as “major” and “European” he has excluded many possible parallels. First, Smith himself noted that “many of the printed books [in the monastic library of Mar Saba] contained extensive handwritten passages”, and

Clement, the author of the Stromateis. To The- [268] odore”) he could imagine the implications for Clementine scholarship. He made three sets of photographs of the letter and left it in the Mar Saba library.<sup>29</sup>

Was Smith’s faith in the ability of an Eastern library to safeguard a book warranted or not? After all—as Smith’s critics pointed out—the Mar Saba library let out books and manuscripts to “members of the order” without keeping records of their coming and going, had witnessed at least one major fire a century earlier, and had employed loose manuscript pages as binding material.<sup>30</sup> In the case of the Clementine letter, however, the trust was well-placed—but only so far, as we are about to see. Eighteen years after Smith’s 1958 visit, Guy G. Stroumsa went to the monastery accompanied by three other scholars from the Hebrew University of Jerusalem.<sup>31</sup> They were able to locate Vossius’s book with its handwritten letter still intact, and bring it to the Patriarchal Library in Jerusalem. An analysis of the ink used to write the letter never happened though, since at the time only the Israeli police could perform such an analysis, and Archimandrite

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that, evidently, paper “had been in short supply at Mar Saba during the seventeenth, eighteenth and nineteenth centuries”; SMITH, *Secret Gospel*, 11. Second, as Hedrick has observed, “it is not unusual for the works of early authors to appear in manuscripts of a very late date and in a considerably different script from the original author’s time period,” citing for examples the works of Thucydides’s *History of the Peloponnesian War* (all the major manuscripts are from the tenth to fifteenth centuries), Polybius’s *Histories* (only extant manuscript from the eleventh century), the *Infancy Gospel of Thomas* (all the principal manuscripts are from the fourteenth and fifteenth centuries), and the *Gospel of Peter* (only extant manuscript from between the sixth and ninth centuries); HEDRICK, “Secret Mark,” 39–42.

<sup>29</sup> Pantuck confirms that “Smith took three different sets of photographs of MS65 at Mar Saba, and only one and a half of these sets have been published,” and that they now reside at the *Jewish Theological Seminary*; personal communication.

<sup>30</sup> QUESNELL, “Clementine,” 49–50. See also, NEUSNER, *Tannaitic Parallels*, 27–31. Recently, the lack of library control has been raised in PIOVANELLI, “Keckheit,” 423, and PIOVANELLI, “Halfway,” 161. Smith, however, gave no indication that anyone besides the monks inside the monastery could borrow books from the library; Morton SMITH, “Monasteries and Their Manuscripts,” *Archaeology* 13 (1960): 172–177. See also Allan J. PANTUCK, “A Question of Ability: What Did He Know and When Did He Know It? Further Excavations from the Morton Smith Archives,” in *Ancient Gospel or Modern Forgery? The Secret Gospel of Mark in Debate: Proceedings from the 2011 York University Christian Apocrypha Symposium* (ed. Tony Burke; Eugene, Ore.: Cascade Books, 2013): 184–211, at 206.

<sup>31</sup> The other three were David Flusser and Shlomo Pines, both professors at the Hebrew University of Jerusalem, and Archimandrite Meliton from the Greek Orthodox Patriarchate of Jerusalem, at the time a research student at the Hebrew University.



Meliton from the Patriarchate would not hand the book over to them.<sup>32</sup> In hindsight, the transfer of the Clementine letter would have been better left undone. Thomas Talley's attempts to study the letter in 1980 were "frustrated," for the librarian Kallistos Dourvas told him that the two folios of the manuscript had been removed from the book and were being repaired.<sup>33</sup> A few years later, Per Beskow obtained permission from the Patriarch [269] to see the manuscript. When he came to the library in November 1984, however, he was denied access on the grounds that "the manuscript had been sprayed with insecticides" – a reason given six months earlier to his colleague Anders Hultgård as well (though Hultgård was trying to consult a different manuscript).<sup>34</sup> Somewhat earlier, in June 1983, Quentin Quesnell had gained access to the letter. At that time, the manuscript was covered by removable plastic, and Quesnell had the opportunity to study it on more than one occasion for about two hours each time. Dourvas, the librarian, took the leaves to *Photo Garo Studio* in Jerusalem and had them photographed.<sup>35</sup> Since the early 1980s, there have been no other sightings of the manuscript, despite numerous attempts to locate it.<sup>36</sup> The most recent search by Agamemnon Tselikas turned up Vossius's book, but failed to locate the pages with Clement's letter.<sup>37</sup> Rumours in the academy of the reasons for the manuscript's disappearance and of its hiding place are too numerous to enumerate here.

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<sup>32</sup> STROUMSA, "Comments," 147–148. For a more detailed narrative, consult STROUMSA, "Introduction," xx–xxi.

<sup>33</sup> Thomas J. TALLEY, "Liturgical Time in the Ancient Church: The State of Research," *Studia Liturgica* 14 (1982): 34–51, at 41.

<sup>34</sup> Personal communication; see also Per BESKOW, *Fynd och fusk: Falsarier och mystifikationer omkring Jesus* (Örebro: Libris, 2005), 147–148; Per BESKOW, "Modern Mystifications of Jesus," in *The Blackwell Companion to Jesus* (ed. Delbert BURKETT; Malden, Mass.: Wiley-Blackwell, 2011): 458–473, at 460.

<sup>35</sup> Personal communication. After Quesnell's death in 2012, materials related to his study of the letter, including the photos, shall be made available through the Smith College Archives.

<sup>36</sup> Dourvas' assurance that the manuscript was in the library until his resignation in 1990 seems uncertain, for it would require that he had actually checked its state shortly before he left; cf. Charles W. HEDRICK and Nikolaos OLYMPIOU, "Secret Mark: New Photographs, New Witnesses," *The Fourth R* 13 (2000): 3–16, at 8–9; BROWN, *Mark's Other Gospel*, 25.

<sup>37</sup> TSELIKAS, "Handwriting Analysis Report", III.

## Manuscript Access Control

Up to this point, we have strived to place the manuscript hunting activities of Smith in their proper context, summarized the discovery story of *Clement's Letter to Theodore* and described its transfer to Jerusalem and its subsequent disappearance. We consider these details important for two reasons. First, scholars have suggested that only the examination of the physical document can dissolve doubts about its authenticity.<sup>38</sup> Second, controlling access to documents called into question plays a prominent feature of many cases of forgery, with the [270] perpetrator doing his best to hinder the attempts of others to study the item at close range.<sup>39</sup> Therefore, an important question is, did Smith really try to control access to the manuscript of *Clement's Letter to Theodore*? And if he did not, what are the implications?

In light of the above discussion, we suggest he did not. Vossius's book was left intact in the monastery of Mar Saba, but not because Smith wanted to hide it away from closer study.<sup>40</sup> Rather, it was because photographing manuscripts and then leaving them undisturbed was the defining characteristic that differentiated the new manuscript hunters from their older, less scrupulous forbears.<sup>41</sup> Though Smith devoted much space to the question of the text's authenticity, he was preoccupied mainly with whether the letter was stylistically Clementine or not, a question for which

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<sup>38</sup> QUESNELL, "Clementine," 49–50; Bart D. EHRMAN, "Response to Charles Hedrick's Stalemate," *J ECS* 11 (2003): 155–163, at 159–160; David HENIGE, "Authorship Renounced: The "Found" Source in the Historical Record," *Journal of Scholarly Publishing* 41 (2009): 31–55, at 52 n. 44; David LANDRY, "Noncanonical Texts: *The Da Vinci Code* and Beyond," *WW* 29 (2009): 367–379, at 374.

<sup>39</sup> Regarding *Clement's Letter to Theodore* this point has resurfaced recently by Henige: "[Smith] seemed remarkably indifferent ... to the fact that his source was no longer available—all this as though personal reputation were a legitimate substitute for free and open access"; HENIGE, "Authorship Renounced," 41.

<sup>40</sup> Part of the academic folklore on Smith documented in BROWN, *Mark's Other Gospel*, 25–26.

<sup>41</sup> Cf. STROUMSA, "Introduction," xx: "he did with [the manuscript] exactly what a scholar working in a library should do: photograph the text, publish a list of the documents analyzed, and put the book back on the shelf afterwards." Cf. also Tony BURKE, "Introduction," in *Ancient Gospel or Modern Forgery? The Secret Gospel of Mark in Debate: Proceedings from the 2011 York University Christian Apocrypha Symposium* (ed. Tony BURKE; Eugene, Ore.: Cascade Books, 2013): 1–29, at 27: "Smith appears to have done what is expected of anyone in his position: he found an interesting manuscript, photographed it, cataloged it (adding his own reference number to the front page), left it where he found it, and returned home to publish his findings."

the physical, apparently eighteenth-century manuscript could contribute no answers.<sup>42</sup> The testimony of Stroumsa proves that Smith did put the book back in its original place,<sup>43</sup> while the experiences of Talley, Beskow and Quesnell demonstrate that there was a period of time when the manuscript was available to some scholars at least. The rumour that Smith had a hand in the final disappearance of the document is unfounded, and the available facts offer nothing to encourage it. On the contrary, Smith himself kept the academic world informed on the whereabouts of the manuscript and reported on its transfer to Jerusalem,<sup>44</sup> which strongly suggests that he did not fear the results of a forensic investigation.<sup>45</sup>

### Four Handwriting Analyses

Until today, a total of four handwriting analyses have been performed on the photographs or other reproduction images of the manuscript of *Clement's Letter to Theodore*. We will focus on three of these four handwriting analyses performed

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<sup>42</sup> Professor Stanley Isser reports in Pantuck, "Ability," 210, that Smith spent "most of his time between his discovery and publication" trying to authenticate the text by comparing "every word and phrase in both the letter and the gospel text ... with the manner and frequency of such words and phrases that were used in Clementine literature and in canonical Mark in order to see if they fit the style in those texts." Based on stylistic considerations, Smith concluded that "the letter is either entirely genuine or a deliberate imitation of Clement's style"; SMITH, *Clement*, 76.

<sup>43</sup> STROUMSA, "Introduction," xx-xxi: "The book had clearly remained where Smith had found it, and where he had replaced it after having made his photographs."

<sup>44</sup> SMITH, "Score," 458-459.

<sup>45</sup> If anyone is to blame for the loss of the manuscript, the guilty party resides at the Greek Orthodox Patriarchal library. Apart from the fact that the manuscript went missing while under their supervision, they have refused to allow tests to be performed on at least two occasions (by Stroumsa's company and by Quesnell), and also obstructed scholars in their attempts to even see the manuscript. Put in this way, the oft-repeated accusation of Smith making "no effort to subject the book to scientific analysis" (Craig A. EVANS, "Morton Smith and the Secret Gospel of Mark: Exploring the Grounds for Doubt," in *Ancient Gospel or Modern Forgery? The Secret Gospel of Mark in Debate: Proceedings from the 2011 York University Christian Apocrypha Symposium* (ed. Tony BURKE; Eugene, Ore.: Cascade Books, 2013): 75-100, at 97) becomes harder to maintain as we do not know if Smith, in fact, tried to gain access and have the letter tested at some point, but was simply refused. Cf. Scott G. BROWN and Allan. J. PANTUCK, "Craig Evans and the Secret Gospel of Mark: Exploring the Grounds for Doubt," in *Ancient Gospel or Modern Forgery? The Secret Gospel of Mark in Debate: Proceedings from the 2011 York University Christian Apocrypha Symposium* (ed. Tony Burke; Eugene, Ore.: Cascade Books, 2013): 101-134, at 131-132.

between 2005 and 2011 by Scott G. Brown, Venetia Anastasopoulou, and Agamemnon Tselikas, noting that we have already dealt with Stephen C. Carlson's analysis in a previous article.<sup>46</sup>

The fact that the physical manuscript was not readily available when it remained at Mar Saba, then later disappeared when scholars started asking to see it at the Patriarchal Library may account for the scant attention that scholars gave to the handwriting. It was generally accepted—based on Smith's report that he had consulted other specialists<sup>47</sup>—that the script agreed with other eighteenth-century handwriting. This assumption, however, was to change with Carlson's book *The Gospel Hoax: Morton Smith's Invention of Secret Mark* (2005), which tried to demonstrate that the letter was written by Smith himself. His interpretation of the appearance of the letter's handwriting persuaded many scholars that the manuscript was forged, and that Smith was the [272] culprit.<sup>48</sup> Consequently, the inevitable criticism began with handwriting, and Carlson's 2005 examination was followed by three subsequent analyses.

The first of these analyses was done by Brown, who debunked Carlson's case point-by-point in a series of articles shortly after the publication of *The Gospel Hoax*.<sup>49</sup> Brown drew attention to various deficiencies in Carlson's analysis, including his lack of requisite training and experience, his inattention to concepts crucial for forensic document examination (natural variation, master pattern, known standards), his improper application of a concept (forger's lapse) and a method (handwriting

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<sup>46</sup> Roger VIKLUND and Timo S. PAANANEN, "Distortion of the Scribal Hand in the Images of *Clement's Letter to Theodore*," *VC* 67 (2013): 235–247.

<sup>47</sup> SMITH, *Clement*, 1.

<sup>48</sup> Paul FOSTER, "Books of the Month: Secret Mark is no Secret Anymore—Secret Mark: Uncovering a Hoax," *ExpTim* 117 (2005): 64–68; Michael J. KRUGER, "Book Reviews: *The Gospel Hoax: Morton Smith's Invention of Secret Mark*. By Stephen C. Carlson," *JETS* 49 (2006): 422–424; Christopher TUCKETT, "Review: *The Gospel Hoax: Morton Smith's Invention of Secret Mark*. By Stephen C. Carlson," *JTS* 58 (2007): 193–195. For more in-depth treatment of Carlson's reception, consult Timo S. PAANANEN, "From Stalemate to Deadlock: *Clement's Letter to Theodore* in Recent Scholarship," *CiBR* 11 (2012): 87–125, at 95–96.

<sup>49</sup> BROWN, "Factualizing the Folklore"; Scott G. BROWN, "The Question of Motive in the Case against Morton Smith," *JBL* 125 (2006): 351–383; Scott G. BROWN, "Reply to Stephen Carlson," *ExpTim* 117 (2006): 144–149; Scott G. BROWN, "The Letter to Theodore: Stephen Carlson's Case against Clement's Authorship," *J ECS* 16 (2008): 535–572; PANTUCK and BROWN, "Madiotes"; Scott G. BROWN and Allan. J. PANTUCK, "Stephen Carlson's Questionable Questioned Document Examination," accessed October 3, 2015, <http://rogerviklund.files.wordpress.com/2014/03/brown-pantuck-2010.pdf>.

identification), his inattention to the signs of authenticity that document examiners look for and his misleading presentation of a report written on his behalf by the English-speaking forensic document examiner Julie C. Edison. Brown's constructive contribution dealt with the comparison between Smith's handwritten Greek and the handwriting of Clement's letter. Brown aimed to establish that general differences exist in the way the two hands produce the three letterforms that Carlson used to connect the manuscript to Smith.<sup>50</sup>

[273] In late 2009, *Biblical Archaeology Review* contacted two Greek handwriting specialists who would independently study *Clement's Letter to Theodore* and compare its script with a variety of samples of Smith's English and Greek handwriting from 1951 to 1984, including his complete handwritten transcription of the letter.<sup>51</sup> Anastasopoulou, a forensic document examiner, saw three very different scripts before her. In her opinion, Clement's letter had excellent rhythm and was written with "freedom, spontaneity and artistic flair."<sup>52</sup> Likewise, Smith's English hand is "spontaneous and unconstrained, with a very good rhythm."<sup>53</sup> Smith's Greek handwriting was an entirely different matter. "It is obvious," Anastasopoulou observed, "that [Smith's] hand is not familiarised in Greek writing" and lacks the spontaneous ease of his English script.<sup>54</sup> She concluded that a person with a Greek hand like Smith's could hardly have produced the complex writing seen in *Clement's Letter to Theodore*.<sup>55</sup> When asked to elaborate further, Anastasopoulou stated that the

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<sup>50</sup> Brown also assessed Carlson's claim that a different photograph of a different Mar Saba manuscript catalogued by Smith (no. 22, reproduced in halftone in *Secret Gospel*, 37) contains not only the same eighteenth-century hand as *Clement's Letter to Theodore* but also displays the same signs of forgery. Working with Allan J. Pantuck, they argue that this other handwriting is a different hand altogether, and that the supposed clues about a bald swindler that Carlson offered to connect this hand to Smith were based entirely on errors; PANTUCK and BROWN, "Madiotes".

<sup>51</sup> Venetia ANASTASOPOULOU, "Experts Report Handwriting Examination," *BAR* 36 (2010): 1–39, at 6–7, accessed October 3, 2015, <http://www.biblicalarchaeology.org/wp-content/uploads/secret-mark-analysis.pdf>.

<sup>52</sup> *Ibid.*, 13.

<sup>53</sup> *Ibid.*, 14.

<sup>54</sup> *Ibid.*, 18.

<sup>55</sup> *Ibid.*, 38. As a general principle in handwriting identification, "no one can successfully imitate a writing more skillful than his or her own"; Heidi H. HARRALSON, *Developments in Handwriting and Signature Identification in the Digital Age* (Oxford, Waltham, Mass.: Anderson, 2013), 7, 21–22. See also

handwriting in Clement's letter was consistent throughout and lacked the usual signs of inauthenticity such as poor line quality or poor continuity in motion of the hand.<sup>56</sup>

Tselikas, a palaeographer, drew an opposite conclusion. He noted seventeen instances in which the author had made errors that are inconceivable for a native Greek writer and eleven additional instances in which the copyist had blundered, some of them suspicious. In nineteen places he spotted letters with "completely foreign or strange and irregular forms" and, contrary to Anastasopoulou's observation, claimed that the non-continuous lines of the letters were evidence of non-spontaneous movement of the hand.<sup>57</sup> Tselikas conjectured that Smith was the culprit and that he concocted his imitative script using certain manuscripts from the Thematon monastery of Cephalonia as a model (one of which even contained recipes for producing genuine eighteenth-century [274] ink),<sup>58</sup> though the handwriting of the Clementine letter still contained some similarities to Smith's Greek writing that point to Smith as the author.<sup>59</sup>

What are we to make of these four studies, two of which proclaim the text a forgery while the other two hold it to be authentic? Recent scholarly responses have begun to treat the handwriting analysis as a lost cause, no doubt due to the divergent expert opinions. Francis Watson, in his efforts to move the discussion "beyond reasonable doubt" (and establish Smith as the forger), disregards handwriting analysis in favour of judgements based on internal evidence.<sup>60</sup> Craig A. Evans treats the respective verdicts of Carlson, Anastasopoulou and Tselikas as commensurate expert opinions, and concludes that "handwriting analysis does not appear to be conclusive."<sup>61</sup> Peter

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Roy A. HUBER and Alfred M. HEADRICK, *Handwriting Identification: Facts and Fundamentals* (Boca Raton: CRC Press, 1999), 295–296.

<sup>56</sup> Venetia ANASTASOPOULOU, "Can a Document in Itself Reveal a Forgery?" *BAR* 36 (2010), accessed October 3, 2015, <http://www.biblicalarchaeology.org/daily/biblical-topics/bible-interpretation/venetia-anastasopoulou-can-a-document-in-itself-reveal-a-forgery>.

<sup>57</sup> TSELIKAS, "Handwriting Analysis Report," IV.

<sup>58</sup> However, as Pantuck has convincingly argued, Smith could not have used any of these manuscripts as a model to imitate the script or to produce the ink, as he did not photograph any of them. PANTUCK, "Response to Agamemnon Tselikas," 3–4.

<sup>59</sup> *Ibid.*, V, VI, IX, X (note that the last two are actually identical).

<sup>60</sup> WATSON, "Beyond Suspicion," 131.

<sup>61</sup> EVANS, "Grounds for Doubt," 91–93. Evans includes the English-speaking forensic document examiner Julie C. Edison in the equation, presenting her as a "professional handwriting expert" who assisted Carlson in his analysis. He can thereby count the number of experts as three to one in favour of

Jeffery, though he acknowledges that Anastasopoulou's study "does raise the bar for those who argue that Smith penned the Mar Saba document in his own hand," nevertheless faults her analysis in a manner that betrays the guild's general lack of familiarity with the established methodologies of handwriting studies, as we will discuss below.<sup>62</sup>

The reasons for the apparent confusion of biblical scholars are twofold. First, it is not generally recognized that forensic document analysis and palaeography are different areas of expertise. Second, Carlson's much-praised study does not represent either of these competencies. One reason is his choice of methods, another his application of them. [275] To put it bluntly: Carlson's handwriting analysis suffers from a fundamental flaw, because the images he examined, the printed reproductions of the letter found in Smith's *Clement of Alexandria and a Secret Gospel of Mark*, were distorted due to the line screen of the halftone reproduction process and wholly unsuitable for questioned document analysis. Since Carlson's handwriting analysis was performed on a material which itself generates false positive signs of forgery through an optical illusion, it cannot be considered valid. Once the original photographs are substituted for the printed images Carlson utilized for his study, the signs of forgery he claimed were present are no longer apparent.<sup>63</sup>

This failure of Carlson's methodology, combined with his lack of training, accounts for the contrasting verdicts of Carlson and Anastasopoulou, the latter being a professionally trained forensic document examiner who had access to the same high-quality images that we have studied. Whereas Carlson determined that the handwriting "was executed more slowly than it purports to be" by a writer who "had

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forgery, counting "Carlson and two handwriting experts" as believing that Smith forged the manuscript against the lone Anastasopoulou. But Edison—though she is a professional handwriting expert—has never stated that she thinks the document is a forgery or suspects Smith of forging it. On the contrary, she has specifically emphasized that "no professional evaluation of mine [concerning the status of *Clement's Letter to Theodore*] was put into writing"; BROWN and PANTUCK, "Questionable," 3.

<sup>62</sup> Peter JEFFERY, "Response to Handwriting Analysis," *BAR* 36 (2010), accessed October 3, 2015, <http://www.biblicalarchaeology.org/daily/biblical-topics/bible-interpretation/peter-jeffery-response-to-handwriting-analysis/>; Peter JEFFERY, "Additional Response to Handwriting Analysis," *BAR* 36 (2010), accessed October 3, 2015, <http://www.biblicalarchaeology.org/daily/biblical-topics/bible-interpretation/peter-jeffery-additional-response-to-handwriting-analysis/>.

<sup>63</sup> For details, consult R. VIKLUND – T. S. PAANANEN, "Distortion" (cited n. 46).

not fully mastered the style of handwriting,"<sup>64</sup> Anastasopoulou came to the opposite conclusion. She found the text to be "written spontaneously with an excellent rhythm" by someone who was "used to writing in this manner."<sup>65</sup>

The question of handwriting, however, cannot be decided before considering the respective verdicts of the two experts hired by *Biblical Archaeology Review*—Anastasopoulou and Tselikas—who reached quite different conclusions. As previously noted, Anastasopoulou's report has received two kinds of critical responses. Scholars have called into question its details<sup>66</sup> and even the validity of the whole field of handwriting comparison as it is currently practised in forensic document examination.<sup>67</sup> Starting with the latter objection, a brief history of the questioned field will enable us to answer the question implied: when we compare two handwriting samples to find out if a given individual is responsible for both of them or when we try to determine if a writing is natural or imitated, are we dealing with fringe science or with a serious scholarly enterprise?

## Theory of handwriting

Forensic document examination (FDE)—in the United States commonly known as questioned document examination (QDE)—had first established itself following the pioneering work of Albert S. Osborn [276] (1858–1947), whose *Questioned Documents* (1910) is the classic text in the field.<sup>68</sup> Serious doubts, however, regarding its evidential basis were raised by D. Michael Risinger, Mark Denbeaux and Michael J. Saks in a 1989 article, where they concluded that "no available evidence demonstrates the existence of handwriting identification expertise."<sup>69</sup>

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<sup>64</sup> CARLSON, *Gospel Hoax*, 35.

<sup>65</sup> ANASTASOPOULOU, "Handwriting Examination," 9.

<sup>66</sup> JEFFERY, "Response to Handwriting Analysis"; JEFFERY, "Additional Response".

<sup>67</sup> EVANS, "Grounds for Doubt," 91–94.

<sup>68</sup> Albert S. OSBORN, *Questioned Documents: A Study of Questioned Documents with an Outline of Methods by Which the Facts May Be Discovered and Shown* (Rochester, N.Y., 1910); our citations come from the revised edition; Albert S. OSBORN, *Questioned Documents* (2<sup>nd</sup> edn; Toronto: Carswell, 1929). In the UK the early key text is Wilson R. HARRISON, *Suspect Documents: Their Scientific Examination* (London: Sweet & Maxwell, 1958).

<sup>69</sup> D. Michael RISINGER, Mark P. DENBEAUX and Michael J. SAKS, "Exorcism of Ignorance as a Proxy for Rational Knowledge: The Lessons of Handwriting Identification "Expertise"," *University of*



A recent report from the National Academy of Sciences (NAS) made another hard hit on many of the branches of forensic sciences, for some of these were “supported by little rigorous systematic research to validate the discipline’s basic premises and techniques.”<sup>70</sup> Handwriting analysis, however, fared reasonably well, with the report suggesting only that its “scientific basis” should “be strengthened.”<sup>71</sup> The field of forensic document examination had, in fact, been in the middle of such process since Risinger, Denbeaux and Saks had mounted their attack and following the occasional U.S. Courts’ decisions to place restrictions on the use of handwriting evidence on the basis of missing scientific validation.<sup>72</sup>

[277] Though the body of empirical studies on handwriting comparison may not yet be large enough to warrant its full-scale acceptance into the canons of the academy, the existing ones are promising. A number of recent papers have established that an expert analyst performs significantly better than a non-expert in distinguishing between authentic and inauthentic handwriting.<sup>73</sup> As for the percentage of erroneous

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*Pennsylvania Law Review* 137 (1989): 731–792, at 750–751. This was followed up by D. Michael RISINGER and Michael J. SAKS, “Science and Nonsense in the Courts: Daubert Meets Handwriting Identification Expertise,” *Iowa Law Review* 82 (1996): 21–74. For other notable critiques from recent years, consult Jane Campbell MORIARTY and Michael J. SAKS, “Forensic Science: Grand Goals, Tragic Flaws, and Judicial Gatekeeping,” *Judges Journal* 44 (2005): 16–33; D. Michael RISINGER, “Goodbye to All That, or, a Fool’s Errand, by One of the Fools: How I Stopped Worrying About Court Responses to Handwriting Identification (And “Forensic Science” in General) and Learned to Love Misinterpretations of *Kumho Tire v. Carmichael*,” *Tulsa Law Review* 43 (2007): 447–476.

<sup>70</sup> Committee on Identifying the Needs of the Forensic Sciences Community, National Research Council, *Strengthening Forensic Science in the United States: A Path Forward* (Washington, D.C.: National Academies Press, 2009), 22. For the immediate reactions and prospects for the future of forensic sciences, consult Jennifer L. MNOOKIN et al., “The Need for a Research Culture in the Forensic Sciences,” *UCLA Law Review* 58 (2011): 725–779.

<sup>71</sup> National Research Council, *Strengthening Forensic Science*, 166; the report goes on to state that “recent studies ... suggest that there may be a scientific basis for handwriting comparison, at least in the absence of intentional obfuscation or forgery”; National Research Council, *Strengthening Forensic Science*, 166–167.

<sup>72</sup> Jane Campbell MORIARTY, “Will History Be Servitude? The NAS Report on Forensic Science and the Role of the Judiciary,” *Utah Law Review* (2010): 299–326; Paul C. GIANNELLI, “Daubert and Forensic Science: The Pitfalls of Law Enforcement Control of Scientific Research,” *University of Illinois Law Review* 53 (2011): 53–90, at 60–61.

<sup>73</sup> Moshe KAM, Joseph WETSTEIN and Robert CONN, “Proficiency of Professional Document Examiners in Writer Identification,” *Journal of Forensic Sciences* 39 (1994): 5–14; Bryan FOUND, Jodi SITA

attributions of authorship, one study found the expert analysts erring in only 6.5 percent of the cases, while non-experts “are grossly over matching” and are almost “6 times more likely ... to [falsely] match two documents that were created by different writers.”<sup>74</sup> One reason for the higher reliability of expert opinions lies in their conservative approach to conclusions. Experts will not hazard a guess, but will instead content themselves with an inconclusive conclusion when faced with ambiguous evidence. Consequently, when experts do offer an opinion, they are more than likely to be correct.<sup>75</sup> Other recent studies have supported the scientific validity of many of the key concepts of handwriting analysis.<sup>76</sup>

[278] While Evans’s general suspicion that “handwriting analysis does not appear to be conclusive”<sup>77</sup> is hard to maintain in light of the research cited above, other scholars have expressed doubts on the more specific aspects of forensic document examination in Anastasopoulou’s study. Jeffery, for example, has objected that Anastasopoulou’s comparison between the handwriting in *Clement’s Letter to Theodore*

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and Douglas ROGERS, “The Development of a Program for Characterising Forensic Handwriting Examiners’ Expertise: Signature Examination Pilot Study,” *Journal of Forensic Document Examination* 12 (1999) 69–80; Moshe KAM et al., “Signature Authentication by Forensic Document Examiners,” *Journal of Forensic Sciences* 46 (2001) 884–888; Jodi SITA, Bryan FOUND and Douglas ROGERS, “Forensic Handwriting Examiners’ Expertise for Signature Comparison,” *Journal of Forensic Sciences* 47 (2002): 1117–1124; Adrian G. DYER, Bryan FOUND and Douglas ROGERS, “Visual Attention and Expertise for Forensic Signature Analysis,” *Journal of Forensic Sciences* 51 (2006): 1397–1404; Adrian G. DYER, Bryan FOUND and Douglas ROGERS, “An Insight into Forensic Document Examiner Expertise for Discriminating Between Forged and Disguised Signatures,” *Journal of Forensic Sciences* 53 (2008): 1154–1159.

<sup>74</sup> Moshe KAM, Gabriel FIELDING and Robert CONN, “Writer Identification by Professional Document Examiners,” *Journal of Forensic Sciences* 42 (1997): 778–786. In SITA, FOUND and ROGERS, “Forensic Handwriting Examiners’ Expertise” the margin of error was found to be even less. Other studies have addressed the more difficult question of distinguishing between simulated and disguised handwriting, but arrived at similar conclusions on the expertise of experts; Bryan FOUND and Douglas ROGERS, “The Probative Character of Forensic Handwriting Examiners’ Identification and Elimination Opinions on Questioned Signatures,” *Forensic Science International* 178 (2008): 54–60.

<sup>75</sup> Carolyne BIRD, Bryan FOUND and Douglas ROGERS, “Forensic Document Examiners’ Skill in Distinguishing Between Natural and Disguised Handwriting Behaviors,” *Journal of Forensic Sciences* 55 (2010): 1291–1295. In this study, an expert statement (when given) was found to be correct in over 95 % of the cases.

<sup>76</sup> E.g. Sargur N. SRIHARI et al., “Individuality of Handwriting,” *Journal of Forensic Sciences* 47 (2002): 856–872.

<sup>77</sup> EVANS, “Grounds for Doubt,” 93.

and Smith's Greek handwriting is "largely an apples-to-oranges comparison," for they are examples of different scripts, the first being cursive while the latter predominantly consists of block letters.<sup>78</sup> On the other hand, Tselikas questioned the entire field of handwriting comparison (and therefore also Anastasopoulou's study) on the basis that "the scribe of the letter would not use the [*sic*] own personal style," which in his opinion would significantly hinder attempts to identify the two handwritings as originating from Smith.<sup>79</sup> However, while these objections seem natural and intuitive, they betray unfamiliarity with the theory behind handwriting identification. As we discuss below, handwriting comparison rests on a fairly non-intuitive premise, though the growing body of empirical research cited above suggests that the premise does have merit.

Tom Davis formulates the methodological basis of handwriting comparison in the following way: "a given writer will tend to produce writing that is idiographic ... a given piece of writing can have characteristics that are ascertainable by expert analysis, constant between different writings by the same individual, and unique to that individual.<sup>80</sup> Idiographic refers to an individual's writing characteristics, idiosyncratic details that differentiate one particular writer from another even when utilizing a different script. Though we learn our handwriting from exemplars, every one of us will adopt distinctive quirks of our own. Through continuous practice of our handwriting, we develop an internalized model hand, *i.e.* the ideal execution of our unique handwriting. The programming language of our internalized model hand is embedded into our kinaesthetic memory as units of movement that constitute the basic building blocks of letter formation, *i.e.* the internalized model hand can be thought of as a modular repository of distinctive twists and turns of the hand, which remain constant in their output of strokes. The end result, the actual lines on the page that form the letters, words and sentences, however, comes about from the interplay between our internalized model hand and the particulars of that specific writing situation which affect our ability to put the arm, wrist and fingers through the necessary motions (whether my hands are cold or warm; whether [279] I am feeling stressed out or content; whether I imbibed large amounts of liquor or ate a healthy

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<sup>78</sup> JEFFERY, "Response to Handwriting Analysis".

<sup>79</sup> TSELIKAS, "Handwriting Analysis Report," VI.

<sup>80</sup> Tom DAVIS, "The Practice of Handwriting Identification," *The Library: The Transactions of the Bibliographical Society* 8 (2007): 251–276, at 261.

breakfast),<sup>81</sup> and from the amount of control the writer needs to utilize to produce the desired writing (whether this is a personal note just for myself, or if I need to disguise my handwriting or simulate someone else's).<sup>82</sup> To further complicate matters, the actual strokes on the paper do not appear as a set-piece compromise between the internalized model hand and the particulars of the writing situation, but as a range of variations in the forms of letters. This variety in stroke execution—stemming from the fact that no human is a machine—is commonly known as natural variation and should not be confused with inconsistency in writing, the former being a normal part of every handwritten note ever produced, and the latter a suspicious sign of foul play.<sup>83</sup>

From these considerations forensic document examiners have established that some aspects of our handwriting are always produced with more conscious effort than other aspects and that the less conscious aspects are the ones more difficult to disguise; hence, they are also the best place to discover idiographic features of the author.<sup>84</sup> Furthermore, writers attempting to disguise their handwriting or simulate someone else's will eventually switch (*i.e.* fall back, or lapse) into their unconscious habits and make use of those units of movement that form their unique internalized model hand.<sup>85</sup> The fundamental difference between writing with one's own handwriting and imitating someone else's is that the former uses proprioceptive feedback (*i.e.* internal feedback [280]) that allows the body to keep track of the relative positions of its parts)

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<sup>81</sup> For the physiology of producing handwriting, consult Colette SIRAT, *Writing as Handwork: A History of Handwriting in Mediterranean and Western Culture* (Turnhout: Brepols, 2006), 432 and the sources cited therein.

<sup>82</sup> The theory of handwriting described here is based on the following: Tom DAVIS, "Forged Handwriting," in *Fakes and Frauds: Varieties of Deception in Print & Manuscript* (ed. Robin MYERS and Michael HARRIS; Winchester: St. Paul's Bibliographies, 1989): 125–137; Colette SIRAT, "Handwriting and the Writing Hand," in *Writing Systems and Cognition: Perspectives from Psychology, Physiology, Linguistics and Semitics* (ed. William C. WATT; Dordrecht: Kluwer Academic Publishers, 1994): 375–460; DAVIS, "Handwriting Identification".

<sup>83</sup> Katherine M. KOPPENHAVER, *Forensic Document Examination: Principles and Practice* (Totowa, N.J.: Humana Press, 2007), 113–130, 157–166; David ELLEN, *Scientific Examination of Documents: Methods and Techniques* (3<sup>rd</sup> edn; Boca Raton, Fla.: CRC Press, 2006), 35–40; DAVIS, "Handwriting Identification," 260–261.

<sup>84</sup> HUBER and HEADRICK, *Handwriting Identification*, 279; DAVIS, "Handwriting Identification," 254–255, 257, 259, 261. Line quality, in particular, is very hard to simulate; consult Abdulaziz Al-Musa ALKAHTANI and Andrew W. G. PLATT, "A Statistical Study of the Relative Difficulty of Freehand Simulation of Form, Proportion, and Line Quality in Arabic Signatures," *Science and Justice* 50 (2010): 72–76.

<sup>85</sup> Marc J. SEIFER, "Disguise in Handwriting," *Rhode Island Bar Journal* 37 (1988): 23–24.

while the latter is essentially drawing, and relies heavily on visual feedback to maintain its desired form.<sup>86</sup> The continuous attention to form involved in disguise or imitation produces mental fatigue, which results in unconscious lapses into one's personal idiosyncrasies, especially when the disguised or simulated handwriting is of considerable length. These lapses tend to become more frequent towards the end of the document (or the later stages in any uninterrupted period of imitating).<sup>87</sup> These facts about how a forger's idiographic traits enter into a forgery lead to one other observation: that a document, especially one that is of considerable length and written in a difficult script, is probably not simulated or disguised if the writing is consistent from start to finish.<sup>88</sup> It is interesting to note here, as Brown has argued in his response to Anastasopoulou's report, that *Clement's Letter to Theodore* is just that: a long, consistently executed specimen of a difficult eighteenth-century script.<sup>89</sup>

Before we move on to discuss the characteristics of the script in *Clement's Letter to Theodore*, let us return to the objections of Jeffery and Tselikas. Did Anastasopoulou make an apples-to-oranges comparison when she considered the handwriting in the manuscript and the Greek handwriting of Smith? Not exactly: according to the theory of handwriting comparison, she judged the available material adequate for comparison purposes,<sup>90</sup> and assessed the idiographic characteristics of the scripts.<sup>91</sup> The difference between the eighteenth-century cursive and Smith's own Greek letters is not terribly important, for Smith's script provides an expert with a number of idiographic features that should—if *Clement's Letter to Theodore* was penned by Smith—be found here and there in the questioned writing as well, a point Anastasopoulou

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<sup>86</sup> Marie-Claude HEPP-REYMOND et al., "Role of Proprioception and Vision in Handwriting," *Brain Research Bulletin* 79 (2009): 365–370.

<sup>87</sup> HUBER and HEADRICK, *Handwriting Identification*, 283; KOPPENHAVER, *Forensic Document Examination*, 129. This is contrary to Carlson's claim that the writing becomes "more fluid" the longer the forger writes, as he begins "to show some comfort in the hand"; CARLSON, *Gospel Hoax*, 30–31.

<sup>88</sup> ANASTASOPOULOU, "Reveal a Forgery".

<sup>89</sup> Scott G. BROWN, "My Thoughts on the Reports by Venetia Anastasopoulou," *BAR* 37 (2011): 1–14, at 3–10, accessed October 3, 2015, <http://dbcfaa79b34c8f5dffa-7d3a62c63519b1618047ef2108473a39.r81.cf2.rackcdn.com/wp-content/uploads/secret-mark-handwriting-response-brown.pdf>.

<sup>90</sup> ANASTASOPOULOU, "Handwriting Examination," 6.

<sup>91</sup> *Ibid.*, 8–18.

herself made both in her original report and in her later response.<sup>92</sup> [281] As for Tselikas's observation; while we agree that a forger would certainly try to avoid his own personal style while forging, the unconscious and the deeply engrained aspects of handwriting are rarely fully suppressed.

### Controlled and Personal Writing: A Quantitative Analysis

The language peculiar to the field of forensic document examination in Anastasopoulou's report has already been opened up and explained by Brown and does not need to be repeated here.<sup>93</sup> We instead wanted to approach her analysis from a different angle and ask if Anastasopoulou's expert opinion could be quantified in any way. When, for example, she talks about specifics such as spontaneity and consistency, could those qualities be expressed in numbers, to let non-experts in forensic document examination better grasp the nature of the handwriting in *Clement's Letter to Theodore*?

Colette Sirat distinguishes between controlled and personal writing. The first results from any number of "internal restraints" and "external constraints," while the latter is in use when no such inhibitions are present. An example of an internal restraint is the writer's own notion that he or she should write the words down clearly for other people to be able to read the writing, whereas an external constraint could be a requirement to fit one's writing on predetermined lines. These two aspects are present in all of our writing, and we switch between the two modes regularly and unconsciously. The amount of control a writing exhibits will fall somewhere on a sliding scale, with some examples of handwriting showing more control than others. The distinction becomes important when we consider the concept of the internalized model hand. Since writing is generated in units of movement, which become manifest on a page in the form of strokes that range in length and complexity from individual, separated strokes to clusters of letters forming complete words, the writing units are

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<sup>92</sup> "when a hand is accustomed to writing with connections and abbreviations, we generally expect to see at least a trace of them [*italics in original*]"; ANASTASOPOULOU, "Handwriting Examination," 29; "In a questioned suspicious writing we are expecting for the forgers [*sic*] genuine characteristic to come up as the handwritten document is getting larger and in such documents we are looking for a distortion in the writing"; eadem, "Reveal a Forgery". These observations went unnoticed by Jeffery, who repeated the apples-to-oranges accusation in his subsequent response; JEFFERY, "Additional Response".

<sup>93</sup> BROWN, "My Thoughts".

quantifiable: in the case of cursive, connected writing the point where the pen nib comes down on the page is the beginning, and the point where it is lifted the end.<sup>94</sup>

In general terms, personal writing results in an informal script that is fluid and spontaneous, where the individual letters lose some of their distinctiveness and merge into more uniform shapes (*i.e.* different letters of the alphabet begin to resemble each other, resulting in difficulties for other people telling them apart), and abbreviations and ligatures flourish. Control in writing produces the opposite effect.<sup>95</sup> For our purposes here, the most interesting difference lies in the tendency of the units of movement to position themselves along a continuum. For a given person controlled writing leads to a focus on individual strokes while personal writing will steer towards units that are more combinatorial.<sup>96</sup> These considerations lead us to pose the following questions: How many times does the scribe of *Clement's Letter to Theodore* make a pen lift? How many ligatures and abbreviations are there in the text? How many of the glyphs (*i.e.* individual, distinct elements of writing such as a letter or a ligature) are written without the pen being lifted in-between; *i.e.* how many clusters of two, three, four and more glyphs are there? All of these questions are easily (albeit laboriously) answered, and their results are suggestive of how controlled the handwriting in *Clement's Letter to Theodore* is. Moreover, to provide a necessary context for the numbers, a simple comparison with other eighteenth-century manuscripts will suffice. And since the question of forgery is relentlessly upon us, we need only to recall that the process of forgery is essentially one of conscious handwriting control in which internal and external constraints affect every movement of the pen.<sup>97</sup> Hence, as Sirat informs us, "One of the best ways to recognize a forgery is to look at the hints of control, such as an abundance of fresh starts."<sup>98</sup>

For comparison with the handwriting in *Clement's Letter to Theodore* we studied two eighteenth-century manuscripts—a copy of the writings of Gregory of Nazianzus (Add

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<sup>94</sup> SIRAT, "Writing Hand," 426–445; eadem, *Handwork*, 429–434.

<sup>95</sup> SIRAT, "Writing Hand," 435–439; eadem, *Handwork*, 308, 429–434.

<sup>96</sup> SIRAT, "Writing Hand," 439; eadem, *Handwork*, 430.

<sup>97</sup> Katherine M. KOPPENHAVER, *Attorney's Guide to Document Examination* (Westport, Conn.: Quorum Books, 2002), 125. Koppenhaver describes how forgers struggle to follow their exemplars as closely as possible, and how the natural variation in the form of letters disappears in the process.

<sup>98</sup> SIRAT, "Writing Hand," 439; eadem, *Handwork*, 430.

MS 8240)<sup>99</sup> and a personal letter of Konstantinos Dapontes (Add MS 8237)<sup>100</sup>—both of which are quite similar to the Clementine letter in their execution of the Greek script.<sup>101</sup> We took a random sampling of 1000 letters or glyphs (excluding abbreviations and most ligatures), beginning from line fourteen of the first page of all three manuscripts.<sup>102</sup> The results are presented in the table below.

	<b>Theodore MS 65</b>	<b>Dapontes MS 8237</b>	<b>Gregory MS 8240</b>
Total number of letters or glyphs	1000	1000	1000
Individual glyphs	352	460	520
Glyphs in groups of two	382	422	286
Glyphs in groups of three	216	106	153
Glyphs in groups of four or more	50	12	41
Ligatures	107	104	51
<i>Nom.Sac.</i> and other abbreviations	3	3	1

While all three manuscripts have many ligatures and other markings of cursive writing in common—e.g. the circular ligature combining *omicron* and *upsilon*, the *stigma* for marking the combination of *sigma* and *tau*, and the word *καί* written more often than not in one continuous stroke—it is notable that *Clement’s Letter to Theodore* and Dapontes use them twice as often as the copy of Gregory does, while their use of *nomina sacra* and other abbreviated words is thrice as frequent. The signs of control<sup>103</sup>

<sup>99</sup> Add MS 8240, ff 92–109v Gregory of Nazianzus, *Contra Julianum imperatorem* 1, from the manuscript collection of The British Library.

<sup>100</sup> Add MS 8237, ff 2–2v Letter by Konstantinos Dapontes to an anonymous correspondent dated Piperi, 10 February 1754, from the manuscript collection of The British Library.

<sup>101</sup> A good overview of the scripts can be obtained from The British Library’s Digitised Manuscripts website at <http://www.bl.uk/manuscripts/Default.aspx>. Of interest are also the many handwritings at the Zagora library (Βιβλιοθήκη Ζαγοράς, <http://diglib.ypepth.gr/awweb/guest.jsp>) that resemble the one in the Clement letter, especially in the item titled Καλλινίκου πατριάρχου Έργα και αντίγραφα.

<sup>102</sup> Line 14 was randomly chosen in order to eliminate any inconsistency at the beginning of the writing. Although most ligatures are excluded from the letter or glyph count, those ligatures that are combined with other glyphs are included, but then counted as just one glyph each.

<sup>103</sup> We prefer to speak of “signs of control” and not “amount of control,” since the only way to decide how much control a specific writing exhibits is to compare it to other writings of the same individual. An estimation of the control in writings done by different individuals can only be indicative, never absolute.



are most evident in the copy of Gregory, in which more than half of the glyphs are written individually, compared to only 352 individual glyphs (c. 35 %) in *Clement's Letter to Theodore*. A similar trend can be seen in the other clusters of glyphs as well. Dapontes's personal letter stands in the middle in this regard, with slightly less than half of the glyphs written individually, but with the largest number of two-glyph clusters. Of these three eighteenth-century manuscripts, *Clement's Letter to Theodore* exhibits the least signs of control, judged solely by the amount of pen lifts.

While none of the manuscripts could be characterized by Sirat's "abundance of fresh starts," the scribe who copied the text of Gregory seems the most interested in preserving the readability of the writ- [284] ing. Compared to *Clement's Letter to Theodore*, it is generally easy to distinguish between his letters *epsilon* and cursive *pi*, for instance, and the letterforms are less simplified, which implies lesser velocity in executing the strokes. These numbers will hopefully render the expert opinion of Anastasopoulou more intuitive. Her observation that the letters in *Clement's Letter to Theodore* are written "unconsciously,"<sup>104</sup> to take but one example, corresponds well with the number of pen lifts we have counted; *i.e.* the handwriting in the Clementine letter leans towards personal writing, which is the realm of our own internalized model hand, unconsciously and automatically executed—an observation we base on the relative numbers of pen lifts, ligatures and abbreviations, and on the generally poor readability of the script in *Clement's Letter to Theodore* compared to other examples of eighteenth-century handwriting. The implications of our study are clear enough: as the general quality of appearing unconscious and inconspicuous is difficult for forgers to imitate, it follows that the more characteristics of personal writing a script contains, the less likely it is to be a forgery. In the amount of signs of control *Clement's Letter to Theodore* exhibits, it is indistinguishable from genuine eighteenth-century manuscripts.

### The Difference between Forensic Document Examination and Palaeography

At this point we have seemingly arrived at a dead end. If Anastasopoulou's qualitative assessment is right (not to mention the quantitative conclusions we have drawn from the amount of pen lifts) and the handwriting in *Clement's Letter to Theodore* is the spontaneous product of a scribe's internalized model hand, how does Tselikas, in the latest handwriting assessment to date, argue for exactly the opposite: that the

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<sup>104</sup> ANASTASOPOULOU, "Handwriting Examination," 9.

scribe moved his hand in a non-spontaneous manner, drawing the letters carefully while lifting the pen in odd places?<sup>105</sup> We believe the disagreement of opinion between Anastasopoulou and Tselikas derives from the fundamental discrepancy in their respective *modi operandi* as forensic document examiners and palaeographers, two fields of study that traditionally “do not communicate with each other.”<sup>106</sup> Despite their shared goal of trying to identify writings produced by the same hand (demonstrating the conclusions in the language of probabilities as befits academic disciplines), their means to arrive at the said conclusions are not the same.

[285] The two disciplines represented by Anastasopoulou and Tselikas have their own peculiar interests and methods. Much of palaeographers’ work is dedicated to the decipherment of writing in manuscripts and where there is seldom reason to suspect forgery. Forensic document examiners often deal with cases in which deception of some sort is suspected. Palaeographers are keen to obtain as much external evidence about the writer, period, and origin of the manuscript as possible to augment their assessment of the (distant) past, about which there is often much to learn. Forensic document examiners do not shun such external evidence, but usually their purpose is to rely on the internal evidence of the manuscript in order to test others’ assertions about these matters. It is common for them to request known samples (standards) of the handwriting of the suspect and attempt to establish the idiographic characteristics of the internalized model hand of both writers (or one and the same writer, as the case may be). Palaeographers do not usually shy away from “generalizations about the characteristics of a hand,” nor would much history be written without a healthy amount of speculation and conjecture thrown in. Forensic document examiners would be laughed out of the court should they be caught making conjectures without firm evidence to back them up.<sup>107</sup>

For one example of the consequences of these differences, consider how Anastasopoulou and Tselikas framed the amount of line terminations in *Clement’s Letter to Theodore*. For the first, the handwriting was “written in high speed and although ... there are letters written one by one in a word ... this does not deter the good writing rhythm.”<sup>108</sup> For the latter, the great number of letters and links with “non-

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<sup>105</sup> TSELIKAS, “Handwriting Analysis Report,” III.

<sup>106</sup> DAVIS, “Handwriting Identification,” 251.

<sup>107</sup> DAVIS, “Handwriting Identification,” 251–252, 260, 267–273; SIRAT, *Handwork*, 491–497.

<sup>108</sup> ANASTASOPOULOU, “Handwriting Examination,” 12.

continuous lines" indicated "that the hand of the scribe was not moving spontaneously, but carefully and tentatively to maintain the correct shape of the letter."<sup>109</sup> The divergent conclusions stem from concentrating on line continuity and connections, respectively. The latter belongs to style elements and the former to execution elements in Roy A. Huber and A. M. Headrick's standard classification in *Handwriting Identification: Facts and Fundamentals* (1999).<sup>110</sup> As Sirat observes, "style elements are the palaeographers' tools for placing documents into time and space, while execution elements are the document examiners' field."<sup>111</sup> In short, when forensic document examiners encounter the end of the line of a unit of movement, their eyes study the changes in pen pres- [286] sure and trace the direction of the hand, assessing its path from the termination of one line to the beginning of another in order to conclude whether the line continuity remains unbroken; *i.e.* if the rhythm and flow of writing is maintained. These details allow forensic document examiners to pronounce whether the script was executed rapidly and with spontaneity. Palaeographers, on the other hand, interpret line terminations as signifiers for the composition of the document: in which century and in which school of writing were these connections taught? Tselikas's conclusion, in this particular instance, simply does not follow from the phenomenon he scrutinizes, especially given that the amount of non-continuous lines was greater in the other two eighteenth-century manuscripts we studied previously.

The above is not the only instance of vagueness in Tselikas's handwriting analysis. His transcription of *Clement's Letter to Theodore* contains fourteen errors,<sup>112</sup> and on four occasions his own erroneous transcription is offered as evidence that the letter contains blunders that a fluent native Greek writer or scribe could not possibly make.<sup>113</sup> While a comprehensive assessment of Tselikas's report is beyond the scope of the present

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<sup>109</sup> TSELIKAS, "Handwriting Analysis Report," III.

<sup>110</sup> HUBER and HEADRICK, *Handwriting Identification*, 105–146.

<sup>111</sup> SIRAT, *Handwork*, 495.

<sup>112</sup> I.2 ἐπιστομίσαι – should be ...ας; I.5 λέγουσιν – should be ...σι; I.8 το – should be τι; I.19 τα ταυτοῦ – should be ταταυτοῦ; I.26 ἐπτάκις – should be ἐ...; I.27 καὶ – should be και; II.6 καὶ – wrong line (*i.e.* II.7); II.7 δόξα – should be ...αν; II.21 Ἱεροσόλυμα – should be Ἰ...; II.22 ἔξῆς – should be ἐ...; III.8 γυμνῶ – should be ...νοῦ; III.12 προσεπορεύοντο – should be προσπ... and ...ται; III.13 γυμνοὶ – should be ...νός; TSELIKAS, "Handwriting Analysis Report," I. On comparison, Smith's transcription in 1973 contained two errors: I.12 εὐαγγέλιου – should be ...ελίου; I.25 τοὺς – should be τοῦς; SMITH, *Clement*, 448–452.

<sup>113</sup> #4, #7, #18, #25; TSELIKAS, "Handwriting Analysis Report," II.

paper, it is necessary to discuss here those observations that pertain directly to handwriting. Two statements of Tselikas in particular should be noted: first, that the script in *Clement's Letter to Theodore* exhibits signs of “poor knowledge of Greek writing”; second, that the comparison between the handwriting in the Clementine letter and Smith's Greek hand provides evidence that the late professor of ancient history did, in fact, forge the letter—contrary to Anastasopoulou's assessment. The latter is relatively easy to explain, for the exact same misunderstandings and misapplications were previously made by Carlson in his attempts to connect the Clementine hand with Smith. As these have already been examined in-depth by Brown,<sup>114</sup> a brief summary will suffice. Tselikas supposes that he can reasonably link Smith to *Clement's Letter to Theodore* by pointing out similarities in their renderings of two letters (τ and θ), two letter-combinations (θη and θου), [287] and a few accents, and by offering one example from each labelled “instability of writing.”<sup>115</sup>

This approach fails first in ignoring dissimilarities between idiographic features of the handwritings—in authorial identification these are of far greater importance. Huber and Headrick even state that “a limited number of differences, perhaps only one” could be enough “to offset the weight of a number of similarities, regardless of their respective importance.”<sup>116</sup> Furthermore, Tselikas does not take into account the phenomenon of natural variation; *i.e.* he does not establish the “range of variation” for those particular letters by examining many examples in order to find the “typical shape” or “master pattern” of those letters.<sup>117</sup> Without the assessment of the extent of the variability of the letterforms a given writer produces, it is trivial to find the occasional match between any handwriting that utilizes the same alphabet,<sup>118</sup>

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<sup>114</sup> BROWN, “Factualizing the Folklore,” 295–306.

<sup>115</sup> TSELIKAS, “Handwriting Analysis Report,” VI.

<sup>116</sup> HUBER and HEADRICK, *Handwriting Identification*, 50–51. Cf. Marie-Jeanne BERRICHON-SEDEYN, “Acte mécanique ou présence vivante?” in *L'écriture: le cerveau, l'œil et la main. Actes du colloque international du Centre National de la Recherche Scientifique, Paris, Collège de France, le 2, 3 et 4 mai 1988. Bibliologia: Elementa ad librorum studia pertinentia*, 10 (ed. Colette SIRAT, Jean IRIGOIN and Emmanuel POULLE; Turnhout: Brepols, 1990): 221–235, at 227–228.

<sup>117</sup> BROWN, “Factualizing the Folklore,” 300.

<sup>118</sup> Ordway HILTON, *Scientific Examination of Questioned Documents* (Boca Raton, Fla.: CRC Press, 1993), 161.

especially with relatively simple letters such as *tau* and *theta*.<sup>119</sup> The difference between palaeographers and forensic document examiners is quite pronounced at this point. For one example, Tselikas offers two instances of the letter *tau* written individually in *Clement's Letter to Theodore*, both of which exhibit a different allographic variation of the letter (small *tau* with counter-clockwise loop and tall *tau* with clockwise curve).<sup>120</sup> When he subsequently places the shorter one side by side with Smith's *tau*, there is no indication that the range of variety in the execution of that particular letter has been accounted for, apart from the obvious distinction between the tall and short form.<sup>121</sup> Differences and similarities at the allographic level, however, are useful in cases where no deceit is suspected (*i.e.* in the usual palaeographic practice [288] of scribe identification)<sup>122</sup>, or where the forger has lapsed into his own writing. The basic problem here is that forgers are imitating someone else's writing, so if the forged writing agrees with the suspect's writing, that is only relevant where an inconsistency (lapse) occurs. Tselikas never shows that the examples he presents are such inconsistencies. If Tselikas never considered going beyond the conspicuous, his juxtapositions of the letters can tell us nothing. As we have stated previously, forensic document examiners concentrate in handwriting comparison on the idiographic, less conscious characteristics of handwriting, for the very reason that features of the internalized model hand are more difficult to disguise in small, inconspicuous details.

What then of Tselikas's list of nineteen examples of poor knowledge of Greek lifted from the script in *Clement's Letter to Theodore*? These can be classified into examples of rare usage of the letterform in manuscripts,<sup>123</sup> examples of cursive hand in which letterforms fall towards uniformity,<sup>124</sup> examples of inconsistency in letterforms,<sup>125</sup>

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<sup>119</sup> Léon Gilissen, for instance, suggests that only the examination of more complex signs enables the palaeographer to safely distinguish between different scribal hands; Léon GILISSEN, *L'expertise des écritures médiévales: Recherche d'une méthode avec application à un manuscrit du XIe siècle: Le lectionnaire de Lobbes. Codex Bruxellensis 18018* (Les publications de Scriptorium 6, Gand: Éditions scientifiques E. Story-Scientia, 1973), 47, 144.

<sup>120</sup> TSELIKAS, "Handwriting Analysis Report," III.

<sup>121</sup> *Ibid.*, VI.

<sup>122</sup> DAVIS, "Handwriting Identification," 254–255.

<sup>123</sup> #2, #5, #7, #11, #14, #16, #18.

<sup>124</sup> #3, #7, #8, #9, #10, #12, #13, #14, #16, #18.

<sup>125</sup> #4, #6.

examples of pen lifts in odd places,<sup>126</sup> and two special cases regarding the use of *nomina sacra* and the use of colons at the end of the line.<sup>127</sup> The majority of the examples, however, do not have much to do with “poor knowledge of Greek writing.” We cannot see how existing (albeit rare) or simplified letterforms would disclose the amount of knowledge of Greek writing a given scribe possesses.<sup>128</sup> There is no problem in the attestation of eighteenth-century letterforms in *Clement’s Letter to Theodore*, and as no one denies that the Clementine letter is written (superficially, at least) by a cursive hand, the simplified letterforms are certainly expected. If these and other signs of cursive hand were missing, we would not call the handwriting cursive.<sup>129</sup> Furthermore, in forensic [289] document examination it is generally held that “abbreviated, distorted and illegible forms, which are sufficiently free and rapid, often actually indicate genuineness rather than forgery even though they are very unusual and not exactly like those in the standard writing.”<sup>130</sup>

Nor do we find troublesome the minor inconsistencies Tselikas spotted, which typify the difference between forensic document examiners and palaeographers. While Anastasopoulou made it her business to focus on line quality and pen pressure, Tselikas directed his attention mostly towards what is conspicuous in the letterforms—a practice well suited to assigning a document to a particular century, but not that suitable for deciding if it is a forgery. Consider, for instance, Tselikas’s claim that the scribe of *Clement’s Letter to Theodore* wrote the letter *delta* in two parts, first the lower circle and then the upward line with a pen lift in-between. It should be noted, however, that most *deltas* are done in one stroke. Tselikas considered this particular oddity to be suspicious, but forensic document examiners from Osborn onwards have maintained

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<sup>126</sup> #1, #5, #15, #17. It is possible that #14 should also be added to this list, but we are unable to interpret Tselikas here. He claims that this scribe writes the letter-combination  $\sigma\iota$  with the miniscule *sigma* by first writing the *sigma* and then attaching the *iota* to the *sigma* beginning from the lower part of the *iota*. As far as we can tell, he also does so in I.24 and possibly also in other places. Since the drawing is done in the opposite direction, this would not count as a pen lift but as two letters written independently. TSELIKAS, “Agamemnon Tselikas’ Handwriting Analysis Report,” IV.

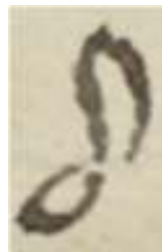
<sup>127</sup> #11, #19.

<sup>128</sup> They might, however, disclose details of the specific school a given scribe was trained in. Smith, for example, conjectured that the scribe of *Clement’s Letter to Theodore* was trained in the Patriarchal Academy of Constantinople; SMITH, *Clement*, 3.

<sup>129</sup> For the characteristics of cursive handwriting, consult SIRAT, *Handwork*, 308–309.

<sup>130</sup> OSBORN, *Questioned Documents*, 365.

that “the most common symptom of forgery is not, as is incorrectly thought by many, divergence in form but a drawn and hesitating quality of stroke or line.”<sup>131</sup> Occasional oddities in letter formation are, rather, a sign of genuineness, and given the thousands of letters in *Clement's Letter to Theodore*, it would be most suspicious if no occasional inexplicable oddities were to be found.<sup>132</sup> As previously mentioned, the tendency in forgery is toward legibility and exact reproduction of the shape of the letterforms used as exemplars, to the detriment of line quality and other assorted characteristics of the handwriting. If anything, this tendency leads to an artificial uniformity in the writing, whereas natural handwriting contains deviations from the norm. As an illustration, consider the image of a letter *delta* below, which is taken from the personal letter of Dapontes, written in the eighteenth century (from one of the manuscripts we studied for numbers of fresh starts). This letter is written quite atypically in two strokes, but this quirk tells us nothing about Dapontes's knowledge of Greek. Furthermore, since students of Ancient Greek are usually taught the proper way to write the letter *delta*, it is much easier to imagine a fluent native Greek writer feeling free to vary his execution of the letter, than it is to imagine a forger skilled enough to produce *Clement's letter to Theodore* with such accuracy writing such a simple letter in a non-standard way.



The few remaining remarks concerning other unnatural pen lifts, *nomina sacra* and the use of colons require further commentary.

[290] Tselikas notes that “two dots occur when a word is divided at the end of the verse, but not always.”<sup>133</sup> Accordingly, he interprets these dots as hyphens. The inconsistency with which the scribe of *Clement's Letter to Theodore* would have utilized colons as hyphens should not concern us much, as such discrepancy even within a

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<sup>131</sup> *Ibid.*, 364.

<sup>132</sup> HILTON, *Questioned Documents*, 158–160; KOPPENHAVER, *Attorney's Guide*, 125, 171.

<sup>133</sup> Tselikas, “Handwriting Analysis Report,” IV.

single manuscript by the same hand can be easily attested.<sup>134</sup> Since scribes, however, never had a unified system for the usage of typographic marks (including punctuation), and because the function of said marks can only be decided on a case-by-case basis by assessing them in their particular manuscript context,<sup>135</sup> we propose another use for colons in *Clement's Letter to Theodore*: a mean to keep the right margin of the text straight.<sup>136</sup> Examples of various typographic marks used to justify margins are found from ancient to modern times.<sup>137</sup> Individual typographic marks such as colons could be used to indicate a parenthesis or in place of comma,<sup>138</sup> or to indicate a strong pause;<sup>139</sup> they are put to use as line-fillers e.g. in *The Gospel of Judas*,<sup>140</sup> and plausibly in the Dead Sea scrolls such as 1QIsa<sup>a</sup> as well.<sup>141</sup> That the scribe of *Clem- [291] ent's Letter to Theodore* used colons as line-fillers can be established by three observations. First, the letter consists of 71 lines (excluding the heading), 31 of which are syllabified, 13 of which have colons, while six of those colons occur at the end of those lines that are also

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<sup>134</sup> Consult, e.g. Charles Brewster RANDOLPH, "The Sign of Interrogation in Greek Minuscule Manuscripts," *Classical Philology* 5 (1910): 309–319, at 309 and the sources cited therein.

<sup>135</sup> Malcolm Beckwith PARKES, *Pause and Effect: An Introduction to the History of Punctuation in the West* (Hants: Scolar Press, 1992), 2.

<sup>136</sup> This was originally suggested by SMITH, *Clement*, 2.

<sup>137</sup> For the ancient times, the standard textbook Eric Gardiner TURNER, *Greek Manuscripts of the Ancient World* (London: Oxford University Press, 1971) provides a number of examples; medieval examples can be found e.g. in Paul BINSKI and Patrick ZUTSHI, *Western Illuminated Manuscripts: A Catalogue of the Collection in Cambridge University Library* (Cambridge: Cambridge University Press, 2011), and in Raymond CLEMENS and Timothy GRAHAM, *Introduction to Manuscript Studies* (Ithaca: Cornell University Press, 2007); for two examples from the seventeenth century, consult *The Southwell-Sibthorpe Commonplace Book: Folger MS. V.b.198* (ed. Jean KLENE, C.S.C.; Medieval & Renaissance Texts & Studies 147; Tempe, Ariz.: Renaissance English Text Society, 1997), and Add MS 81083, from the manuscript collection of The British Library.

<sup>138</sup> PARKES, *Pause and Effect*, 48–49.

<sup>139</sup> Nicephori BLEMMYDAE, *Autobiographia sive curriculum vitae necnon epistula universalior* (ed. Joseph A. MUNITIZ; Corpus Christianorum Series Graeca 13; Brepols: Leuven University Press, 1984), liii.

<sup>140</sup> Lance JENOTT, *The Gospel of Judas: Coptic Text, Translation, and Historical Interpretation of "the Betrayer's Gospel"* (Studien und Texte zu Antike und Christentum 64; Tübingen: Mohr Siebeck, 2011), 117.

<sup>141</sup> Emanuel TOV, "Scribal Markings in the Texts from the Judean Desert," in *Current Research and Technological Developments on the Dead Sea Scrolls: Conference on the Texts from the Judean Desert, Jerusalem, 30 April, 1995* (ed. Donald Wayne PARRY and Stephen David RICKS; Studies of the Texts of the Desert of Judah 20; Leiden: Brill, 1996): 41–77, at 68.



syllabified. In other words, c. 44 percent of the lines (31/71) are divided, and—should the colons be distributed randomly—c. 5.7 lines in this letter ( $13 \times 31/71$ ) should end with both a divided word and a colon. The actual outcome matches the statistics almost perfectly, as the colons coincide with a word division in six instances. Second, every line that ends with a colon would have ended to the left of the line above without it. Third, typographic usage of colons is evident on lines I.12 and I.26 where the space between the last letter of the line and the colon is highly pronounced, and the colon justifies the right margin exactly. As for the somewhat unusual forms of *nomina sacra* in *Clement's Letter to Theodore*, the variety probably follows from their fall into disuse by the eighteenth century, as discussed by Émile de Strycker in his survey of the manuscript copies of the *Infancy Gospel of James* through the centuries.<sup>142</sup>

Finally, the one remaining problem is pen lifts in places where they are uncalled for.<sup>143</sup> None of the four examples Tselikas presents are easily detected as non-continuous. The connecting line between *spiritus lenis* and the letter *alpha* seems continuous to us in all of the 13 instances, apart from maybe one (ἀπόγραφον in Theod. II.6). In the 43 cases of the letter *upsilon* connected with the circumflex accent, we can find only a few examples in which the line with some probability could be described as non-continuous, and these exceptions are quite explainable. They are all coherent

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<sup>142</sup> Émile de STRYCKER, "Notes sur l'abréviation des *nomina sacra* dans des manuscrits hagiographiques grecs," in *Studia Codicologica* (ed. Kurt TREU; Texte und Untersuchungen zur Geschichte der altchristlichen Literatur 124; Berlin: Akademie-Verlag, 1977): 461–467. De Strycker also notes the extent of variety in the execution of *nomina sacra*. For the word ἰσοράηλ, for instance, he records no less than four different abbreviated forms.

<sup>143</sup> If that is what Tselikas means when he speaks of non-continuous lines. We contacted him to obtain clarifications for some of his statements and asked him: 1) if he had some particular examples in mind, or if he meant that the line is non-continuous in every instance; 2) if he by non-continuous lines means that the scribe has actually lifted his pen, and 3) if the somewhat peculiar "twist" this scribe sometimes makes when writing certain letters is the place in which he perceives the pen lift? Tselikas replied that he was "not willing to deal" with this matter anymore. The only answer he had to our questions was that "the scribe shows hesitation in completing the letters" and that "the writing of many letters is not flowing, but split, which indicates that the scribe did not have enough experience in Greek script." He also emphasized that he is "not a handwriting analyst, but a paleographer with extensive experience in Greek script"; personal communication. Recently, Tselikas has been challenged to explain his reasonings by HEDRICK, "Secret Mark," 35 n. 21.

with the way this scribe wrote [292] certain letters or letter-combinations.<sup>144</sup> Tselikas also claims that “the upper section” of the letter *theta* has been added to the lower part of the letter, and that the letter *delta* is written in two parts. Yet he never clarifies whether this applies to some or all of the particular letters. Nevertheless, we can find only one clear example (among 59 possible) of the letter *theta* written with two strokes (ἀληθῆς in III.18a).<sup>145</sup> The letter *delta* is sometimes done in two strokes with the upward line being added to the lower part.<sup>146</sup> This, however, shows only that this [293]

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<sup>144</sup> There are a total of 56 circumflexed *upsilons* (ῥ) in the manuscript. Thirty-seven of those are in the word τῶ, and there the *tau* is always written alone, the *omicron* and the *upsilon* are written as ligature and are as far as we can tell connected with the circumflex accent in a continuous stroke in every case. Of the remaining 19 circumflexed *upsilons*, 13 are written with the circumflex as a tilde (this includes all 7 instances of Ἰησοῦ...). The remaining 6 times the circumflex is seemingly done as a continuation of the *upsilon* or *omicron-upsilon* ligature and then sometimes apparently done separately. But this is all explainable. In the *omicron-upsilon* ligatures of II.13 θεοῦ, II.24 ...θοῦ... and III.10b θεοῦ, the circumflex is probably added in II.24 and possibly in III.10b. But then also θοῦς in I.11 is written with a tilde, which indicates that this scribe used to add the circumflex afterwards when an *omicron-upsilon* ligature was preceded by *theta* or both *theta* and *epsilon*. The δοῦ in I.4 is not done as a ligature and the circumflex seems to be added to the *upsilon* in a continuous stroke. If the circumflex was added to III.8 γυμνοῦ (and this is not obvious) it would still be in line with the fact that it was written as a tilde in II.15 νοῦν (which shows that the scribe also used to add the circumflex separately in the letter-combination νοῦ). Also in II.6 μυστικοῦ the circumflex seems to be added in a separate stroke. Since this is the only instance of the letter-combination κοῦ, there is no way to tell if this scribe used to add the circumflex afterwards also in these cases, although the *kappa* and the *epsilon* are written quite similarly.

<sup>145</sup> Though there are other possible examples. In I.7a, II.16b and II.25b there are minor dislocations between the lines, which could be a sign of the line being done in two strokes. Yet this could just as easily be an effect of ink spread. In I.7b, I.9c, I.11a, I.24, III.2a, III.2b and III.6a there are barely visible openings in the lines. As similar openings are found on many other places in the writing they are probably due to loss of pen pressure. Only in III.18a and III.5 are there rather large openings. The first seems to be due to the pen being lifted and the *theta* accordingly done in two strokes. The second *theta* might also have been done this way, but it could also have been drawn in one continuous movement where the pen was lifted somewhat while the hand was moving.

<sup>146</sup> There are 70 *deltas* in the manuscript. This letter is most probably done in two strokes in III.17, as the upward stroke was added to the bottom line of the lower circle. It is probably also done in two strokes in I.15, I.22, II.8a, II.13a, II.17 and III.4. In other instances it is possible that the up-going line is added afterwards but it is often impossible to know for certain, not least due to the lower part of the *delta* frequently being done so small that it almost becomes just one ink blob. The possible places are I.8, I.12a,b, I.16, I.23a,b,c, I.26, II.1, II.2, II.3, II.4a,b, II.5a, II.8b, II.9, II.15a, II.21a, II.23, II.25a,b, III.9a and III.14.

scribe could vary the way he wrote this letter and is better viewed as a personal idiosyncrasy.

Sometimes there are hooks of different magnitudes in the middle of the lines, as if the scribe suddenly twisted his hand. These are extremely noticeable, and obviously a characteristic of this particular scribe. The hooks are present in some lines from the *alpha* up to the *spiritus lenis*, where instead of a smooth curve there is a more narrow turn in an angle of 90 degrees or more. In the letter *theta* there is occasionally a distinct angle of about 45 degrees in the transition between the lower part of the letter and the more upward stroke.<sup>147</sup> Similar sharp turns of direction can also be found in the connecting line between the letter *upsilon* and the circumflex accent. We cannot tell whether or not Tselikas considered these to be non-continuous lines. As far as we can see, this is a shift in direction and not a pen lift or a stop (as there are no obvious ink blobs). In many instances, the line gets thinner and sometimes disappears, probably due to loss of pen pressure, the nib hitting and skipping a raised fibre, or the nib running out of ink. Normally when the line vanishes in these letters or letter-combinations it connects anew along the original trajectory of the pen. Most importantly, there are seldom any obvious ink blobs where the line ends or when it begins anew, indicating that the pen is moving rather swiftly in a continuous movement.

Since neither of the authors of this article is an expert forensic document examiner, it seemed prudent to contact Anastasopoulou and ask for her opinion on these alleged non-continuous lines. She responded that the movement, in her professional opinion, is continuous and that she cannot explain Tselikas's wording. Beyond that, however, she did not wish to comment on Tselikas's report, because he comes "from another professional point of view."<sup>148</sup> It should be clear at this point that Tselikas is not talking about or perceiving the same phenomenon as Anastasopoulou, no doubt due to his training as a palaeographer instead of a forensic document examiner. Tselikas's unfamiliarity with the latter is evident in his disregard of important forensic elements in his comparison between Smith's Greek hand and the scribal hand in *Clement's Letter to Theodore*.

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<sup>147</sup> Most obvious examples are the second *theta* on line I.5 and the *theta* on line II.12.

<sup>148</sup> Personal communication.

## Epilogue

Our study of the signs of control in *Clement's Letter to Theodore* might elicit the objection that the highly connected character of this handwriting could represent nothing more than the kind of handwriting that a forger chose to simulate. That is, might we not have misidentified a surreptitiously controlled writing as a relatively uncontrolled personal writing, lacking, as we do, the credentials of a forensic document examiner? This objection would miss the point in two respects. First, the observation that the script in the Clementine letter is written spontaneously and unconsciously is not ours, but rather one made by Anastasopoulou. Our quantitative study merely supplements her qualitative opinion and offers some numbers to render her assessment more intuitive for non-specialists in handwriting studies to grasp. Second, although forensic document examiners tell us that it is extremely difficult to produce a natural-looking imitation of writing that is skilful, artistic, and complex (as the manuscript in question is), and that it is all but impossible to imitate the rhythm that it displays, there remains the banal truism that, in Osborn's words, "perfect forgery cannot be detected by anyone."<sup>149</sup> The long lists of "signs of genuineness" and "signs of forgery" found in the literature on handwriting studies are only indicative and never absolute. Exceptions to these rules exist, whether they are scripts that are executed with a degree of skill that renders them almost indistinguishable from authentic writing,<sup>150</sup> or authentic handwritings that exhibit all the signs of forgery in the book.<sup>151</sup>

In most discussions of *Clement's Letter to Theodore*, it is not recognized that these tools of authenticity detection do not answer the question of whether or not a given document is authentic, but rather, whether or not a given document is indistinguishable from an authentic document. Strictly speaking, the methods that forensic document examiners have at their disposal are designed to answer two questions: Is this particular writer responsible for this particular document? And does

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<sup>149</sup> OSBORN, *Questioned Documents*, 367.

<sup>150</sup> The handwriting in the so-called salamander letter is often cited as an example of master forgery, though other details in its production offered food for suspicion; consult Joe NICKELL and John F. FISCHER, *Crime Science: Methods of Forensic Detection* (Lexington, K.Y.: University Press of Kentucky, 1999), 183–188.

<sup>151</sup> The handwriting of Robert Burns is a prime example; Kenneth W. RENDELL, *Forging History: The Detection of Fake Letters & Documents* (Norman, Okla.: University of Oklahoma Press, 1994), 83.

this particular document exhibit signs of forgery? The trouble with setting one's mind to explore the grounds for doubt in cases for which both questions are answered in the negative is that doubt becomes intractable, because the evidence that points to authenticity is being ignored. This kind of hermeneutic suspiciousness serves to breed yet more suspiciousness. For instance, if we are to conjecture, as Jeffery<sup>152</sup> and Tselikas<sup>153</sup> have done, that Smith might have had a collaborator responsible for the production of the difficult eighteenth-century script, [295] we suddenly have a conspiracy for the manufacturing of a faux early Christian text for reasons that are hard to understand. Suppose, for argument's sake, that we then accept Pantuck's demonstration of the inadequacy of Smith's Greek language skills for the composition of a Clementine letter.<sup>154</sup> In that case, whom do we conjecture to have assisted Smith in this task? Is there, apart from Smith, more than one conspirator involved, and who is this person or persons whose skills surpassed those of Smith's in the crucial discipline of manuscript forging?

The tenacious attachment to the forgery hypothesis has been made possible by its adherents' concentration on the purported motives of Smith.<sup>155</sup> Unfortunately, such motives as they relate to manufacturing forgeries—lacking a death-bed confession, which Smith did not make—are necessarily ambiguous, and the actions from which they were inferred can usually be ascribed to a different motive, as was the case with the inferences from Smith's certainty regarding the evidential value of *Clement's Letter to Theodore*.<sup>156</sup> For this reason, we have opted to refer to Smith's motives only when they have been explicitly stated by Smith himself, and have thereafter sought to interpret Smith's actions as applicable. The emerging picture of Smith the manuscript hunter, who places intrinsic value on the production of inventories and catalogues,<sup>157</sup>

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<sup>152</sup> JEFFERY, "Additional Response".

<sup>153</sup> HEDRICK, "Interview," 65 (#30); HEDRICK, "Secret Mark", 36 interprets Tselikas's position as follows: "Smith were the "mastermind" behind the forgery, someone else would have had to provide the technical skills to pull it off".

<sup>154</sup> PANTUCK, "Ability".

<sup>155</sup> Cf. HEDRICK, "Secret Mark", 32.

<sup>156</sup> See footnote #11.

<sup>157</sup> On his letter to Gershom Scholem dated August 1, 1955, regarding the uncatalogued collection of manuscripts in the Meteora (a group of Greek monasteries), Smith noted that "nothing approaching even an adequate check list has as yet been published, so it's a worthy cause"; *Correspondence*, 80 (#40).

is perfectly honest, akin to Stroumsa's "total trustworthiness of Smith's account".<sup>158</sup> Yet we are far more satisfied with arguments that do not hinge on alleged motives, but assert more demonstrative theses. Such theses include Smith's lack of skill regarding patristic Greek and eighteenth-century cursive script, or—as we have attempted to substantiate in this article—the signs of control present in the handwritten script itself.

In a previous article, we have rejected virtually all of Carlson's contributions to the study of the handwriting in *Clement's Letter to Theodore*.<sup>159</sup> In this article, we have rejected many of Tselikas's contributions. It is our opinion that nothing can salvage Carlson's analysis, because it is based on sources that are fundamentally flawed due to line [296] screen distortion.<sup>160</sup> Though the implications of Tselikas's palaeographic observations have not been exhaustively assessed in this article, it is our opinion that these observations are not probative with respect to the question of forgery. We maintain that Tselikas has made a number of "common-sense" inferences regarding the signs of forgery, especially where he argues for Smith being the forger, that are simply wrong in light of forensic considerations. In short, if *Clement's Letter to Theodore*, as we have argued in this paper, is indistinguishable from an authentic eighteenth-century manuscript, there is no basis for treating it as anything else than a manuscript copy from the eighteenth century.<sup>161</sup>

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<sup>158</sup> STROUMSA, "Introduction," xxi.

<sup>159</sup> VIKLUND and PAANANEN, "Distortion".

<sup>160</sup> The report of Edison that Carlson had brought into the discussion cannot strengthen his position because Edison viewed the same distorted images. Moreover, contrary to the common inference that she endorsed his conclusions (e.g. EVANS, "Grounds for Doubt," 91), an inference Carlson has permitted by withholding significant portions of this report, she in fact declined to offer a professional opinion due to her unfamiliarity with Greek handwriting, and also noted a fundamental problem with his method; consult BROWN and PANTUCK, "Questionable," for details.

<sup>161</sup> Cf. Eckhard RAU, "Weder gefälscht noch authentisch? Überlegungen zum Status des geheimen Markusevangeliums als Quelle des antiken Christentums," in *Jesus in apokryphen Evangelienüberlieferungen: Beiträge zu außerkanonischen Jesusüberlieferungen aus verschiedenen Sprach- und Kulturtraditionen* (ed. Jörg FREY and Jens SCHRÖTER; Tübingen: Mohr Siebeck, 2010): 139–186, at 186.