

Review: Tony Burke (ed.), *Ancient Gospel or Modern Forgery? The Secret Gospel of Mark in Debate*. Proceedings from the 2011 York University Christian Apocrypha Symposium. Cascade Books, 2013. ISBN 978-1-620320186-7.

In 1960 American scholar Morton Smith announced that he had discovered at the monastery of Mar Saba a previously unknown letter "To Theodore" by Clement of Alexandria containing extracts from a "Secret Gospel of Mark". He published the text and translation in 1973. Soon afterwards Quentin Quesnell asked whether the find was in fact a hoax¹. By 1993 vigorous accusations of forgery were being made against Smith by personal enemies², although with no conclusive evidence appearing either way. In 2005 PhD student and former attorney Stephen C. Carlson wrote a careful monograph to show that the text was indeed a hoax, and that the hoax was perpetrated by Morton Smith³. The purpose of the hoax was two-fold; it was partly a joke, but partly to demonstrate his intellectual superiority at a time when he was finding it difficult to find a suitable academic post.

Carlson's work was a definite step forward in terms of methodology, and it convinced most that no further attention need be paid to the subject. Attempts at a rebuttal by Scott G. Brown and others inevitably tended to fall on deaf ears.

In 2011 Tony Burke convened a conference to discuss the state of the question, with contributions mainly but not exclusively from the remaining members of the pro-Smith camp. This volume contains all the papers and addresses from that meeting, together with a transcript of the debate and various other items produced in evidence during the sessions.

The volume is professionally produced, and the indexing and bibliography are of a high standard. The contributions are surprisingly lively and readable, and the whole book may be read in a day without indigestion. One could wish that more "Proceedings" volumes were so accessible.

A ten-page forward by Paul Foster summarising the contents of the volume is followed by an introduction by Tony Burke. Burke gives the background to the story, describes the responses to Carlson's book – including a strange-sounding attempt by Scott G. Brown to force Stephen C. Carlson to respond to his work at another conference – and explains the purpose of the conference. He also gives an account of the conference, and finally indicates the problems he sees with Carlson. As often with such collections, the reader may be perhaps best advised to read these items last, and so approach the contributors directly with an open mind.

A few of the items in the volume may be dealt with quickly. An address by Hershell Shanks, *Was Morton Smith the Bernie Madoff of the Academy* is fun, but not a serious article. It simply asks whether a man of Morton Smith's "sterling character and reputation and scholarly achievement"⁴ would choose to do such a thing to his colleagues. An appendix to this item is *A summary report of Agamemnon Tselikas*, on three pages, which was circulated as a hand-out at the seminar. Tselikas notes that the writing is not continuous, from which he infers that the writer was copying the letter forms rather than writing naturally, and that there are some odd, non-Greek, letter forms. He

¹ Quentin Quesnell, *Mar Saba Clementine: a question of evidence*, *Catholic Biblical Quarterly* 37 (1975), 48-67.

² Jacob Neusner, *Are there really Tannaitic parallels to the Gospels: A refutation of Morton Smith*. *South Florida Studies in the History of Judaism* 80, Scholars Press, 1993.

³ Stephen C. Carlson, *The Gospel Hoax: Morton Smith's Invention of Secret Mark*, Baylor, 2005.

⁴ P.142.

concludes briefly that it was probably written by Smith. The report is too brief and too general to have any real weight compared to Carlson's analysis. Scott G. Brown's paper, *Behind the seven veils: I: The Gnostic life-setting of the Mystic gospel of Mark*, attempts to explain the meaning of a phrase in the letter by analysing the undisputed works of Clement of Alexandria. The analysis seems well done, but it does not bear on the purpose of the conference or the main question before it. *The secret gospel of Mark in debate: a scholarly Q and A* is a transcript of a discussion. While inevitably patchy, it is valuable in that it allows us to see some of the preconceptions of the contributors expressed informally. For instance Scott G. Brown states that he never saw the text as indicating a homosexual element, a statement that will surprise most people; and from Burke's introduction, this denial by Brown is a longstanding one. The debate ends with Peter Jeffrey listing a number of items which scholars do not have; a full statement by a Greek paleographer on the date and location of the handwriting, real or apparent; an agreement by Clement specialists as to how the work does or does not fit into the *corpus* of Clement's works; a serious study of Morton Smith's idiosyncratic theory of magic, which led him to write *Jesus the Magician*; and, finally, "a good, serious, sympathetic biography of Morton Smith". These are indeed all *desiderata*, and those interested in the subject might usefully adopt these hints and work towards providing them.

The volume opens with Charles W. Hedrick's "*Secret Mark*": *Moving on from Stalemate*. Hedrick was tasked to make the case that the text is authentic; a task that he admits is impossible. To demonstrate the authenticity of an ancient text which is under suspicion of forgery is a very difficult thing to achieve, and relies more on creating a general feeling of acceptance than on waving a magic wand of some kind.

Hedrick's approach is to presume authenticity unless it can be shown otherwise, and then to concentrate on summarising rebuttals of those points made against authenticity. This may be the only possible approach, but it is methodologically risky. Everyone knows that it is easier to find fault with something than to establish something. Destruction is always easier. A million objections can be imagined against the most self-evident proposition. It is always very easy to assert that a document is a forgery, and find fault with it. Indeed it is so very much easier to demonstrate inauthenticity that, in the 19th century, a destructive text criticism reached all kinds of conclusions about authentic texts which have not held water. So the anti-"Secret Mark" camp have a built-in advantage; the more objections they make or can imagine, valid or not, the more that belief in the authenticity of the text will wane. Hate-literature of every type relies on the psychology of heaping up "problems", as do libels and, sometimes, investigative journalists.

What the approach taken in Hedrick's paper does is to invert this process. This means that, instead of combatting an endless series of objections, the objectors are placed on the defensive. It is then the pro-"Secret Mark" camp who can instead come up with an endless series of objections to the objectors; and the anti-"Secret Mark" camp who must instead start to establish *their* case. This may be clever, whether conscious or not; but it is unlikely to achieve the desired results.

Stephen C. Carlson's book, *The Gospel Hoax*, achieved its impact by going beyond the endless cycle of vituperation. Instead of "he said, she said", it attempted to step back and come up with something solid and measurable. It was immensely successful for just this reason. Even critics recognised that it was a force to be reckoned with. So it is disappointing that Hedrick here dismisses it much too easily, on the strength of the supposed authority of the work of Allan Pantuck and Scott

G. Brown, but since few will have seen these, and their contents do not appear in the volume, it is impossible to comment on this claim. Few will find this convincing, therefore.

Curiously Hedrick – like Burke – criticises Carlson for not responding to those who have disagreed with him. But Carlson is right to ignore these demands. He wrote a scholarly monograph. He is a young scholar. He is under no obligation to engage in trench-warfare with the partisans of opposing views. “Secret Mark” is doubtless peripheral to his research interests, and he would be imprudent to allow so marginal a subject to continue to engage all his attention at this point in his career. Carlson has found a way to take the debate a step forward. It is for those who disagree to likewise raise their game.

The paper also makes a plea that scholars should not rush to condemn Morton Smith, a fellow scholar, for forgery, without more evidence than “beyond reasonable doubt”. It is not clear quite why persons distinguished only by owning a PhD and a job in a university should be supposed to be specially virtuous in this way.

Hedrick rightly refers to the work of E. J. Goodspeed on *Strange New Gospels* (although not the sequel, *Modern Apocrypha*). Certainly there are things to be learned from this species of literature, although the paper does not really reflect Goodspeed’s approach. There is also an attempt to suggest that the scenario in which “Secret Mark” was discovered is not unlike that of other apocrypha. This is interesting, but reads as if the argument has not fully materialised, and so seems weaker than it might possibly be made. Apocryphal finds are *not* made on the fly-leaves of Latin printed editions, but in papyri or in circumstances where the authenticity of the manuscript is not in question. Hedrick might have referred, however, to the *Apocriticus* of Macarius Magnes, known to us only on the authority of a printed edition, and where the sole manuscript has been seen about as often as that of “Secret Mark”.

Hedrick raises, but does not answer the question of why the text breaks off in mid-page, precisely at the point at which a modern reader might get bored. Anyone who has seen fiction in which mock-sources are quoted will be familiar with the way in which they always quote a trailing sentence, which indicates that the subject is now changing and nothing more need be expected. “Secret Mark” does the same.

The “too Markan to be Markan” argument is explored, but not conclusively. Hedrick rightly points out that identifying a text as “too Markan” is the reverse of how we usually evaluate texts for stylistic failings; but this is not really any argument against it. Then follows an argument that ancient students of rhetoric were trained in imitation, and so the text may be an ancient imitation. Indeed it may; but nothing in the data requires such a hypothesis.

The paper ends with a useful transcript of a conversation with Agamemnon Tselikas, containing various useful snippets of background about the manuscript and its style.

In the end, the paper fails to convince. The task has been undertaken valiantly, but it must fail, because the approach taken is a suspect one.

Bruce Chilton was given the task of responding. His paper *Provenience: a reply to Charles Hedrick* is a very short collection of sensible anecdotes about other finds suspected of forgery. These are

welcome, as likely to shed light on how such items come into being and are uncovered. It does not really address Hedrick's paper, however.

The next paper *Morton Smith and the Secret Gospel of Mark: exploring the grounds for doubt*, by Craig A. Evans, reads rather like an address, containing a compilation of points originally made by others, rather than original research on the points in question. Perhaps Dr Evans was doing what we have all done, which is to concentrate on those points which seem most suggestive. It was good to see the famous anecdote of Coleman-Norton inventing an agraphon of Christ, "Teeth will be provided", and then attempting to get it published (successfully in the end). The reader will find it together with many other similar examples of scholarly jokes and hoaxes in Anthony Grafton's *Forgers and Critics*.⁵ Evans is right to draw a connection, and the overstated accusations of forgery from some quarters must be counter-productive. The mention of the *Mystery of Mar Saba* novel is familiar, but such parallels can never be conclusive. The links with Smith's own research prior to the find are very suggestive, but again can probably be pushed too far. And this does indeed happen (p.89-90) when, in material borrowed from Watson⁶, Evans remarks of one parallel, "The parallel is amazing, both in substance and language." The reader will revolt at such language, redolent of the exaggerated claims of the crank and headbanger. The Atlantis cultists point to the "amazing parallel" of pyramids in Egypt and pyramids in Mexico, and treat this as proof of UFO's in mid-Atlantic; yet the real origin of the parallel is that people pile stuff up and gravity means it forms pyramids. The parallel in Evans' work is too trite for words. Here the anti-"Secret Mark" claims become ridiculous. But Evans' paper is nevertheless an interesting read, if somewhat wandering, and was doubtless interesting to listen to.

The next item in the volume differs from the others, in that it was not delivered at the conference. Instead *Craig Evans and the Secret Gospel of Mark: exploring the grounds for doubt*, by Scott G. Brown and Allen J. Pantuck, was composed afterwards. It consists of a point by point rebuttal of the points made in the Evans paper, with suggestions throughout that Evans has been personally less than honest. This was a mistake by the authors, which will win them few friends and makes their paper extraordinarily hard to read sympathetically. For it is rarely possible to demonstrate that a clever literary forgery is such, within a few decades of its composition, and it will always be a matter of judgement, even of instinct, by scholars, unless scientific analyses are available. It is not dishonest for one man to be unconvinced by an argument that convinces another, and it does very little good for the second man to get angry about this. Better, by far, to prove one's case calmly and rationally, and to leave the supposed moral shortcomings of one's foe to his wife, his confessor, and his personal enemies.

The first couple of pages of the article⁷ are dedicated to demonstrating that a "parallel" between *The Mystery of Mar Saba* and the Coleman-Norton story, described in half a page by Evans,⁸ is not in fact that good a parallel; or, as the authors put it, "Evans's [*sic*] description is mostly erroneous... Evans's [*sic*] erroneous description ... Evans's misrepresentation of Hunter's book" (etc). But the reader who compares the two will feel that this is a mountain out of a molehill. Evans has not set down a

⁵ Anthony Grafton, *Forgers and Critics: Creativity and duplicity in western scholarship*, Princeton University Press (1990). This gives many examples of hoaxes and frauds, often by major scholars.

⁶ Francis Watson, *Beyond Suspicion*, JTS 61 (2010) 128-70

⁷ Pp.102-104.

⁸ Pp.80-81.

solemn and detailed list of parallels; he has offered the suggestion that the general links between the two may have inspired Morton Smith. So they may. Or they may not. Parallels, as remarked earlier, are of limited utility. They tend to prove nothing unless very carefully limited in scope. Evans returned to parallels later, this time with Morton Smith, and with equally little value. But the reader will ask why the article spends the best part of three pages grinding away on something so inconsequential that a reader can barely be troubled to wade through either the claim or rebuttal? Worse is to follow; at the foot of p.104 is a rebuttal of a claim that Evans does not make; that Morton Smith somehow changed his name!

Fortunately the paper then engages⁹ with the real point that Evans made; that Morton Smith's research interests and publications in the lead-up to the discovery of "Secret Mark" reflect the contents of that item. Each element of Evans' argument is examined forensically. Brown and Pantuck discuss the point in detail, complete with claims that Evans said something else at the conference from the published paper. Some of what is said is correct. They argue, rightly, that the linkage between "the mystery of the kingdom" and "forbidden sex" is not nearly as apparent from the full quotation as it is in the abbreviated text that Evans quoted (although it is still there), and this weakens his argument. But they do not seem to realise that this does not advance the pro-"Secret Mark" case at all. They themselves recognise that the arguments they rebut are in fact repeated from others. But in that case, the reader must inevitably ask, why not go to the original and address that? The answer, perhaps, is that both have already done so in their separate monographs; but these the reader will not usually have read.

The paper continues in a similar vein, fault-finding and nit-picking. To nearly everyone this paper will be unreadable, one fears, except to those as interested in what this scholar said, and how he quoted or misquoted that scholar, as themselves. But there seems to be little awareness that the authenticity of "Secret Mark" will never be established by these kinds of arguments, nor the criticisms made debunked thereby. The problem signalled with the approach taken by Charles Hedrick in his paper applies a million-fold to the paper by Brown and Pantuck.

The next paper is from the late Marvin Meyer. Many, perhaps most, posthumous papers tend to be disappointing things. The reader is aware that the writer once did excellent work, but too often finds only a shadow of the former things. Meyer commences by demanding that we defer the discussion of whether "Secret Mark" is authentic – the only question of importance, until it is resolved – and then the remainder of the paper presumes that it is, and engages in some quite purposeless discussion. It also contains a demand that the debate should not be disrespectful of Morton Smith as a fellow academic, and "member of the guild".

Pierluigi Piovanelli has written what is easily the stand-out paper of the volume, *Halfway between Sabbatai Tzevi and Aleister Crowley: Morton Smith's "Own concept of what Jesus 'Must' have been" and, once again, the questions of evidence and motive*. He also manages the longest article title, for which bibliographers will not lightly forgive him. The article is full of interesting snippets, but, while he places himself in the anti-"Secret Mark" camp, he makes no progress on the subject of authenticity. However those weary of the beating of the war-drums will find it an enchanting and interesting read.

⁹ P.105 f.

Piovanelli refers to continental scholars who discuss the question of whether a collection of the letters of Clement of Alexandria actually existed. The phrasing is less than ideal, so it is worth teasing out the argument. John Damascene, in the *Sacra Parallela* attributed to him (7th century A.D.; probably a later compilation), refers to the existence of such a collection through three passages which are supposedly from Clement's letters. John Damascene was thought to have worked at Mar Saba, and thus such a collection could reasonably be found there. But in fact John worked at Jerusalem, although this is not far away. More importantly, if an old collection existed in these locations, and contained the *Letter to Theodore*, why is it unknown to Eusebius of Caesarea, who knows nothing of the details of the early Alexandrian church contained in the latter? The latter argument is somewhat weak, since the failure to use a source is never indicative of ignorance. But the point is an interesting one.

The author also discusses Morton Smith's career in the 50's, in which he spent a year travelling around Greece collecting 10,000 photographs of pages of manuscripts (mainly 10-19th century) of the collected letters of Isidore of Pelusium. This was at the prompting of his mentor, A. D. Nock. How one wishes that he had dedicated his time to editing these letters, most of which are still awaiting a critical edition, rather than the fruitless controversies in which he wasted his life! Smith's letters have been published, which reveal an interesting portrait of a young scholar of a period now almost seventy years ago.

One curious remark made by Piovanelli is worth special attention: he is unable to perceive how publishing "Secret Mark" might be a joke.¹⁰ No doubt this is a cultural thing, since the possibility seems to be universally understood otherwise. But it is a reminder of how parochially *North American* the discussion about "Secret Mark" tends to be.

The quotation of verbatim extracts from Smith's letters gives the reader some real insight into the man and his abilities. One of these may be sufficiently well-put, yet sufficiently misleading, to give the reader pause. Smith writes:

...I have introduced the common-sense observations that (a) *it is more likely than not* that a man's teachings are reflected by the practices of his disciples, and (b) *it is plausible to suppose* that disputes and divisions found almost universally in the earliest attested forms of the movement (the churches known from the Pauline letters) go back to some peculiarity in its origin.

Piovanelli comments, "These kinds of methodological questions have rarely been raised – at least, not in such a direct and uncompromising way – and legitimately should be included in any serious anthology devoted to the research on Jesus of Nazareth."

But in fact Smith has fallen into a misunderstanding. Contemporary experience can supply the key. At modern universities there are often Christian Unions, all with the same confession of faith and biblically based. But often it is found that people introduce new ideas borrowed from the surrounding culture in the period in question. These may be innocuous or malign, but in either case have nothing whatever to do with the confession of faith, nor arise from the bible, although either may be quoted in support. The beliefs and behaviour of those who call themselves the disciples of

¹⁰ P.169.

Christ may reflect the teachings of Christ. But a rudimentary knowledge of church history tells us that they may not, and that the surrounding society is the source of many of the differences. The pressure to conform is strong. Likewise we need not suppose that disputes found in the churches described in Paul's letters go back to Christ in Judaea. They might do so, of course; but it is even more likely that they reflect the cultural norms of the surrounding society in Graeco-Roman cities in Greece, Rome and Asia Minor. Indeed Paul makes just this point himself in his letters to the Corinthians.

In his argosy across Smith's life, Piovannelli discusses Smith's interest in ancient magic, and likewise in modern occultists. In the process Smith began to see Jesus through the lens of the modern Jewish messiah, Sabbatai Tzevi (although surely it is less anachronistic to do the reverse?), as he works towards his *Jesus the Magician*. It is certainly true that scholars have tended to concentrate on the higher forms of ancient literature, and to neglect vulgar, non-classical literature, including the magical texts and the technical literature of antiquity.

Allan J. Pantuck, *A question of ability: what did he know and when did he know it?*, discusses Morton Smith's abilities in an interesting and well-argued way. Did Smith actually possess the necessary skills to compose "Secret Mark", he asks? Pantuck rightly states that the ability to translate into a language not one's own is very much greater than that required to translate. The translator must possess a fluency in the idioms of the destination language. I have myself given up hiring people as translators into English unless they are native speakers of English, after repeated experiences of people who knew their own language well, and could write their own thoughts fluently to me, but were unable to take a set form of words and render it idiomatically in English. However Pantuck goes on to over-estimate the difficulty of composition; this was once part of every schoolboy's set exercises, as even I remember. He then discusses Smith's fluency in Hebrew, on which we are informed from the published letters, as a guide to his likely fluency in Greek in the same period. The approach is sensible, and the conclusion – that Smith's Hebrew was not really fluent even after years of study – is probable enough. Yet the reader will feel that none of this is conclusive either. Smith was, after all, a Greek scholar. The text in question is solidly based on other texts. Glossaries exist. I found myself wondering whether the idea could be tested by setting the job as a student exercise, and assessing the results!

One item in the argument, however, is of first-rate value. This is the testimony of Roy Kotansky, who worked with Smith, and critiqued his translations for Betz's *The Greek magical papyri in translation*. This was nearly thirty years later, but Kotansky testifies that Smith at that time did not apparently possess the Greek skills to compose the text. The only query is whether the somewhat specialised Greek of the magical literature is entirely a fair test of skills quite adequate for an apocryphon?

Pantuck then discusses Smith's paleographical skills, but here he fails to convince. Smith's career in this period, spending a year on the road cataloguing manuscripts, is enough to teach anyone not determined to be ignorant, a very great deal about palaeography and manuscripts. It is doubtless true that in 1950 he knew little enough; but by 1959 he must have learned much. Here one feels the tendency to overstate a case is creeping in. This unwelcome tendency appears on both sides and is doubtless caused by the stalemate. But it must be resisted.

Pantuck then states that "there is no evidence that Smith had any expertise in the highly relevant area of ancient epistolography". Yet we know that he was proposing to do his Th.D. on the letters of

the 5th century writer Isidore of Pelusium. These letters, or more accurately fragments of letters collected together, reach a total of almost two thousand in the more complete manuscripts. They do not have the form of the *Letter to Theodore*, however. Again the argument is overstated; Smith certainly had expertise in ancient epistolography, but it is unclear whether he had enough of the right period and kind for the job.

The paper ends with a great deal of further discussion of the archival materials for the period in which Smith discovered “Secret Mark”, and what he was doing during the pre-publication period. It is interesting and useful, but not conclusive.

Peter Jeffrey contributes *Clement’s mystery and Morton Smith’s magic*. This is an interesting but unsatisfying paper, which strikes the reader as rather diffuse. Jeffrey makes the point that we should not simply compare “Secret Mark” with Mark’s Gospel and the works of Clement of Alexandria; but also with those of Morton Smith. He means that we should look for the characteristics of the latter – he identifies a number of these, including a fondness for double-entendres – and see if these are present in the supposedly ancient text. He accepts that few will be motivated to read that much of Morton Smith. Methodologically he is probably right. But who is likely to invest the effort? Unfortunately Jeffrey finishes his paper in a way that reads a little like a dig at the anti-“Secret Mark” camp.

There are some common themes in many of these papers, which are worth pulling out and discussing by themselves.

A number of papers discuss the character of Morton Smith. Certainly there is value in so doing, because he lived an interesting life in a world that is now in some ways rather distant from most of us. But too much has been made of the kind of argument that runs “Morton Smith was One of Us, so he could not possibly have composed a forgery”. It has attracted (or perhaps was in response to) a corresponding argument of the kind, “Morton Smith was an unhappy weirdo who hated everyone, clearly Not One of Us, and so an obvious candidate to compose a forgery”. Both arguments miss the point, whether the claims are true or not. The scholarly hoax has a long career and has been perpetrated by persons far from disreputable.¹¹ Erasmus himself composed a work which he published as by Cyprian¹². A desire to show one’s own cleverness at the expense of fellow-academics is not an unusual event in the history of scholarship, and it does no-one any favours to treat it as if it was a huge crime, rather than a prank. Here we may observe that some of the anti-“Secret Mark” camp have failed to understand Carlson’s carefully nuanced point, that “Secret Mark” is a hoax, not a forgery, and so have found themselves engaged in unnecessary personal attacks on Morton Smith. Likewise it would be a brave or self-oblivious academic who would wish his integrity to be judged based on whether others find him “weird”! But the attempts by the pro-“Secret Mark” camp to shut down debate by suggesting that members of the academic establishment should not be attacked will not impress the observer either. Smith was certainly an oddball and a hater,

¹¹ See Grafton, *Forgers and Critics*.

¹² *De duplicio martyrio*, in his 1530 edition of the works of Cyprian. See Grafton, p.43-45. “It is preserved in no known manuscript or ancient library. It explicates scriptural passages in peculiar ways, ways also found in Erasmus’ New Testament commentaries. And it is written in a beautiful but peculiar Latin honeycombed with biblical and patristic citations and marked by a frequent use of nouns with diminutive endings – the very Latin in which Erasmus wrote the great literary works that he acknowledged, like *The Praise of Folly*, and the funnier one that he did not, the *Julius Excluded from Heaven*.”

although neither is unusual in academia. Either way, it seems unlikely that any certainty as to the origins of the text will be found from examining the personal life and letters of Morton Smith.

Equally futile are attempts to show that Smith did, or did not, have enough Greek to compose the text. It is really not too difficult to compose a text mainly derived from scriptural allusions, especially when glossaries are available. No doubt there will be mistakes; but today we are using much the same tools to evaluate the text as Smith had, so it is unlikely that we would see them. In the 1950's, indeed, it was far from uncommon for professors to set their students the task of composing a text in the style of one or another classical writer.¹³ Whatever Smith's level of Greek, it was probably quite equal to a task routinely performed by students at that period. Morton Smith himself would perhaps have smiled at the irony that his enemies, the anti-Smith camp, must cry up his learning, while his allies, the pro-Smith camp, must play it down.

So what should we make of the volume as a whole? Where does it leave the "Secret Mark" argument.

When we look at the kinds of arguments made, it is hard not to think of the publications about the authenticity of the Shroud of Turin. Those who remember the days before technology resolved the question will also recall how there were endless explanations of how the Shroud might be authentic. Book upon book detailed ingenious ways in which difficulties might be side-stepped, parallels and connections established, and theories concocted. Arguments for and against were often overstated. But, once carbon dating had demonstrated conclusively that the Shroud was medieval, the process of proving its authenticity did not cease. The promoters of the Shroud industry instead turned to attempts to undermine the proof, to find "problems" with the technology, and so forth. These continue even today, and doubtless continue to make money for the publishers; but they convince no-one other than those who wish to believe for other reasons.

Likewise anyone familiar with the promoters of the crank idea that Jesus never existed – the "Jesus Myth" claim – will find an uncomfortable familiarity in the type of arguments made by some of the pro-"Secret Mark" writers: the deployment of endless minutiae, the reliance on hypothesis, the attempts to disprove what no reasonable person would query, indeed the certainty that the attempts are successful and the creation of a counter-reality where no reasonable person could doubt the claims made, and anger at those who do, and even claims of personal dishonesty. This feels much too like the small world of the crank, of the lunatic fringe. Nor do some writers of the anti-"Secret Mark" camp avoid falling into the same sorts of errors. The debate goes into ever-decreasing circles, where intensity starts to trump evidence.

¹³ For instance a New Zealand Greek scholar of an earlier generation, E. M. Blaiklock in his autobiographical paperback, *The Bible and I: A spiritual autobiography*, London: Marshalls (1983), p.61-2, writes as follows: "In these days when the old disciplines of a classical education are so widely forgotten, it may be difficult to realise that students were once trained to write in the style of Cicero or Demosthenes, and that competent classicists in senior university classes could make fair attempts at rendering a piece of English into the style of, say, Tacitus or Xenophon. I have routinely set such exercises, and provided model translations in more than one style. There is no finer way of developing a linguistic consciousness. It would be no impossible task for a sound Greek scholar to rewrite, shall we say, the first letter of Peter in the simpler style of the first letter of John." No doubt other references could be found to this element in university classical education in the 1950's; this one merely was the first that came to hand.

No truth is established, or destroyed, by the kinds of arguments being used in either case. Heaping up great mountains of doubtful reasoning is not a valid method to evaluate any claim of truth or falsity, but instead a psychological technique to cast doubt in men's minds. There are definite signs, on both sides, that the "Secret Mark" argument is degenerating to these kinds of levels.

Scholarship owes a debt to Tony Burke for organising the conference, and publishing the papers. It does take the debate forward. For, despite some very good efforts, and some interesting points and qualifications, the volume does not succeed in retrieving the cause of the pro-"Secret Mark" camp. In fact it tends to do the opposite. The reader is left with a definite feeling that, if so many and so eminent contributors can find no better arguments than these to revive interest, then "Secret Mark" and "Theodore" are dead. Carlson has triumphed, it seems, and the arguments of his detractors are of the kind that merely reinforce this impression. However this would be too hasty an impression; for a volume of proceedings from a conference cannot be of the same level as a monograph.

It took a step-change in argumentation for the anti-"Secret Mark" argument to gain the ascendancy that it currently enjoys. For the pro-"Secret Mark" agenda to be heard again, it will take a similar step-change in approach and methodology. At the moment, it seems, no scholar need pay any attention to "Secret Mark".

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