

# **From Stalemate to Deadlock: Clement's Letter to Theodore in Recent Scholarship**

## ***Abstract:***

This article reviews the literature pertaining to the recent debate over the question of authenticity of Clement's Letter to Theodore (including the so-called Secret Gospel of Mark) and argues that the academy has tied itself into a secure deadlock. The current 'trench warfare' situation is due to various scholarly malpractices, which include the practice of non-engagement with other scholars, abusive language towards them and mischaracterization of their position. In order to remedy the situation and move the discussion forwards a number of correcting acts are suggested.

## ***Keywords:***

Clement of Alexandria; forgeries (modern); Letter to Theodore; Secret Gospel of Mark

## ***Introduction***

The purpose of the present paper is to assess the most recent debate concerning the authenticity of the Letter to Theodore by Clement of Alexandria (including two extracts from the so-called Secret Gospel of Mark), a text of uncertain provenance known only from a single allegedly 18th-century manuscript. Specifically, the paper will focus on the accusations of forgery, laid against Morton Smith, the purported discoverer of the manuscript. Following a summary of the earlier discussion much space will be devoted to the latter half of the 00s, during which time the debate has rekindled with new perspectives challenging the previously undecided academy. I will strive to point out the odd demeanour of the discourse since the publication of the text of the letter in 1973, and how little the debate has changed in the last five years after the publication of Stephen

C. Carlson's 'The Gospel Hoax: Morton Smith's Invention of Secret Mark' (2005), in which the notion that the letter is a hoax perpetrated by Smith has been argued with much persuasive power. In order to keep the presentation of the relevant literature uncluttered, two chronological threads will be utilized. The claims for forgery will be discussed first, followed by the counter case for authenticity, and two important questions regarding the handwriting in the manuscript and the character of Smith will both have dedicated sections of their own. Furthermore, the claims for forgery are classified into two categories, due to differences between the two recent monographs on the subject, the aforementioned 'The Gospel Hoax' and Peter Jeffery's 'The Secret Gospel of Mark Unveiled' (2007a). These categories are *the hoax hypothesis* for the first, and *the double entendre hypothesis* for the latter. Lastly, concluding remarks on the nature of the debate and its future prospects will be offered. Conscious of the dangers of understatement I am nevertheless compelled to observe that the academy has not yet come to a firm decision regarding the authenticity of the Clementine letter. Or, to put it bluntly, the academy is locked in a vicious circle of ever more diminishing engagement, as discussed at the very end of the paper.

A detailed account of the discovery of the manuscript of Clement's Letter to Theodore is found in a book that Smith wrote for the general public (1973b: 1-25). Briefly recounted, Smith, who had been appointed Assistant Professor of Ancient History at Columbia University a year before, was cataloguing the manuscripts present in the tower library of the monastery of Mar Saba in 1958. Near the end of his stay he stumbled upon a printed edition of the authentic letters of Ignatius of Antioch. Scrawled on blank back pages of the book were two and a half pages of tiny cursive handwriting, later assessed to be typical of the 18th-century by the experts. Smith took three sets of black-and-white photographs of the manuscript and returned the book to its place. Subsequently, unbeknownst to him, the manuscript stayed in the monastery until 1976 when a group of scholars that included Guy G. Stroumsa transferred to the Orthodox Patriarchate Library in Jerusalem (Stroumsa 2003: 147-48). About this time the manuscript pages were removed from the

book and photographed in colour by the head librarian Kallistos Dourvas. The whereabouts of the manuscript were known until Dourvas' retirement in 1990, but have been unknown for the past 20 years. Recent endeavours to locate it have been unsuccessful, and tests on the physical manuscript have never been performed (Hedrick and Olympiou 2000: 8-9; Brown 2005: 25).

The first line of the handwriting read, in Smith's translation, 'From the letters of the most holy Clement, the author of the Stromateis. To Theodore.' The letter appears to be Clement's response to a query from Theodore, concerning a variant of the Gospel of Mark which Theodore had encountered whilst conversing with the Carpocratians, a group of early Christians heavily vituperated by various church fathers. Clement affirms that in Alexandria Mark the evangelist expanded the Gospel that he had written in Rome during Peter's lifetime, and that this 'μυστικὸν εὐαγγέλιον' (Theod. II.6,12; 'secret Gospel' in Smith's translation) was still in use in Alexandria. The Carpocratian version, however, was yet again a different variant. According to Clement, Carpocrates had obtained a copy of the Secret Gospel and 'polluted' its text by 'mixing with the spotless and holy words utterly shameless lies' (Theod. II.8-9). For one thing, Mark never wrote the words 'naked man with naked man', which Theodore asked about (Theod. III.13). To dispel the misinformation Theodore had been given, Clement cites two passages from the Secret Gospel of Mark, placing the first between Mk 10.34 and 10.35, and the second after the first sentence in Mk 10.46.

'And they come into Bethany. And a certain woman whose brother had died was there. And, coming, she prostrated herself before Jesus and says to him, 'Son of David, have mercy on me.' But the disciples rebuked her. And Jesus, being angered, went off with her into the garden where the tomb was, and straightway a great cry was heard from the tomb. And going near Jesus rolled away the stone from the door of the tomb. And straightway, going in where the youth was, he stretched forth his hand and raised him, seizing his hand. But the youth, looking upon him, loved him and began to beseech him that he might be with him.

And going out of the tomb they came into the house of the youth, for he was rich. And after six days Jesus told him what to do and in the evening the youth comes to him, wearing a linen cloth over his naked body. And he remained with him that night, for Jesus taught him the mystery of the kingdom of God. And thence, arising, he returned to the other side of the Jordan' (Theod. II.23-III.11).

'And the sister of the youth whom Jesus loved and his mother and Salome were there, and Jesus did not receive them' (Theod. III.14-16).

### ***The first three decades of the debate***

Following the publication of the letter text in 'Clement of Alexandria and a Secret Gospel of Mark' (Smith 1973a), a number of scholars have attempted to summarize the state of the debate. In 1982 Smith himself conducted a review of the first ten years of research. At that time, the authenticity of the Clementine letter was accepted by almost everyone 'though', he noted, 'a substantial minority are still in doubt' (p. 451). Opinions were more divided over the composition of the Secret Gospel of Mark. Some scholars saw pre-Markan traditions behind it, while others judged the gospel text to have emerged from bits and pieces of canonized material, augmented by the imagination of the compiler. The one thing everyone agreed on concerned the historical significance of the story of Jesus and the young man. No one accepted Smith's suggestion that the historical Jesus practised baptism as a rite of initiation into the kingdom of God and as a liberation from the Mosaic law (p. 455). In the end, Smith was slightly disappointed because of the low number of even 'relatively objective studies', concluding that 'serious discussion has barely begun' (p. 449).

The reader of Smith's review cannot but notice some odd slurs and insults thrown around, clear indications that something in the scholarly discussion was not right. Why does Smith have to reproach Werner Kümmel's treatment of the Secret Gospel (Kümmel 1975) as 'a disgrace both to the *Theologische Rundschau* and to the objective tradition of German criticism' (p. 451)? Why does

Smith have to completely dismiss form criticism and redaction criticism as full of 'mutually contradictory conjectures' (p. 455)? And why does Smith have to remark in an abridged version of the article that the names Achtemeier and Fitzmyer both rhyme with *liar*, a 'curious coincidence' indeed (Smith 1985: 150 n. 7)? Answers for such perplexing questions were tracked down by Shawn Eyer in an article with an apt subtitle 'How Morton Smith's Discovery of a Lost Letter of Clement of Alexandria Scandalized Biblical Scholarship' (1995). When Smith published the manuscript in 1973, he did so in not one but two monographs. The latter to have come out of the printers was an exhaustive yet erudite scholarly treatise, with readable photographic plates of the three manuscript pages included (Smith 1973a). The first one, however, was a popularizing account which elaborately narrated Smith's journey to the monastery of Mar Saba and the events there, leading to the discovery of the unknown Clementine letter in 1958 (Smith 1973b). Eyer speculated that some of the unwritten rules of the guild were broken in the process. Smith not only attributed way too much historical significance to the story of Jesus and the young man, but dared to offer the impish suggestion that the initiation of the young man in the mystery of the kingdom of God might have involved not only spiritual symbolism of union with Jesus (like the Eucharist) but physical symbolism as well – all in a book aimed at mass market. If the conservative scholars were to save the souls of the Christian laity, an immediate reaction was in order. Eyer catalogues the furious responses, which include such rhetorical gems as 'a morbid concatenation of fancies' (Skehan 1974), 'an a priori principle of selective credulity' (Achtemeier 1974), and 'in the same niche with Allegro's mushroom fantasies and Eisler's salmagundi' (Danker 1974). One reviewer thought it worth noting that Smith was bald (Fitzmyer 1973). Another held that Smith was being dishonest: instead of the secret gospel mentioned in the title of the popular account the manuscript contained only two short extracts (Gibbs 1974).

Whatever the reasons may have been, the debate certainly took a turn for the worse right at the beginning. It is difficult to picture how a normal discourse could be resumed in an atmosphere

as vitriolic as the above suggests; but things were soon worse still. After Smith had been accused of fantasizing a historical reconstruction of early Christianity, some scholars began to doubt the discovery itself. In 1975 Quentin Quesnell speculated that a scholar, particularly one who shares some of Smith's interests and abilities, might have created a faux early Christian text as a 'controlled experiment', to study 'the question of how scholarly conclusions relate to evidence' (pp. 57-58). Furthermore, Quesnell chastised Smith for producing only 'less than satisfactory' photographs, when the authenticity of the manuscript could be secured only by examining its physical aspects (pp. 48-53). These observations were interpreted as 'insinuations' by Smith (1976: 197), and the relations between the two scholars were strained ever since (Stroumsa 2003: 148). Even though Quesnell did not claim *directly* that Smith had forged the manuscript, many scholars chose to read his article as implying such an accusation, resulting in an ever deepening atmosphere of suspicion surrounding the manuscript (see also Brown 2005: 34-48 for a more thorough examination of Quesnell and his reception). Still in 1995, Eyer had to conclude that even though some have already studied the Secret Gospel of Mark as a genuine piece of early Christian literature, there is a 'comparative dearth of good studies', explainable only by a 'stubborn refusal to deal with information which might challenge deeply-held personal convictions' (p. 119).

In a more recent survey Charles W. Hedrick shares Eyer's sentiments (Hedrick 2003). For Hedrick, the 'firestorm of criticism' Smith received in the 1970s remains a lasting embarrassment to the academy, though one should not forget the many neutral, or even positive, reactions to Smith's discovery. Thirty years of research had not changed the fundamental deficiency in the study of Clement's letter, for the information it provides remains greatly undervalued. Hedrick reproves the guild, asserting that 'simply ignoring them [new discoveries of extant texts, like Secret Mark], or deliberately eliminating them from the discussion, is not a historian's solution'. He also points to the curious fact that a completely hypothetical Gospel (Q) receives continued scholarly attention Secret Mark lacks; a fair enough observation even though the two texts have no real similarities to speak

of (pp. 139-44).

While the situation is not as hopeless as Hedrick paints it – the Secret Gospel of Mark has played an important, constructive part in the study of the Gospel of Mark for such prominent scholars as Helmut Koester (1983) and John Dominic Crossan (1985), and had finally become the sole subject of a PhD dissertation a few years earlier (Brown 1999) – it is arguably true that no adequate *scholarly* reasons can be given to the half-heartedness with which scholars have tackled the questions Secret Mark poses to our understanding of early Christianity. Other reasons, however, are readily found. Eyer's survey shows clearly how the debate got off on the wrong foot, a situation notoriously hard to remedy. Hedrick considers how much a latent homophobia, bearing in mind the one sentence in both 1973 books by Smith in which a 'physical union' between Jesus and the young man is suggested, could be responsible for the tone of the debate. The question remains unanswerable but natural enough (p. 136). In an expanded version of his 1999 dissertation Scott G. Brown suggests that the main source of scholars' reluctance to study this text was the talk of 'suspicion and controversy', which became 'self-fulfilling', a vicious circle wherein the folklore of forgery generated the suspicions that validated the folklore (2005: 39). Who among scholars could bet much on a text of uncertain provenance? Was there not something ambiguous in its discovery, in its discoverer, and in the original reconstruction of Christian origins it was used for? Although Clementine scholars were mostly content in treating the new Clementine letter as authentic, the field of exegetics was divided over the Secret Gospel, some embracing the text (Meyer 2003) while others pronounced extreme indictments of the obvious deception and the fools who had been duped by it (Akenson 2000). Hedrick got it essentially right in the title of his survey. The academy had played itself into a stalemate, or, in the words of Brown, 'most [scholars] frankly have no idea what to make of this text' (p. 19). It would take a whole new perspective to rekindle the debate, and to bring something new to the table for scholars to try and reach a more unanimous decision regarding the use of Clement's Letter to Theodore in the study of Christian origins.

## *The hoax hypothesis*

A whole new chapter on the issue of authenticity was begun in the summer of 2005 when Stephen C. Carlson, then a practising patent attorney, now a PhD candidate at Duke University, made a detailed case against Morton Smith in 'The Gospel Hoax: Morton Smith's Invention of Secret Mark'. Although the accusations of forgery were rehashed time and again during the earlier phase of the debate, Carlson claimed to have found a unique perspective that promised a firm resolution. If Clement's Letter to Theodore did not originate in antiquity, the deception could be uncovered reliably even without access to the actual manuscript. Following the analysis of literary forgeries in Anthony Grafton's 'Forgeries and Critics' (1990), Carlson notes that forgeries are always made to be relevant for their time of origin; otherwise they would go unnoticed. Consequently, as the times change, the underlying subtexts become apparent, perceived as anachronisms by the critics, thus exposing the true authorial time frame.

Another important analytical concept is the distinction between 'forgeries' and 'hoaxes'. The former are the clear-cut cases, intended for deception in an effort to gain monetary or other rewards such as fame and reputation. The latter, in Carlson's mind, is the correct 'genre' for Clement's Letter to Theodore, for the motives for purporting a hoax are usually more ambiguous than with forgeries. A hoaxer may wish to challenge his peers to a game of wits, or she may play the part of a trickster, willing to fool her colleagues by constructing the practical joke of the century. Such a game has a complex emotional dimension involved. Naturally, the hoaxer will want to make people fall for her sham creation, but what is the purpose of fooling everyone if no one will ever get the joke? For such concerns, as Carlson infers, 'it is not uncommon for the hoaxer to plant deliberate mistakes or jokes as clues to the fake's true nature' (p. 16). Supplementing the disclosure of anachronisms and clues and jokes with an ingenious handwriting analysis, and making a case for the classic group of 'means, motive, and opportunity' with Morton Smith as the suspect, Carlson begins a new era in the authenticity debate. His is the most prominent case for the faux Gospel, and deserves a close look



into both its workings and its reception.

In the preface to 'The Gospel Hoax' Carlson relates how he concluded that Clement's Letter to Theodore was not authentic. In an article published in 1995 Andrew H. Criddle examined the word frequencies in Clement's Letter to Theodore, especially *hapax legomena*, words that are used only once in the entire corpus of a writer. Beginning with the assumption that the frequency of *hapax legomena* remains consistent and author-dependent, Criddle concluded that the author of Clement's letter had deliberately chosen Clementine one-words in order to make the text look like it was written by Clement himself. It is curious to note that the starting point of Carlson's inquiry had less to do with the question of forgery – that is, whether the Clementine letter is a forgery per se – and more with the question of the identity of the forger. In other words, according to Carlson, the study of Criddle had already shown that the letter is not from the real Clement of Alexandria and, consequently, the real question pertains only to the identity of the forger. In his own words: 'Was it [Clement's letter] the eighteenth-century idle musings of a bored Greek Orthodox monk or a Dutch humanist... Was Morton Smith a victim of a malicious forgery, or did he himself have something to do with it?' (pp. xv-xvi)

In the end, only Smith is suspect enough to warrant a search for possible means, motives, and opportunities. Carlson argues that he had all of them in abundance. No one doubts Smith's erudition as a historian, his language skills or his keen interest in manuscripts – after all, he did make numerous trips to the Orient as a 'manuscript hunter', as he himself notes (1973b: 8). Carlson maintains that Smith's ability in 'commenting on their [the manuscripts'] inaccuracies in orthography and accentuation' gives him a plausible basis for writing his own, while his expertise regarding Clement of Alexandria is prominent in an article published only a few months before his trip to the monastery of Mar Saba in 1958 (pp. 74-76). The opportunity presented itself during this visit. Smith had in all likelihood prepared the forged text beforehand, and could have easily smuggled it into the monastic library, as library security rarely works to prevent the flow of books

*that way* (pp. 76-78). The possible motives, however, are not that simple. Judging from various jokes Carlson sees all around – Smith dedicating his popular treatise on the Secret Gospel to 'the one who knows', and other subtle hints – he infers that we are dealing with a hoax, with our roguish author having a laugh at our expense. Other possible reasons for forging a Clementine letter include the tenure Smith did not receive from Brown University in 1955, his love of a good controversy, and the amusement many of his colleagues and students claim he received from poking fun at the conservative Establishment with his alternative narratives of Christian origins (pp. 78-86). Such speculations do not constitute enough of a case for deciding an issue with a dead scholar's reputation at stake, but Carlson has much more concrete proofs to present for the jury. Analysis of the handwriting, historical anachronisms and deliberate clues pointing to the forger's identity are the real core of his case.

Applying the principles of QDE (questioned document examination) to the handwriting in Clement's Letter to Theodore Carlson observes various signs of forgery including 'forger's tremor', blunt letter endings, unnecessary pen lifts, and retouching of the letters. These features occur when handwriting is not natural, when the writer *draws* the letters in an effort to get the handwriting to look like it was written by someone else. Furthermore, the existence of tremor and careful retouching should not occur within the same document. Though the first is plausible in situations of stress, old age, or fatigue, careful retouching requires fine control of the writing hand, the lack of which is betrayed by the tremor in the first place. Consequently, Carlson infers that Clement's Letter to Theodore is a drawn imitation of an 18th-century hand (pp. 25-32).

In the process of drawing an imitated hand forgers occasionally lapse into their natural handwriting. A comparison of Clement's letter to the handwriting of Smith from marginal notes identifies the culprit. Carlson argues that the letters lambda, theta and tau are too similar to attribute to coincidence. For example, both handwritings start the letter theta with a short horizontal line beginning from the left, about the height of the middle of the letter. Furthermore, the first of the

hidden clues is closely tied to the handwriting. Carlson claims that Smith penned a hidden confession into another manuscript at Mar Saba, using the same handwriting with which he composed Clement's letter (including forger's tremor and blunt endings), and signing it with a clever pseudonym Μ. Μαδιότης, a non-existent Greek name formed from the verb μαδάω (literally: to bald, figuratively: to swindle). The conclusion is clear. M[orton] Μαδιότης is a pseudonym of Morton Smith, who was, after all, bald, and should he have composed the letter, he was also a swindler (pp. 42-47).

Anachronisms are the tell-tale signs of literary forgeries, and should Clement's letter turn out to be one, authorial blunders are practically guaranteed. Carlson finds a reference to a modern euphemism in the sentence 'And he remained with him that night' (Theod. III.9), which, if re-translated idiomatically as 'spent the night with him', points to sexual intercourse taking place between Jesus and the youth. Furthermore, the love between Jesus and the youth, and his avoidance of the three women (Theod. III.14-16), identifies Jesus as exclusively homosexual. This description, as well as the portrayal of Jesus and the youth as social equals, does not comport with a genuine ancient text, but points to the modern origin of the composition. When the youth in the Secret Gospel is clearly intended to parallel the youth escaping from Gethsemane in Mk 14.51-52, the latter passage must also be read differently. Combined together, the nocturnal arrest of Jesus functions as a deliberate reference to the raids of homosexuals in public parks during the 1950s in the USA, another clue by Morton Smith of the text's modern origin (pp. 66-70).

Yet another anachronism is found in the imagery of salt Clement uses: 'For the true things being mixed with inventions, are falsified, so that, as the saying goes, even the salt loses its savor.' (Theod. I.13-15) Ancient texts do not support the idea of adulterating salt, for it was used in lumps. In order to mix salt with other substances it would have to be *free-flowing*. The invention of free-flowing salt is rather recent: in 1910 a chemist working for the Morton Salt Company came up with the necessary procedure, enabling the company to secure the market for table salt for decades. The

anachronism is not inadvertent, either, but the work of the master hoaxer himself: *Morton* Smith hides a reference to his name within an anachronism about free-flowing salt, developed by *Morton* Salt Company (pp. 59-61). In a similar fashion, Carlson also finds a hidden reference to Smith's last name, and a personal seal of authenticity, forming an ingenious triune confession of the true provenance of Clement's Letter to Theodore (pp. 62-72). Not only does the handwriting bear all the marks of forgery. Not only did Smith have the means, the motive and the opportunity. Not only is the homosexual subtext more pronounced now than during the previous decades. Smith also planted deliberate clues, a trail of breadcrumbs for the academy to follow, should they prove clever enough to earn the uncovering of the hoax. As Carlson concludes, 'Smith's last laugh from the grave is also his last lesson', for 'scholarship is ultimately about truth, not about faith in others', a lesson Smith seemingly taught both in life and death (p. 86).

### ***Reception of Carlson's hoax hypothesis***

The hoax hypothesis was received well. Two scholars who wrote endorsements for Carlson's book were extremely pleased, Mark Goodacre describing it as 'utterly convincing', while Larry W. Hurtado, in a foreword, assessed the case to be 'persuasive, decisive, practically unanswerable' (Carlson 2005: xii). Only one book review, published in a peer-reviewed journal, calls many of Carlson's arguments 'bizarre' (Jay 2008b). The vast majority of reviewers were persuaded by Carlson's overall case, with glowing verdicts: 'Compelling, if not devastating, case...' (Kruger 2006); 'Impressive, almost irresistible, case...' (Marshall 2006); 'Should put to rest the claims of the fragment's authenticity...' (Shiell 2007); 'A very convincing case...' (Webb 2007a). Some reviewers were more convinced of the letter's inauthenticity than of Smith's guilt (Holmes 2006; Chilton 2007), and some thought that the case could never become completely foolproof though Carlson had still done a most impressive job (Tuckett 2007). The opinions were more divided regarding the individual details of Carlson's case.

Scholars were practically unanimous in their acceptance of Carlson's handwriting analysis, with many of them labelling it the strongest of all of his arguments (Foster 2005; Kruger 2006; Tuckett 2007). The anachronisms were likewise accepted by almost all, but a clear division was seen in the scholarly assessment of the clues Smith had allegedly left behind. While half of the reviewers accepted the case for a hoax, that there were jokes and deliberate clues, like the previously mentioned Morton Salt, to be found (Price 2005; Gray 2006; Blomberg 2007), the other half held them, in the words of one reviewer, 'a great deal more subjective and esoteric than the other parts of his book' (Kruger 2006), more questionable than the other arguments presented (Foster 2005; Chilton 2007). It seems that the inauthenticity of Clement's Letter to Theodore has been well established by Carlson, and that the scale of probabilities leans towards Smith as its author. Even those who questioned the alleged clues were comfortable with the handwriting analysis, including the comparison between the letterforms in Clement's letter and the letterforms in Smith's own marginal notes, as one of the most persuasive arguments in linking the putative discoverer of the manuscript to its composition.

### ***The double entendre hypothesis***

Since Carlson's rekindling of the debate, others have strived to expand and supplement his arguments for the faux Gospel in the form of one other monograph by Peter Jeffery (2007a) and two articles by Birger A. Pearson (2008) and Francis Watson (2010). Independently but echoing Carlson Jeffery puts much weight on anachronisms he finds in Clement's letter. If Clement's church utilized the Secret Gospel of Mark in their baptismal liturgy, why does it contain symbolism more closely connected to the Resurrection at Easter, when the baptism of Jesus at the Epiphany (January 6) would be expected in an Alexandrian context? (pp. 55-90) Why does the clearly evident homosexual relationship between Jesus and the young man present the youth as the initiator, combined with the complete rejection of the three women, when both are at odds with Greek

literature on pederasty, the practice of love between a man and an adolescent boy? (pp. 185-212)

The evidence favours Smith as the true author of Clement's Letter to Theodore. Jeffery follows Grafton and observes, as Carlson had done, that even the most ingenious forgery is a product of its time of composition and should be read 'against the background of its proper historical context' (pp. 43-44). The outlook of the Secret Gospel looks very much like the Anglican notions of early Christian baptismal liturgy during the 1950s, 'a kind of spin-off from the Book of Common Prayer' (pp. 71-90), and the compositional procedure of the Gospel extracts, a cut-and-paste procession of canonized gospel material, resembles the way Smith composed his 1973 commentary, bringing together disparate elements from all over the ancient world, with no regard for their proper context or function (pp. 91-122). On the whole, Clement's letter shares features with certain 19th-century gospel parodies, leading to the disclosure of its correct genre. For Jeffery, Clement's Letter to Theodore is an example of 'extended double entendre', a well-known genre in American folklore, in which the author has composed a text with double meanings, pretending to be unaware of the humorous, sexual dimension clearly implied. Reading the first extract of the Secret Gospel as a double entendre reveals that the woman did not simply 'come', but had an orgasm, that she did not simply prostrate before Jesus, but kissed him (during the 1950s the Greek word for prostrating, προσκυνέω, was thought to be etymologically linked to the ritualistic kissing of idols), that the young man did not simply exit the tomb, but came out of a confined space, that is 'a closet', and that Jesus did not simply grasp the youth's 'hand', but his male organ (pp. 91-95).

Furthermore, if we are to read the whole Clementine letter as 'one of the Wildean satires of Christianity', all of the problems in the text disappear (Jeffery 2007b: 6). Particularly relevant to this reading is the play 'Salomé' by Oscar Wilde, for the forgery contains allusions to it, including Salome in the second Gospel extract, whom Smith speculated to have had a sexual encounter with Jesus in the original version of the Secret Gospel, and Salome's 'Dance of the Seven Veils' in the form of 'truth hidden by seven veils' in Smith's translation (Jeffery 2007a: 226-31). These literary

allusions lead to the ultimate conclusion that *the implied reader of Clement's letter is not 'Theodore the orthodox Christian' but an unnamed Carpocratian.*

Reading the letter in this way Jeffery observes how Clement denies the very things that are clearly evident in the Secret Gospel. When he writes that there is no such thing as 'naked man with naked man' (Theod. III.13), the double entendre reading reveals that this is precisely what happens in the Gospel. Clement turns out to be a hypocritical teacher of the church, ready to deny the truth on oath (Theod. II.11-13), even though the libertine tradition of the Carpocratians *is* the real tradition deriving from Jesus himself. The truth behind Clement's 'seven veils' is actually Salome, 'a selfish, vengeful teenager', 'bloodthirsty temptress' and 'homicidal virgin', a necrophile with the severed head of John the Baptist, complete opposite of Jesus the Pure who denied the whole of womanhood (Theod. III.14-16) and chose the love of a young boy – as a Carpocratian would prefer it (pp. 226-31). Such a text cannot be anything but the product of a man who had gone through some sort of psychological crisis, namely, Morton Smith, whose scholarship on the subject, in Jeffery's estimate, amounts to 'hundreds of slovenly pages filled with ignorance, foolishness, and angry jokes' (pp. 180-84, 251).

### ***Reception of Jeffery's double entendre hypothesis***

Even though the conclusions Jeffery reached were much akin to Carlson's, his reception in the academy has been much more ambiguous. Whilst one reviewer found it 'difficult not to be impressed' (Kelley 2009) and another observed that his 'careful work complements that of Carlson, and together they provide an extremely damaging case against Smith and his Secret Mark' (Webb 2007b), others remained unsure (Rousse-Lacordaire 2008) or concluded that 'maybe the pendulum has moved slightly in the negative direction' though the last word remains to be pronounced (Foster 2007). 'Review of Biblical Literature' even saw two of the most singular reviews ever published in academic journals, no small accomplishment considering how over-the-top some of the reviews of

Smith's books were in the 1970s. The highly critical review, all 47 pages of it, judged Jeffery's case to be 'unsubstantiated', full of 'unfounded projections', and concluded that the Secret Gospel he analyzed was actually 'a mental pastiche' he had 'devised... himself in the course of seeking evidence of forgery'. The only prevalent method in the 'madness' of reading Clement's Letter to Theodore as a double entendre was 'a hermeneutics of desperation' (Brown 2007). On the other hand, the highly approving review went so far as to consider the aesthetic appearance of the form of Jeffery's book, including 'fondling' and 'smelling' of the physical product, which was found to be 'lovely' in this and every other respect (Ellens 2009).

Why was Jeffery's hypothesis met with such divergent assessments, considering that his conclusions lined up perfectly with Carlson's well-received case? One reason lies in Jeffery's handling of the character of Smith, including the suggested motives for the presumed forgery. Whereas Carlson talked of Smith in a rather neutral, low-key manner, giving the impression of almost sympathizing with the master hoaxer in his greatest moment of leg-pulling, Jeffery started by praying 'for the late Morton Smith – may God rest his anguished soul' (p. ix), and ended by lamenting 'the tragic paradox of the man' who 'wasn't a good Christian... wasn't even a very good Gnostic' (p. 251). In between, for one example, Smith is portrayed as fantasizing of homosexual rape scenarios in church (pp. 128-30). The three motives that Jeffery infers are likewise unpalatable. First, the Secret Gospel was written in order 'to provide for homosexuality a respectable history, and a literary and spiritual tradition' (p. 239). Second, it was 'meant to be satirical, to ridicule Jesus as a mere pedophile and Christianity as his misbegotten offspring' (p. 206). Third, the deceit of Smith is 'arguably the most grandiose and reticulated "Fuck You" ever perpetrated in the long and vituperative history of scholarship' (p. 242). All of these could certainly function as proper motives if we entertain the suggestion, as Jeffery does, that 'Smith did not know what he was doing', that he began with one objective in his mind, but ended up with another, unsure himself of the message he was trying to get through (p. 243). Jeffery has recently defended his portrayal of Smith by claiming



that he simply 'noted the many curious symptoms' Smith seems to have exhibited (<http://www.music.princeton.edu/~jeffery/smithfaq.html>). Nevertheless, it is hard to see how 'Smith bashing' (Brown 2007) would not be a rather accurate description of the treatment Smith receives at Jeffery's hands.

### *A 20th-century forgery*

The two recent articles follow the model laid down by Carlson and Jeffery. Pearson is of the opinion that the 20th-century origin of Clement's Letter to Theodore 'has been conclusively demonstrated in books by Stephen Carlson and Peter Jeffery' (p. 7). He brings one additional piece to the portrayal of Smith, the lawsuit he threatened Fortress Press with after they had translated and published Per Beskow's Swedish book as 'Strange Tales About Jesus' in 1983, in which Beskow discusses the Secret Gospel of Mark in one chapter (Beskow 1983: 96-103). Smith objected to the treatment of his person and his 1973 commentary, and managed to persuade Fortress Press to pull Beskow's book from circulation; it was later republished with the offending statements removed in 1985 (Pearson 2008: 5-7). Retracting his earlier endorsement of the authenticity of Clement's Letter to Theodore, Pearson concludes that now 'we have the possibility of reading the Secret Gospel of Mark with the author's intentions in mind' (p. 10). This rereading, to give one illustration, reveals that in the second gospel extract 'the youth's sister and Jesus' mother try to arrange for Salome, well known as a loose woman, to meet Jesus. Salome presumably wants to seduce him, but Jesus, who prefers men to women, will have nothing to do with her' (p. 11).

Watson, on the other hand, is closer to Carlson in his assessment of Clement's Letter to Theodore. From the title onwards, he wishes to move the discussion 'beyond suspicion', and establish the inauthenticity of Clement's letter for good. Jeffery's treatise contains 'rather too much of... speculating', for the question of authenticity will only get resolved 'on the basis of the internal evidence of the Clementine letter, read against the double background of the undisputed work of

Clement (and Mark) on the one hand, and Smith's own work on the other' (p. 131). For this reason he also rejects the handwriting analysis altogether. Anachronisms and deliberate clues are, however, both found among Watson's five main arguments. It is hardly to be doubted that the former will find acceptance much more readily than the latter.

Following a suggestion first proposed by Charles E. Murgia in 1975, Watson argues that the real intention of the text (as he understands it) is hardly suitable for a genuine ancient letter. Theodore's discomfort can only be imagined when he learns how large a role the Secret Gospel plays in the church of Alexandria with its references to a 'scantly clad' young man, and, furthermore, Clement is clearly too detailed with his information on the contents and placement of the Gospel extracts. Consequently, Clement's letter is not an appropriate response to an individual, but wishes to disclose the contents of the Secret Gospel, an objective hardly believable in an ancient context (pp. 145-48). The letter also contains curious parallels to the famous statement of Papias, preserved by Eusebius, regarding the origins of the Gospel of Mark. Whilst Papias wrote of 'the things said or done by the Lord', Clement has 'the deeds of the Lord', and both utilize a similar sentence structure of two (Clement) or three (Papias) negative statements followed by a positive one, in the form of 'not... nor... rather' (pp. 148-51).

The embedded clues follow the example of Carlson. An inspirational connection to an evangelical spy thriller from 1940, 'The Mystery of Mar Saba' by James H. Hunter, is suggested on the basis of thematic similarities, and a few parallel sentences that are utilized in a similar context (pp. 161-70). And although Watson is doubtful about the veiled allusions that Carlson proposes to the Morton Salt Company and the surname Smith, Watson nevertheless finds two different allusions to Morton Smith in the very same lines of the text s (Theod. I.13-15). The verb παραχαράσσω refers to the act of forging, specifically to the production of counterfeit coins. The English equivalent, 'forge', was originally used for all kinds of metalwork, but was later reserved for producing inauthentic objects. Nowadays, the common verb for the honorary metallurgist is 'smith'.

Though in itself this fact would not have much significance, it is noteworthy that 'the forgery metaphor occurs not in isolation but in conjunction with a second metaphor, concerned with the corruption of salt' (pp. 152-54). The verb for corruption,  $\mu\omega\rho\alpha\nu\theta\tilde{\eta}\nu\alpha\iota$  in its aorist passive infinitive form, is grammatically correct, but could have been replaced with a finite verb-form, giving a closer connection to the Gospel passage alluded to. Watson notes that it is impossible to be certain of such word-plays, but suggests, nevertheless, that the infinite verb-form was chosen because '[t]hat  $\nu$  is essential if  $\mu\omega\rho[\alpha\nu]\theta\tilde{\eta}\nu[\alpha\iota]$  is to mark the spot where the name 'Morton' lies concealed' (p. 155). It is a fascinating perspective on the mind of the master hoaxer, with one nagging question. When both Carlson and Watson found *different* hidden clues, references to 'Morton Smith' from the same sentence in Clement's Letter to Theodore, surely both of them cannot be right.

### ***A critical remark on the current debate***

Most commentators are happy to treat the respective cases of Carlson, Jeffery, Pearson and Watson as a single, unified argument for the inauthenticity of Clement's Letter to Theodore. There are, however, good reasons for distinguishing between them, as their reception has settled down two parallel lines. Carlson reads Clement's letter as a hoax, giving weight to the handwriting analysis, anachronisms and clues disclosing the true identity of the author. Jeffery prefers to read the letter as an extended double entendre, concentrating on anachronisms, but is not keen on the clues or the handwriting. Pearson echoes Jeffery the most, while Watson brings more clues and anachronisms to the table. Roughly speaking, the two alternative cases are *the hoax hypothesis* (Carlson and Watson) and *the double entendre hypothesis* (Jeffery and Pearson). Scholars have received the latter with less enthusiasm than the former. There is overlap between the cases, and the above distinction is not generally recognized. It will, however, help us to write a coherent history of the scholarly reception of the claims of forgery, as we will see.

How far have these arguments travelled in the scholarly guild? Recent commentaries on the

Gospel of Mark have opted for two basic stances. Either they have adopted the forgery hypothesis as such (Collins 2007), or they withhold their opinion in favour of the on-going scholarly debate (Boring 2006). This trend continues in other treatises and individual articles, with scholars either noting that 'the debate continues' (Meyer 2009: 75) and that '[l]ibre à chacun de se forger sa propre opinion' (everyone is entitled to form their own opinion) (Piovanelli 2006: 254), or, as the majority of secondary literature on Secret Mark has opted for, proclaiming that the case has been settled. To cite a few recent examples, Evans and Tov refer to Carlson as 'convincing evidence that the Clementine letter is a hoax' (2008: 169 n. 33), Wallace states bluntly that the secret Gospel has been shown to be 'an elaborate hoax created by Morton Smith' (2008: 32), and Köstenberger speaks of 'compelling evidence supporting the suspicion that M. Smith created the text as part of a scholarly hoax' (2009: 134). In short, the case for forgery seems to be the prime candidate for becoming a new scholarly paradigm on the Secret Gospel of Mark.

Yet the portrayal of the debate in secondary literature is a curiously one-sided affair. Indeed, judging by many of the summarizing statements one is quite surprised to learn that Smith even *has* his defenders among the academy. Craig S. Keener, to take but one prominent example, lists diligently all the main points Carlson and Jeffery have argued for, and concludes that the 'evidence... now seems to be in on this case' and that the Secret Gospel of Mark is fraudulent (p. 60). For Keener, the criticism of the hoax hypothesis does not exist, or, for reasons that are left unclear, does not deserve a hearing. Even more noteworthy is the practice of non-engagement one encounters in primary literature where the issue of authenticity is extensively discussed. Pearson, for instance, 'cannot see how anyone... could entertain the possibility that the Secret Gospel of Mark plays any role at all in the development of the canonical Gospel of Mark' (p. 9), seemingly unaware that such possibilities were indeed being entertained in numerous peer-reviewed journals. While citing both Carlson and Jeffery as authoritative, Pearson decides to let at least three critical articles by Brown (2006a, 2006b, 2006c) go unmentioned, all of which were published months before

Jeffery's book came out, and one of which he read a draft version of and offered 'helpful suggestions', as is acknowledged in the beginning of that article (Brown 2006a: 291). Watson, on the other hand, does mention Brown, but relegates his engagement with some minor issues to the *footnotes*, pushing Brown's core criticism of Carlson completely aside.

It is telling that one of the more elaborate scholarly assessments of Brown's criticism of the hoax hypothesis has appeared in an article that discusses the Secret Gospel of Mark as part of a wider trend of non-canonized early Christian texts and their popularity. The engagement comes down to this: 'Brown's arguments are shrill and unpersuasive, besides which one must bear in mind that his dissertation on *Secret Mark* (the first ever on the dicey subject) was written on the assumption that it was authentic, and that hence his entire career depends on Stephen Carlson being wrong' (Landry 2009: 376). In other words, one of the most comprehensive discussions of Brown's criticism of the hoax hypothesis is a one-line rebuttal, followed by an *ad hominem*, that we should not ascribe much weight to Brown's arguments since he already wrote a dissertation on Secret Mark! Recently, more favourable responses have been published (Burke 2010), and the parallel debate, where the Secret Gospel of Mark is argued to be an extended double entendre, has seen livelier interaction due to Jeffery, who has continued to offer written responses to some of the objections raised (Jeffery 2007b; 2007c; 2010; 2011). Jeffery's willingness to go on with the debate notwithstanding the current situation in general, recently described as 'trench warfare' by one commentator (Shanks 2010), cannot go on indefinitely, and scholars who have previously stated their support for the claims of forgery have to decide between following Jeffery and answering the criticism, and retracting their support. Despite the manner in which numerous scholars are choosing to portray the debate, a proper survey of the discussion concerning the authenticity of Clement's Letter to Theodore cannot omit the critical response to Carlson, Jeffery, Pearson and Watson. Below I will try and remedy the deficiency. Two specific themes, the analysis of the handwriting and the question of motive, will be explored in more detail, followed by a chronological survey of the

various other writings pertaining to the question of authenticity.

The most prolific author to criticize the claims of forgery has been Scott G. Brown, the author of the first dissertation on the Secret Gospel of Mark (1999) and of the monograph 'Mark's Other Gospel: Rethinking Morton Smith's Controversial Discovery' (2005), in which an argument for Markan authorship of the fragments in Clement's letter has been further developed. The latter was published some six months before Carlson's 'The Gospel Hoax' turned the tables. In late 2005 'The Expository Times' offered both Brown and Carlson a chance to review and respond to each other's monographs. While Carlson challenged the reading of the two extracts from Secret Mark as forming an intercalation with Mk 10.35-45 (2006), Brown analyzed one of the specific claims of Carlson's handwriting analysis (2006c). Subsequent articles by Brown and other scholars have refined and enlarged this line of criticism. The choice of handwriting analysis as a starting point was natural enough: as shown before, many scholars, whether they had been persuaded by Carlson's case or not, were of the opinion that the handwriting analysis was the strongest of his various arguments.

### ***Handwriting in Clement's Letter to Theodore***

According to Morton Smith and the experts he consulted, the manuscript containing Clement's Letter to Theodore derives from the 18th-century (Smith 1973a, 1). As previously mentioned, Carlson contests the consensus and identifies numerous suspicious details in the handwriting: poor line quality which he interprets as 'forger's tremor' and other signs, such as retouching of letters, likewise interpreted as favouring the hoax hypothesis; use of a narrow pen nib and some specific letterforms contrary to the other Mar Saban manuscripts; the similarity between the handwriting in Clement's letter and the handwriting in the manuscript containing the personal name Μ. Μαδιότης, disclosing the author as Smith; the similarity between the handwriting in Clement's letter and Smith's own Greek handwriting. The basis for these conclusions are the

methods used in QDE (Carlson 2005: 23-47).

Following the academic approval, criticism of Carlson's case first concentrated on the handwriting analysis. In his first reply to Carlson, Brown challenged the notion that the same hand responsible for penning out Clement's Letter to Theodore had written the first words on the manuscript containing the name M. Μαδιότης. He observed that the procedures of QDE were not followed to the letter, as Carlson had considered only similarities between the handwritings, but had paid no attention to the differences. According to Brown, the standards of QDE allow identification of two hands to the same author only if no 'fundamental dissimilarities' are present, a point which Carlson did not discuss. A second problem lies in the source material itself, as there is no 'variety of samples' to be compared to nor is the quantity adequate – instead of 'four or five pages of carefully selected continuous, natural writing' the photograph of the manuscript containing M. Μαδιότης shows only sixteen characters in total (Brown 2006c: 145-46).

Brown follows with a counter-attack, as the standards of QDE permit identification of two handwritings to two *different* authors based on a small number of 'significant differences'; and the sixteen legible characters might just suffice for such conclusion. A close comparison between the handwritings yields dozens of differences in the letterforms, in the manner various letter combinations are tied together, in the ligatures and in the accents (pp. 146-48). In the 'curly pi' in Clement's letter, for one example,

'the initial stroke slopes upwards rather than being nearly horizontal, and the point at which the letter terminates is very different... when pi occurs by itself or precedes the letters alpha, epsilon, eta (without accent), lambda, nu, rho, and tau, the stroke loops back over the letter in order to complete the horizontal line and connect with the next letter. Only before eta (with acute accent), iota, omega, and the omicron-epsilon ligature does the stroke connect with the next letter without any loop. So where Madiotes used this latter form of curly pi before an alpha, the writer of the Letter to Theodore invariably used the form with a loop'

(p. 147).

Ultimately, the manuscript containing M. Μαδιότης is an example of 'hand printed (unconnected)' writing while Clement's Letter to Theodore is an example of 'cursive (connected)'. This last observation effectively undermines Carlson's suggestion that M. Μαδιότης acts as a clue to Smith's identity as 'bald swindler'. Why would he not have used the same handwriting (p. 148)? Brown can only conclude that '[t]here is no connection between these two texts to warrant the hypothesis that this name is a clue left behind by Morton Smith' (p. 149).

'Factualizing the Folklore' (2006) repeats Brown's earlier observations and concludes that 'Carlson has simply mistaken two different writers for one' (pp. 293-98). Expanding on his previous treatment Brown notes four subsequent fundamental errors in Carlson's handwriting analysis, namely that Carlson 1) assumed that all the manuscripts in Mar Saba should contain similar traits, 2) neglected one of the key aspects of QDE known as 'natural variation', 3) misapplied the premise that a forger would occasionally 'lapse' into her own handwriting, and most importantly, 4) acted in a 'highly dubious' manner in placing a few letters of Smith side by side with letters from Clement's letter, while focusing his comments only on their similarities. To briefly cite the reasons for these objections, no. 1 is fallacious since the Greek writing styles have been de-standardized since the 12th-century; Mar Saba was a monastery for 'experienced monks' who would have necessarily learned their handwriting all over the Orthodox world; and we cannot know for sure that the manuscript in question was even penned in Mar Saba to begin with (p. 299), while no. 3 is applicable only when imitating someone's handwriting in public – signing a fraudulent check under the eyes of the bank teller, for instance – and not when copying an exemplar prepared beforehand in private (pp. 300-301). 'Natural variation' refers to the range of variation that occurs in the way writers render any letter, a detail which could have some bearing to the handwriting analysis but is not discussed by Carlson (pp. 299-300). The latter ties in with the last objection raised, since people using a common alphabet will inevitably have some similarities in the way they render their letters.



For this reason, a limited sample of questioned writing cannot be ascribed to a particular writer unless both handwritings contain several 'highly individual habits', for otherwise the identification will be impossible (p. 301).

Having already tried his hand at comparing scripts in his first response to Carlson, Brown meticulously assesses Smith's handwriting from numerous sources and compares its letterforms with the ones in Clement's Letter to Theodore (pp. 302-305). Of the letter lambda in Smith's handwriting, for one example,

'55 percent... have a low left leg, 35 percent are formed with the left leg intersecting the middle of the right leg, and 10 percent intersect above the middle. This natural variation in the way Smith wrote *lambda* is not reflected in [Clement's letter], where 96 percent of the *lambdas* have a low left leg. The consistency of this feature in [Clement's letter] eliminates the possibility that we are dealing with a forger's lapse here, as does the fact that a high left leg is too obvious a feature for a forger to overlook so consistently if it appeared in his or her exemplars' (pp. 302-303).

Due to the phenomenon of natural variation, it is not difficult to find the occasional 'match' in the way two writers form a letter, even when they normally form that letter in a conspicuously different way. Since Carlson never mentions 'natural variation' in his discussion or takes this fact into account, the tables in which he places similar letters side by side and states that the scripts mirror each other are effectively worthless; they merely 'create the impression' that there is a remarkable similarity here rather than proving it. When the overall range of variation is taken into account, in the words of Brown, '[i]t is hard to imagine a less compelling paleographical argument tying this manuscript to Morton Smith' (p. 305).

Up to this point the whole debate had been between two people who utilized the methods of QDE but who had no formal education in its intricacies. Such things were soon to change, first with a joint article by Allan J. Pantuck and Brown (2008), and recently by Venetia Anastasopoulou

(2010), a Greek handwriting expert, who was hired by 'Biblical Archaeology Review' to conduct the first professional handwriting analysis on Clement's Letter to Theodore. The main point of Pantuck and Brown's was a photograph of the Mar Saban manuscript Smith had catalogued as number 22 – containing the alleged Madiotes clue – which Pantuck had uncovered from Smith's archive, located at the Jewish Theological Seminary in New York City. The two previously known specimens, published by Smith in order to illustrate the availability of old manuscript leaves at Mar Saba and of the scarcity of paper during the 18th-century, were cropped vertically so far that half of the essential page (folio 1, recto) was missing (p. 111). The complete page reveals information that turns Carlson's efforts to link this manuscript to the question of authorship literally upside down.

In his 1960 catalogue Smith had described MS22, folio 1, recto as containing three personal names, the first one (M. Μαδιότης) from the 20th-century (p. 119). Following Smith's description Carlson had deducted that the first visible hand in the cropped photograph was by Madiotes. His suspicions were raised because of the 18th-century character of the hand; was there a deliberate clue, a fallacious attribution of the hand to the 20th-century, to be had here? The signature of M. Μαδιότης was, however, not visible in the cropped photograph (pp. 42-43). The original from Smith's archive reveals more. Five different hands have written on the page. The signature of M. Μαδιότης is not the first hand, but the second and does not resemble the first hand in the least. Furthermore, Madiotes has written his name upside down compared to the first hand on the page. The source of Carlson's confusion comes down to Smith's description. Since two of the hands do not contain a personal name Smith had omitted them from his discussion. Carlson is probably right in placing the first hand to the 18th-century, but Madiotes certainly belongs to the 20th. Furthermore, the exact spelling of the name is far from clear. Smith had made corrections to the offprint of his catalogue, changing Μαδιότης to Μαδεότας. Pantuck and Brown suspect that the correct form could very well be Μοδέστος, which happens to be a common Greek name. In any case, the suggestion that M. Μαδιότης is a pseudonymous way to refer to the true author of

Clement's Letter to Theodore – the bald swindler or Morton Smith – can hardly be deemed persuasive (Pantuck 2008: 112-23). But the erroneous attribution of the first hand on the Madiotes manuscript to the author of Clement's letter has even more serious repercussions.

If Brown is right arguing that there is no connection between the handwriting in Clement's letter, the first hand and the second hand (Madiotes) in MS22, and the Greek handwriting of Smith, what conclusions should we reach? For Pantuck and Brown, since the first hand of MS22 uses a narrow pen nib but derives undoubtedly from the 18th-century – as Carlson himself concluded – and is not penned by the author of Clement's letter, it is *an independent witness* to the use of narrow pen nib in Mar Saba during the 18th-century, and not an anomaly as Carlson suggested. Furthermore, when Carlson claimed to have found suspicious features from a hand that does not seem to have any connection with Madiotes, Clement's letter, or Smith, what should we conclude of Carlson's general competence in spotting and interpreting these features (p. 124)? Would a professional handwriting analysis of Clement's letter, and a comparison of its script to Smith's Greek handwriting, arrive at a different conclusion?

As part of its special feature on Secret Mark in 2009 'Biblical Archaeology Review' hired a professional Greek handwriting expert, Venetia Anastasopoulou, to conduct an analysis on the handwriting of Clement's letter and to compare it with numerous examples of Smith's own handwriting, including the complete transcription of the letter from 1958. Anastasopoulou who, according to BAR, 'has frequently testified in Greek courts' and holds various degrees on the field, judged the provided material to be 'sufficient in quality and quantity' for a conclusion (p. 7). She assessed Clement's letter to have been written 'spontaneously with an excellent rhythm' (p. 9), indicating 'freedom, spontaneity and artistic flair' (p. 13). Smith's Greek, on the other hand, had 'constrained' movement (p. 15) and gave the overall impression of a 'school student' (p. 18) – noteworthy, since Smith's normal English script was 'spontaneous and unconstrained, with a very good rhythm' (p. 14), illustrating the difference in writing one's mother language and a foreign one.

Anastasopoulou concluded that 'it is highly probable that Morton Smith could not have simulated the document of "Secret Mark"' (p. 38). Her assessment was completely different from Carlson's. Where he spotted 'orthographic errors and anomalous letter forms' and concluded that the 'writer had not fully mastered the style of handwriting' (Carlson 2005: 35), Anastasopoulou saw 'a difficult style of writing' which required 'a lot of practice', observing that '[t]he movement of the writing indicates a hand used to writing in this manner' (Anastasopoulou 2010: 9). Even Jeffery, though he would have preferred Anastasopoulou to have been more thorough in explaining her stance on some of the issues involved, such as the question of 'forger's tremor', concluded in his response that Anastasopoulou's report 'does raise the bar for those who argue that Smith penned the Mar Saba document in his own hand' (2010).

As if Anastasopoulou's analysis had not been enough, 'Stephen Carlson's Questionable Questioned Document Examination' (2010), a joint effort between Brown and Pantuck, dismantled Carlson's original case further, accusing him of quoting selectively from a letter by a professional document examiner Julie C. Edison, whom Carlson had consulted in the course of writing 'The Gospel Hoax'. Carlson had introduced his choice quotes from Edison's letter in his blog *Hypotyposeis* soon after the publication of his treatise, claiming that he had 'hired a professional forensic document expert' in order to 'review' his analysis and produce a 'report' (<http://hypotyposeis.org/weblog/2005/11/some-initial-reviews-and-a-second-opinion.html>). Two ellipses were present in the quotations offered, and the whole of Edison's letter reveals a different story. As Brown and Pantuck summarize their case, scholars

'would have been far less impressed had they known that Carlson's consultant is unable to read Greek, that she met with him for only a few hours, that they looked exclusively at halftone reproductions of Smith's photographs, that she disavows having expressed an opinion on the manuscript's authenticity, and that her positive comments were prefaced by the "most important" observation that the absence of "known standards" in Carlson's

analysis violates one of the “fundamentals” of forgery detection' (Brown and Pantuck 2010).

It would be tempting to proclaim the question of handwriting in Clement's Letter to Theodore done and dusted after these observations (especially considering the further remarks on Anastasopoulou's analysis found in Brown 2011); but some scholars remain unpersuaded. Recently, Agamemnon Tselikas, another Greek expert hired by BAR, has challenged the conclusions of Anastasopoulou, Brown and Pantuck. His article is to be published soon in BAR. The exact details of his coming analysis notwithstanding, an end game for the question of handwriting still seems to be playing itself out, and the debate must be regarded as an open question – at least, until further articles either corroborate or challenge Tselikas' new ideas.

### ***The inconclusive quest for the motive***

Compared to the analysis of handwriting the question of motive represents an area in which clear and compelling arguments are even more difficult to come by. Soon after his 'Expository Times' review of Carlson, Brown discussed the possible motives Smith could have had in creating a faux Gospel, and whether any of the options were as plausible as Carlson had suggested. He chose to evaluate three such scenarios, dubbing them as 'The Gay Gospel Hypothesis', 'The Hoax Hypothesis' and 'The Controlled Experiment Hypothesis', the last proposed by Quesnell in 1975 but never developed further. The lack of interest in the controlled experiment hypothesis is easily fathomed, as such an experiment should have been stopped at some point with results to be discussed in the academy, and, furthermore, the then newly discovered Nag Hammadi codices would have given enough of an opportunity to study the reception of extra-canonical Gospels, without the trouble involved in creating a forged one (Brown 2006b: 380-81).

The other two scenarios are more complex, but have, in Brown's assessment, implausible premises or implications that do not fit into the overall picture of Smith and his scholarship. The gay gospel hypothesis seems to demand that Smith had a grievance with Christianity, that he argued

on the basis of Secret Mark that Jesus was a homosexual, and that Secret Mark actually supports such reading. Brown grants the first premise too easily, even if only to 'save space' (pp. 353-54). Smith had been ordained a priest for the Episcopal Diocese of Maryland in 1946, but obtained an 'indefinite leave of absence' from his clerical duties in 1948. Still, he never left the office but filed a written report to continue his extended leave diligently year after year. Whatever we may want to make of Smith's relationship with organized religion, a 'magic bullet' explanation, like 'hatred he reportedly bore the church for opposing [homosexuality]' (Price 2005), will simply not suffice. One of the more persistent themes in the debate is the denigrating attitude many scholars have towards Smith. Such 'character evidence', however, should have no bearing on the question at hand.

Brown holds fast that Smith never made a serious case for a homosexual Jesus. Remarks of this nature have been proposed over the years, as Eyer first documented in 1995, but these are simply 'bereft of truth' (p. 355). Smith never wrote anything even remotely resembling a 'leader of a gay Judean underground', with 'drag queen' followers and 'an orgiastic rite with overtones of cannibalism', as one scholar chose to portray Smith's contribution to the study of historical Jesus (Allen 1998: 266-67). In all of his published works Smith had only two sentences, one in 'Clement of Alexandria and a Secret Gospel of Mark' (1973a: 251) and one in 'The Secret Gospel' (1973b: 114), in which he speculated of a 'physical union' between Jesus and the disciple, in the course of the initiatory baptism into the mystery of the kingdom of God. While these two sentences may have been intended only as a sensationalist joke (Brown 2006b: 364-65), or Smith may have had legitimate scholarly reasons for his suggestions (p. 358), it is nevertheless clear that Smith understood that the homoerotic reading was not an option for early Christian readers of Secret Mark. In other words, Smith saw that a gay reading of any Gospel pericope does not give adequate grounds for its preservation as part of the Gospel in the first place; the author of Secret Mark must have meant something else (p. 360).

Furthermore, that we should choose to read Jesus' encounter with the youth as implying of

homeroetic relationship, is not an inherent, necessary 'meaning' of the text – and I would be hard pressed to accept that any one 'meaning' should be an inherent quality of any one text. Many scholars, liberal and conservative alike, have found the Secret Gospel of Mark to be 'benign', and every story element in it has numerous verbal and thematic parallels all over the canonized Gospels (pp. 366-70). If Smith did not have an axe to grind with Christianity, if he did not suggest that Jesus of history was gay, and if the text of Secret Mark does not compel us to make such a reading, the gay gospel hypothesis does not present an obvious case for motive for forging Clement's letter.

The variant of the gay gospel hypothesis, reading of Clement's letter as a double entendre as proposed by Jeffery and Pearson, challenges the above criticism regarding Smith's behaviour and the correct interpretation of Secret Mark. Jeffery has remarked that other sentences in Smith's corpus could be read as referring to homosexual Jesus, such as his comparison of the disciples to *daimones* who were 'called to enter the magicians and unite with them' (Smith 1996: 210) – in fact, Jeffery describes a 'pervasive pattern' of sexually-laden material that he finds emerging from Smith's scholarly writings (2007c: 7-11). The question of meaning is more difficult, as a 'proper interpretation' of a text depends on the context in which the reading takes place. In any case, a double entendre has to have some form of plausible deniability; a risqué meaning cannot be too obvious (pp. 11-15). Consequently, the possibility to arrive at 'benign' readings could be evidence only of the lack of imagination of many biblical scholars; or of lack of education, as Jeffery has suggested elsewhere (2007b: 6). Ultimately, if we accept the notion that Smith could not 'perceive clearly what he was actually communicating' (Jeffery 2007a: 243), any question of motive becomes hard to substantiate, for a man in a persistent psychological crisis would not necessarily play by the most conventional rules of the society.

In conclusion, a clear-cut gay gospel hypothesis has problematic premises and, as such, becomes unpersuasive, whilst a double entendre reading is effectively impossible to verify one way or the other. For Brown, the same unverifiable nature pertains to the hoax hypothesis as well. The

likely implications of a hoax would be indifference towards scholarship and towards one's colleagues who would take the forged text seriously. As Brown shows, Smith followed the academic discussion closely, wrote literature reviews and answered his critics (for example in Smith 1982 quoted above), adjusted his overall case based on the criticism, writing, for one example, 'approximately seventy-five addenda and twenty-five corrigenda in the margins of his personal copy of [Clement of Alexandria and a Secret Gospel of Mark]' as well as three pages (over two thousand words) summarizing the "objections" raised in the "important reviews", and corresponded privately with numerous scholars (pp. 375-80). Many have been inclined to ask whether Smith would have done all of the above if he had really composed the text himself. Helmut Koester, in one example of 'character evidence' I would rather see dropped, would not grant the title of 'Great Thespian' to Smith, and cannot bring himself to believe that a colleague and a friend could have fooled him for decades (2009: 58). Despite the efforts to find a plausible motive for purporting the forgery, none have presented themselves with such indisputable proof we would need for deciding the issue on this point alone. As other options operate outside the normal scheme of verifiable/falsifiable claims, the quest for motive will likely remain as inconclusive as it stands today.

### ***Other points of criticism briefly enumerated***

Having dealt with the question of motive to some satisfaction, Brown adopted a methodical approach to Carlson's case, and began a thorough evaluation of it one argument at a time. This approach has occasionally taken him up to the point of being pedantic for the sake of pedantry. In 'Factualizing the Folklore' (2006), for one example, Brown remarks that the 'verb *swindle* implies defrauding people of their money, which, as Carlson notes, is not the objective of a hoax... this supposed meaning of Madiotes is inconsistent with Carlson's hoax hypothesis' (p. 295 n. 15). The erudite style, coupled with the tendency to repeatedly add insult to the injury has had the unfortunate effect of shielding his main contribution, a detailed criticism of Carlson's hoax



hypothesis, from being recognized for what it is worth. In short, as the above discussion of Carlson's handwriting analysis has established, Brown has in this and other areas mounted a devastating dismantling of the main arguments of Carlson, of which chosen details will be discussed below. The question of emotional involvement and its effects will likewise be considered later on.

One of the alleged confessions of Smith was found in Clement's use of salt imagery. For Carlson this functioned as a deliberate reference to *Morton Salt Company*. In Brown's assessment, however, the text fails to voice anything of the sort. The Greek conjunction ὥστε 'indicates that the dependent clause conveys a consequence or result of the independent clause'; in other words, the salt parable is not interested in the factual mechanism of salt losing its savour, but functions as 'a metaphor for the loss of innate goodness and value' (p. 307). Clement states that the true Secret Gospel of Mark, mixed with Carpocratian lies, has lost all of its innate goodness it had in the beginning – an argument apparently useful for Clement's purposes. Furthermore, even if we granted that the mechanism of salt losing its savour does have its place in the questioned sentence, we would not need 'free-flowing table salt' to understand Clement, for salt in antiquity was often impure, and could, in fact, lose its 'sodium chloride (through leaching or disintegration) or acquire an unpleasant taste (through mixture with gypsum)', as every theological dictionary is able to explain in detail (pp. 306-11). As the argument featuring Morton Salt is the key to some of the other 'clues' disclosing Smith as the true author, its critical reception – should Brown's counter case turn out to be solid – lays a shadow of doubt on them as well.

Of the various anachronisms most weight has been given to the homoerotic interpretation of Secret Mark. Carlson claimed, for instance, that the text featured a modern euphemism for sexual intercourse taking place between Jesus and the youth. Brown's response is dry: Carlson has brought the euphemism into existence by rewriting the sentence in question. While 'spent *the* night with him', as Carlson renders the sentence, could certainly do the trick, the Secret Gospel of Mark speaks

of the youth who 'stayed with him *that* night' (ἔμεινε σὺν αὐτῷ τὴν νύκτα ἐκείνην), followed by a description of the nocturnal proceedings: 'for Jesus was teaching him the mystery of the kingdom of God' (Theod. III.9-10). As Brown concludes, '[i]f the sentence... is not an euphemism in Greek, and would imply nothing sexual within the social world represented in this text, then there can be no justification for rendering this sentence as a sexual euphemism' (p. 319). Furthermore, the sentence has a parallel in Jh 1.39, as has long been recognized by scholars (pp. 313-22). Should other details in Clement's Letter to Theodore be considered anachronistic for the real Clement of Alexandria? Such claims have been strongly contested by Brown and Jeff Jay, whose two articles in 2008 concentrated on the Clementine letter surrounding Secret Mark. Jay's epistolary analysis compared Clement's letter to other ancient letters, all of whom are interested in correcting confusing textual variants, which also happens to be one of the explicit goals of the Letter to Theodore (II.19-20). The Clementine letter is argued to conform with its ancient counterparts in matters of form, content and function; in its use of technical terms referring to ancient compositional procedures, for instance (pp. 576-78, 586-96). Brown argued that the form of the Letter to Theodore was as Clementine as could be. In Strom. 3.4.38.2-5, for instance, Clement cites the text in question, repudiates an interpolation his opponents have suggested, and then informs the reader of the correct interpretation of the uninterpolated text (p. 545). As Brown concludes:

'This, in a nutshell, is Clement's strategy for answering heretics who distort the gospels in order to justify impious practices: show (wherever feasible) that scripture predicted their heresy and refute their heretical statements by quoting their proof texts and giving the true interpretation. This two-pronged attack is taken in the Letter to Theodore, which first informs Theodore that the Carpocratians are "the wandering stars" predicted in Jude 13 (1.2-7) and then deconstructs their interpretation of their proof texts' (p. 546).

Other recent works challenging some aspects of the hoax hypothesis still deserve a mention. Guy G. Stroumsa, one of the Western scholars to have seen the manuscript of Clement's letter with

his own eyes (in 1976; Stroumsa 2003: 147-53), edited and published the correspondence between Smith and Gershom Scholem in 2008. Though scholarly reactions are yet to come out in print, I foresee that Stroumsa's interpretation of Smith gaining an insight into the Secret Mark only gradually will be challenged by referring to instances of Smith's correspondence, revealing an interest in Clement of Alexandria as early as in 1948 (p. 28) and to his writing a book on the Gospel of Mark already in 1955 (p. 81; but see also Grafton 2009). Whether these facts carry enough weight to place them into future variants of the claims of forgery remains to be seen. In 2009 Andrew R. Solow and Woollcott K. Smith gave a firmer statistical framework for Criddle's study, which already in 1995 had argued that Clement's Letter to Theodore used words occurring only once in Clementine writings contrary to the real Clement of Alexandria. Solow and Smith verified Criddle's analysis (Herbert A. Simon's model of text generation) and presented it in a more rigorous manner. They were, however, compelled to note that 'the validity of Simon's model is open to question', observing that the source of Clementine vocabulary, Otto Stählin's concordance from 1936, was inaccurate in its information on Clementine one-words (p. 257).

Lastly, 'Biblical Archaeology Review' published four articles on Secret Mark in its November/December 2009 issue, two of which were penned by Shanks, the editor of the magazine. Charles W. Hedrick wrote the introductory piece, narrating the discovery of the manuscript and the debate up to the present. His conclusion echoed his previous assessment from 2003: 'The stalemate with regard to Secret Mark continues.' (2009: 48) Shanks built a rather neutral presentation of the main arguments for forgery, pondering about Smith's capabilities and the alleged flaws and anachronisms in the letter (2009a), only to go and reject all of these in another article that was also penned by him (2009b). Regarding Morton Salt, for instance, Shanks follows Brown in noting that salt could certainly be mixed in antiquity, of which the Mishnah and the Talmud provide numerous examples (2009b: 60). Helmut Koester titled his contribution 'Was Morton Smith a Great Thespian and I a Complete Fool?' and reminisced his long discussions with Smith during the 1960s. If Smith

could have pulled off the stunts of acting as if he 'seriously struggled to understand and interpret this document' he would have had to be 'an accomplished actor and [Koester] a complete fool' (p. 58). Though Koester also gives reasons for considering the Secret Gospel of Mark the best option to make sense of the 'minor agreements' between Matthew and Luke, the 'character evidence' he is bound to give of Smith is yet another indication of the curious nature the authenticity debate brings out of scholars participating in it, as we will see.

Yet it is far from simple to try and write down a coherent presentation of the criticism of the claims of forgery, for as I argued in the beginning of this section, the discussion has settled on two parallel lines, with the hoax hypothesis, originally by Carlson, receiving the most attention. Reading Secret Mark as a double entendre, originally by Jeffery, has stayed in the background, with scholars on both side of the issue judging Jeffery's treatise to have 'rather too much of... speculating' (Pearson 2010: 131) and being 'immensely erudite but largely irrelevant to the question of whether Morton Smith forged the Clement letter' (Shanks 2009: 88 n. 28). Consequently, the debate on this line has taken place solely between Jeffery and Brown, whose 47-paged review of 'The Secret Gospel of Mark Unveiled' (Brown 2007) was met with two responses from Jeffery (2007b; 2007c); furthermore, Jeffery has recently offered some careful thoughts regarding Anastasopoulou's handwriting analysis (2010; 2011). No doubt, considering the amount of criticism the hoax hypothesis has received, the future will see the double entendre reading of Secret Mark gaining more attention, and responses both critical and encouraging will start accumulating.

In conclusion, the criticism of the claims of forgery has been voluminous as well as substantive and even vitriolic on occasion, despite the silence with which the majority of secondary literature and some of the key authors for the forgery have confronted it. If there was a great desire to proclaim the case prematurely close, some scholars are also eager to leave the entangled debate behind and move on to treat the Secret Gospel of Mark as an ancient document, another primary source in the study of early Christianity. In a recent article about beloved disciples in early Christian

Gospels Marvin Meyer takes a bold step and assumes the authenticity of Secret Mark '[f]or the sake of this essay' (2009: 73). Eckhard Rau, likewise, in his assessment of the state of the debate for the German audience and rather harsh criticism of Carlson, decides that as long as the physical manuscript and, consequently, concrete proof of the forgery remains unavailable, there are no adequate reasons *not* to treat Clement's letter as an ancient text (2010b: 166, 186). In an essay published in the same collection as 'Weder gefälscht noch authentisch?' discussed above, Rau goes on to keep his word and reconsiders the function of the young man in the Secret Gospel of Mark in light of other authority figures found in early Christian texts, such as the beloved disciple in the Gospel of John (2010a). These are promising signs of the craving to move onwards, to the next phase of the debate, in which Clement's Letter to Theodore and the Secret Gospel of Mark become, for the first time, part of the normal discourse in the field of biblical studies. Another good omen is the first symposium in the York University Christian Apocrypha Symposium Series, to be held on April 29, 2011 at York University (Vanier College), that is dedicated to the question of forgery of the Secret Gospel of Mark, and features some of the scholars discussed in this paper. The modest goal of the one-day conference is that 'some progress can be made' (<http://www.tonyburke.ca/yorkchristianpocrypha/>), not an altogether impossible endeavour even the discussion below regarding the nature of the current debate considering.

### ***The nature of the most recent debate***

To briefly recap the earlier debate, many reviewers of Smith's 1973 treatises accused him of imagining a fanciful story with no basis in the text itself, and some began to suspect that Smith had forged the Clementine letter. Following the death of Smith in 1991 the abusing language has become almost a standard in dealing with his discovery, with many scholars having a tendency, as Eyer noted, 'to project onto Smith's entire interpretive work an imaginary emphasis on Jesus being a homosexual' (p. 109). It seems that the weight of the earlier debate has carried on into the present,

and that the decisions made in the past have been diligently repeated. The sins of the fathers, so to speak, are the practice of non-engagement, vitriolic language and mischaracterization. Carlson, to take an example from the author who begun the newest phase of the debate, cited Brown's 'Mark's Other Gospel' (2005) occasionally, but chose to ignore most of its contents – Brown was mentioned only three times in the body of the work (pp. 4, 34, 77), and eight times in the footnotes (pp. 106 n. 18 (twice), 113 n. 21, 113 n. 22, 121 n. 35, 124 n. 1, 126 n. 17, 128 n. 1). The omission, though in line with the earlier debate, becomes problematic when we recall that much of Carlson's case rested on the premise that the letter was certainly not Clementine, a conclusion reached by Criddle after a study of word frequencies (Carlson 2005: xv-xvi). Brown had, in fact, criticized Criddle for 'unwarranted generalizations' and 'uncertainties' that seriously undermined, in his opinion, the conclusion Criddle had reached (pp. 54-57). If Brown was right, then the whole enterprise of hunting anachronisms and clues from Clement's Letter to Theodore was in jeopardy – a possibility Carlson could have, at the very least, notified his readers of, even if he did not deem it necessary to challenge Brown directly.

This practice of non-engagement has been the rule in both primary and secondary literature on Secret Mark. Previously, we draw examples of this behaviour from Keener, Pearson and Watson, and noted that for two of them, the criticism of forgery arguments did not exist or did not deserve a hearing, and that for Watson the whole engagement was relegated into the footnotes, and did not concern itself with the main points of criticism. The latter needs to be addressed in more detail here. The footnotes where Watson mentions Brown amount to ten. Four of these contain more detailed discussion of Brown's stance but only on the question of motive (p. 136 n. 25), the question of translating a specific passage in the letter (p. 138 n. 32), the question of whether Smith's views prior to 1958 are congruent with the letter (p. 156 n. 77), and the question of whether Hunter's novel 'Mystery of Mar Saba' is analogous to Smith's discovery story (p. 164 n. 101). The basis for disagreeing with Brown is occasionally an odd one. Consider, for instance, the following: 'Although

Brown rejects this assessment, Smith himself could speak of...' (p. 141 n. 40), which dismisses Brown, in a single sentence, on the grounds that Smith's interpretation was different. In a similar move, Watson rejects Brown's translation of Theod. II.11-12 partly because it 'contrasts with Smith's [translation]' (p. 138 n. 32). There is a certain logic to be found. If Smith did forge the letter, as Watson strongly believes, then his interpretation would have to be given precedence – at least, if we wish to uphold the old notion of *intentio auctoris* being the true meaning of the text. The most pressing criticism Watson does not even acknowledge is the cohesiveness between Clement's letter and other ancient letters interested in correcting textual variants, as analyzed by Jay. In the long run, such selective treatment of arguments is an unsustainable foundation for an academic debate.

Another prevailing detail from the earlier phase of the debate is the vitriolic, abusive language scholars are accustomed to quip each other with. It seems clear that there is a complex interaction between the non-engagement and abusive language, one feeding into the other, for while the proponents of the forgery are more responsible for not dealing with the arguments, the defenders of the authenticity have resorted to the abusive language more readily. The lack of engagement could well be construed to imply that the scholarship not acknowledged is not academically worthy, and as such an insult to the scholar whose arguments remain unmentioned. On the other side of the fence, such indirect approach has not been found to be adequate. More than other with scholars, we can observe from Brown's writings a certain pattern emerging. In the earliest response to Carlson only one sentence need to be considered here. While preparing his first comparison between the handwritings, Brown noted jokingly that he did 'not even own a lab coat', pressing home the fact he did not have formal education in questioned document examination (2006c: 146). Simultaneously, the sentence draws attention to the lack of credentials for Carlson, whose handwriting analysis was done under similar circumstances.

From 'Factualizing the Folklore' (2006) onwards, Brown has hardly ever been content to simply present his case against Carlson, impressive as it is, but has regularly added an insult to the

injury, as previously mentioned. Brown accuses Carlson of having 'conveniently ignored the problem' (p. 292), arguing in a 'highly tendentious' (p. 299 n. 26) and 'highly dubious' (p. 305) manner, and concluding that '[i]t is hard to imagine a less compelling paleographical argument tying this manuscript to Morton Smith.' (p. 305) Furthermore, Carlson is said to practice 'unseasoned exegesis' (p. 308), having an 'highly unorthodox approach' (p. 313) and producing readings that derive from the 'figment of his imagination' (p. 326). On two occasions Carlson is accused of deliberate fraud. First, Carlson's table featuring handwriting from Clement's letter and from Smith's marginal notes 'creates the impression' (p. 300), explained later to be deceptive '[g]iven the wide variation in the way Smith wrote this letterform', for 'it is easy to find the occasional "match"' (p. 305). Second, Carlson's discussion of the function of adding iodine to salt and of the change it brings to the taste is judged to be irrelevant and, consequently, 'simply deceptive' (p. 309).

From Brown's 'The Letter to Theodore' (2008) we find the blunt charge that Hurtado, one of the more enthusiastic supporters of Carlson mentioned above, had failed to do his job as an academician properly (p. 536). Carlson's interpretation of Clement's use of biblical metaphors is found to be 'curiously naive' (p. 566), while the observation that 'Carlson omitted the word *not* (οὐ)... (p. 568)' stops barely short of accusing Carlson of deliberately altering the evidence in his favour. Such accusation would follow in 'Stephen Carlson's Questionable Questioned Document Examination' (2010), where Brown and Pantuck revealed how Carlson had created a false impression by quoting selectively from the letter of Edison, as previously discussed. I do not wish to suggest that Carlson's conduct in this instance would not have been a serious breach of trust and a hit for his own scholarly integrity – which, ironically, reflects well with his own lecture that 'scholarship is ultimately about truth, not about faith in others' (Carlson 2005: 86). But in light of the examples above (considering that dozen others from various scholars could be easily produced) it is easy to fathom why Carlson, as well as many other scholars, having received less than constructive criticism, have found it uncomfortable to participate in the discussion. Thus, the



abusive language will only encourage the practice of non-engagement, and a vicious, unproductive circle of ever more diminishing engagement will keep spinning on.

The third detail that carries itself over from the earlier phase of the debate is the mischaracterization of scholars and of scholarly positions. As many were wont to characterize Smith's interpretation along the lines of 'Jesus the gay magician', the case for forgery has begun to accumulate fantastic elements of its own. For some reason, scholars are prone to exaggerate the circumstances and skills of the leading proponents of the hoax hypothesis. For examples we can cite Evans who transforms Carlson's one-afternoon consult with a qualified handwriting expert into multiple 'experts in the science of the detection of forgeries' who combed the handwriting for signs of spuriousness (Evans 2006: 95; see also Evans and Tov 2008: 169), Watson who gives the faulty impression that Carlson used a substantial body of Smith's Greek handwriting in his analysis instead of the marginal notes of only two pages (Watson 2010: 130 n. 7), Landry who transforms Carlson himself from a patent attorney into an individual with 'forensic expertise and facility' (Landry 2009: 374-75), and Pearson who holds Carlson to possess 'considerable experience in the detection of forgeries' (Pearson 2008:8). But to what 'experience' and 'facility' are they referring to? All of the above statements are embellishments, folklore that has begun to grow and cover the actual argumentation beneath. The function of this folklore is clear enough: it aims to strengthen the foundations of the hoax hypothesis, and, read between the lines, could be even seen as a subtle response to the mounting criticism. On the other hand, the 'character evidence' some scholars are willing to give of Smith – though not necessarily a *mis*characterization per se – tries to scale the balance of probabilities towards the other side, arguing like Koester does that Smith could not have feigned his gradually developing interpretation of the Secret Gospel of Mark (2009: 58). As the meeting of arguments seems already to be a struggle on its own, I cannot but comment how we would do far better if the exaggerations and 'character evidence' were altogether dropped.

All of these problems have had an ill effect on the reception of the case for forgery. As it

now stands, dozens of scholars are already on record as having stated in reviews and articles that the case for forgery is airtight. But has their resolution not been reached at least a decade too early? The ambivalent nature of these quick yet steadfast conclusions is perfectly illustrated by Hurtado, who on the one hand held that the arguments for inauthenticity 'will... be subjected to the judgment of other scholars', while on the other proclaimed these arguments to be 'practically unanswerable' (Carlson 2005, xii). Should we follow the Kuhnian model of scientific revolutions, such abrupt changes would certainly be expected. In the debate at hand, however, the prompt acceptance of the various forgery hypotheses seems to have been exceptionally unfortunate considering the question of handwriting analysis presented above. Scholars were keen to close the case on this one point alone, even though none of them had any knowledge of the mechanics of questioned document examination. Why did they not qualify their endorsements with qualifications like 'if the analysis becomes accepted among the handwriting specialists', 'if the data utilized in the analysis turns out to be representative', or any number of alternative ways not to tie their hands to specific conclusions they are not capable of assessing?

The downplaying of criticism is partly to be held responsible; evidently some scholars remained ignorant that the dissenting voices even existed. Yet, from the beginning, there was also a hunger for a firm resolution to be had, and Carlson was the first to argue wholeheartedly for a case that fit precisely to the perverse suspicion with which many evangelical scholars have viewed all the non-canonized texts. The quick initial reception has, in turn, lead to an even more precarious situation for scholars who may wish to retract their position. Though an ideal portrayal of scientific inquiry presents scholars constantly adapting their views based on data, the reality finds such progress taking place, to quote Max Planck, 'funeral by funeral' only; especially if the scholars in question (like Pearson) had already retracted their position once in response to Carlson and Jeffery. Considering the reluctance with which scholars view the possibility that they might have to drastically change their stance, debating Secret Mark we are not dealing with a mere stalemate as

Hedrick thinks, but a full-versed deadlock. Both sides of the issue find scholars who are essentially defending a position of faith (choosing either to ignore or insult the opposition), entrenched with their particular perspective through which they assess all the data. Unless the debate can alter itself, the authenticity of Clement's Letter to Theodore will end up like the question of Shakespearean authorship, in which a prolonged 'trench warfare' has been part of the debate between the various factions (stradfortians, oxfordians, marlovian and baconian) since the mid-19th century, with no end to the controversy in sight.

The deadlock holding the debate may still be broken. The following suggestions are a good place to start.

- 1) Secondary literature on the Secret Gospel of Mark will need to begin and address the state of the debate as it stands, at present, inconclusive. Specifically, secondary literature has to mention that Clement's Letter to Theodore has not been unanimously deemed inauthentic, nor is the defence of authenticity a group of fringe scholars but members of the academy whose treatises have been published as part of prominent monograph series (Brown 2005) and in peer-reviewed journals (Brown 2006a, 2006b, 2006c, 2008; Jay 2008; Pantuck and Brown 2008). The practice at present, portraying the debate as a one-sided affair where there are only arguments supporting suspicions of foul play, will not do anymore.
- 2) Scholars producing primary literature on the Secret Gospel of Mark will need to begin and address the arguments of scholars with whom they are at odds with. A detailed challenge along this line has already begun in various articles by Brown, but others will have to pick up and try to dismantle his counter case in turn. Furthermore, facts are better off when they are left unembellished and no appeals to 'character evidence' and arguments from emotion are used.
- 3) The vitriolic language should have no place in academic discourse. There are no scholarly reasons whatsoever to use abusive language of one's colleagues or of their scholarship.

Arguments have to be met with arguments, and not with references to the (lack of) mental faculties of scholars who present them.

- 4) It will be beneficial to everyone involved to recognize how this specific debate has never been an exemplary one, and how the sins of the fathers, so to speak, continue to haunt the most recent debate as strong as they have ever been. Such recognition of the extraordinary character of the debate could be one step for scholars to feel more at ease in changing their opinion on the subject, one way or the other. Ultimately, the difficulties in arguing over any scholarly topic are common all over the academy and cannot be solved here. A less oppressive atmosphere will be, however, one step towards a more ideal environment in which to have a discourse.

For the conclusion of this article it is necessary to say a few words of my own affinities with the various parties. Having read selected parts of Carlson's work in the autumn of 2005, right after its publication, I became duly impressed, and placed a tag titled 'recently discovered hoax' under the 'Secret Gospel of Mark' entry, located in my mental index. It took me two years to begin the re-evaluation of my thoughts. In the course of producing an article on Secret Mark (in two parts; 2008a, 2008b) I had to retract my earlier acceptance of the hoax hypothesis. My position at that time, however, was not overly difficult as I was not yet in print with my ideas; only some marginal notes on 'The Gospel Hoax' were proof of my initial thoughts. Having a strong opinion on the question of authenticity – that the arguments suggesting Smith forged Clement's Letter to Theodore are mostly in-between the unpersuasive and the unfathomable; having, in fact, concluded in print that there is no distinction between fringe scholarship produced by the likes of Michael Baigent and Joseph Atwill, and Carlson – has, no doubt, been observable in the manner I have discussed various scholars and their theories above. It is only a wish that wearing a hat labelled 'objectivity' will enable one to practice this most queering of all the scholarly virtues. But as to the above suggestion number two, I would foremost like to see two general questions addressed when the detailed

criticisms of Brown, Pantuck, Jay and others start accumulating:

- 1) Scholars are, to my mind, all too willing to accept the notion that Clement's Letter to Theodore is full of obscure 'hidden clues', illuminating the path to the solution of an ingenious textual puzzle. The old philosophical adage, 'no difference without distinction', is not firmly held here. It is perfectly understandable if biblical scholars are largely unaware of the Shadow Academia, a category under which all sorts pseudoscientific, pseudohistorical and fringe scholarship in the (paranoid) style of conspiracy theorizing is produced. For the lack of any kind of peer-review procedures the Shadow Academia manages to put out far more titles than the old academy; and they are more popular as well! Proponents of the hoax hypothesis should aim to argue why the particular clues Carlson and Watson have unearthed should be taken any more seriously than similar clues by fringe scholars, disclosing true identities of this and that author. Specifically, this would mean differentiating the hoax hypothesis from Barbara Thiering's 'Jesus the Man' (1992), Joseph Atwill's satirical reading of the Gospels, Lena Einhorn's theories that Jesus was also Paul, the various textual clues pointing to someone else as the true author of Shakespeare's works, and even the claims that Paul McCartney died in 1966 and was replaced by a look-alike, a notion that derives from various 'hidden clues' in Beatles' album covers and song lyrics.
- 2) Scholars are, likewise, willing to accept the extended double entendre reading of Jeffery and Pearson too readily. Though it shares the occasional resemblance with another critical style of interpreting texts, queer reading, it has a different, and highly problematic, methodological basis. The proper queering of biblical texts, as practised by numerous gay and lesbian writers since the beginning of the 1990s, aims to reread the familiar passages from a consciously adopted minority (LBGTI) position, in order to provide suitable interpretations for the 'queer community', and to challenge the modern notions of gender and sex interpreters are usually willing to ascribe to the Bible (consult, for example, Take Back

the Word 2000 and Stone 2005). But Jeffery and Pearson 'queer' the Secret Gospel for an altogether straight purpose, namely to argue that a queer reading of the text discloses the modern origin of the composition. As the claim rests on the premise of a dirty-minded professor with a vengeance for Christianity having composed the letter (Jeffery 2007a: 129-30; Pearson 2008: 10-11), it cannot but beg the question, supposing as it does the very thing it is trying to prove; a backwards move towards *intentio auctoris* no genuine queer reading of the Secret Gospel would ever dare to make.

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## **Abbreviations**

BAR Biblical Archaeology Review

CBQ Catholic Biblical Quarterly

HTR Harvard Theological Review

JBL Journal of Biblical Literature

JECS Journal of Early Christian Studies

JSHJ Journal for the Study of the Historical Jesus

JTS Journal of Theological Studies

QDE Questioned document examination

RBL Review of Biblical Literature

Theod. Clement's Letter to Theodore