

The Consequences of Truth¹

Karin Westin Tikkanen*

Authenticity debates and charges of fraud are not infrequent within the world of archaeology. Depending on the importance of a chosen object, such discussions may reach the level and quality of courtroom rhetoric. Accusations may be hurled, back and forth, through scholarly chambers, and the question of guilt if the object is proven a fraud may be debated, fiercely, with or without considerable proof on either side.

This article discusses the reception of two inscriptions, from late nineteenth century until the current date: the Latin *Fibula Praenestina*, from Praeneste (modern Palestrina), Italy, and the Kensington Rune Stone, from Kensington, Minnesota, USA. The fibula is said to date to the early seventh century BC, and the runic inscription itself mentions the date "1362".

However, shortly after their discovery, both of these inscriptions were accused of being forgeries. Their importance would be significant if they could be proven to be authentic. And yet there is continuing debate about their authenticity and their value to scholarship.

Inscriptions are often regarded as the closest things we have to authentic, first-hand reports from the past. Although remains of writing on perishable materials do exist (Greek and Latin papyri from Egypt, the Latin bark letters from Hadrian's Wall), mostly all remnant manuscripts of ancient texts have suffered numerous re-writings, with misspellings and/or corrections, along with lacunae where the scribe has skipped a word or two, or perhaps even a whole line or paragraph. Inscriptions instead remain perfectly true to their original, in that they *are* the original. They are, to the extent to which they preserve single letters in the order of the moment of incision, veritable "gate-ways" into history.

These "gate-ways" become especially vital when speaking of a "first", meaning the oldest known inscription, in a given language, or of a certain type, or from a particular area. Such inscriptions affect our over-all knowledge concerning the extent to which a given language, or a particular type of inscription, was used, or can provide evidence of the actual spread of a certain population.

But inscriptions can too, of course, lie. An inscription can be fake, they can be cut at a much later time, and at a different place than what is claimed, and these can be evident either directly, by way of the wording in the inscribed text, or by implication, through its archaeological context. If these latter two elements appear contradictory, that is, if the text says one thing but circumstances surrounding the time and place of the find indicate something else, the interpretation and dating of the inscription will have to be evaluated, and following careful consideration of the collected evidence, a verdict of "true" or "false", or real or fake, can be determined.

If the verdict is "true", that is, if the object and its inscriptions are both considered to be authentic, then questions of date, interpretation and contextualization follow and need to be researched and resolved, as with any other an-

cient object. If, on the other hand, the verdict is "false", the questions that follow are usually: who faked them? along with how were they faked? and why were they faked? These comprise the *bona fides* of a real *whodunnit*, and exploring them necessitates considering the character, motif and possibility of any chosen suspects. And, often, the collected evidence might be noncommittal, or unable to support a precise choice between one or another of the options, leaving the verdict unresolved or pending, until more evidence enables a final decision to be made.

The following is a brief survey of the histories of two such alleged, but very much questioned, "firsts" that both appeared at the end of the nineteenth century: the Latin *Fibula Praenestina*, and the Kensington Rune Stone. Although very different, with regard to material, composition, object and language, both are problematic when it comes to their contents and discovery, and both have consequently been questioned. Both inscriptions have been subjected to ruthless examinations, re-examinations, analyses and hearings, and the collected bibliographies of both items would easily fill a small library. The defendants have been persistent, sometimes self-righteous and poignant, the plaintiffs equally adamant and unrelenting. The court and its jury have been gathered, time and time again, and the collective verdict has changed back and forth depending on which side, summoning or respondent, happens to be using the more convincing voice.

The Kensington Rune Stone

In the fall of 1898, Olof Ohman (birth date uncertain, died 1935), a Swedish immigrant from Forsa, Hälsingland, was grubbing trees on his farm in Kensington, Minnesota. His ten year old son, Edward, joined him after school and watched his father's work and was present when, from under one of the tree stumps there appeared a large stone. Ohman at first did not pay it much attention, stones of that kind being a common thing in the area. His son, however, bent down to scrape off some of the mud, unveiling several lines of writing in odd letters. The stone, a rectan-

* Email: Karin.tikkanen@gmail.com

gular slab (79 x 41 x 14 cms) of greywacke, weighing some 90 kilos, was brushed and washed. Friends and neighbours were called in to look at it, and several eye-witnesses would later testify about the white lined marks, under the tree-roots that were entangled around the stone. This strange writing on the stone baffled everyone, since no one present, it was said, knew how to read it, and it was suggested that the markings had been put on the stone by white men, or Indian robbers, who had buried a treasure on the spot (Holand, 1932: 1-2).

The letters, appearing on the front side of the stone, and also along one of its sides, proved to be runes, stating, in what appears to be Old Swedish, that a number of Swedes and Norwegians had come to the place in the year 1362 (edition and translation following Nielsen and Wolter, 2005: 218-220):

**8 göter: ok : 22 : norrmen : po : / ...o:
 oppagelsefardþ : fro : / vinlanþ : of : vest : vi :
 / hafþe : / läger : veþ : 2 : sklar : en : / þags : /
 rise : norr : fro : þeno : sten : / vi : var : ok : fiske
 : en : þagh : äptir : / vi : kom : hem : fan : 10 :
 man : röþe : / af : bloþ : og : þeþ : AVM / fräelse
 : af : illu // här : 10 : mans : ve : havet : at : se
 : / äptir : vore : skip : 14 : þagh : rise : / from :
 þeno : öh : ahr : 1362**

Translated as: "Eight Götalanders and 22 Norwegians ["Northmen"] on this reclaiming/plundering journey far to the west from Vinland. We had a camp by two² one day's journey north from this stone. We were fishing one day. After we came home we found 10 men red from blood and death. Ave Maria, Save from evil." (on front). "There are 10 men by the sea to look after our ships fourteen day's journey from this island. Year 1362." (on side).

The Kensington Rune Stone is thus a rather remarkable "first", conclusive evidence, some say, of pre-Columbian Viking presence in North America. Others take a more critical stance, and question whether a group of thirty Vikings, from Sweden and Norway, could really have come this far inland in Minnesota, in North America, and why they would decide to cut and bury this strange inscription, so unlike, in length and language and writing, any other known runic inscription from Scandinavia. The Kensington Rune Stone is a scholarly dispute at best, but its authenticity also involves hereditary claims, and the unity of an immigrant society.

The Ohman Story

The date on the Kensington Rune Stone itself is indeed baffling. There are several tales of Viking journeys to the west, for example, the most well known one being that of the Swede Leif Eriksson, and his legendary exploits in Vinland. Today there is little doubt that at least some of these tales are based on true stories, although the only determining evidence is a Norse village site that has been excavated in New Foundland, L'Anse aux Meadows (Fr. *L'Anse-aux-Méduses*), dating to around the year 1000. There are, however, no other definite signs that Vikings made it as

far inland, in northern America, as Minnesota. The Ohman farm is surrounded by a swampy marsh that could, considering land elevation, at some point, well have marked a previous coast line. Claims have been made that the man-made holes in rock boulders in the area represent mooring holes for (Viking) ships, but this remains inconclusive (Wahlgren, 1958: 79-80).

There are no documents detailing the find of the runestone itself. Affidavits signed in 1909, by Ohman, his son and two neighbours, state that the stone was discovered in August, 1898. Near-contemporary newspaper articles instead mention the date of the find in November of the same year. The earliest actual recording of the stone and its discovery is a letter written by John P. Hedberg, a Kensington business man, to the Swedish American Journal in Minneapolis, *Svenska Amerikanska Posten*, dated January 1, 1899, several months after the reported find. The first publication of the stone is an article in the weekly publication of the University of Minnesota, *Ariel*, on January 14 of the same year (Blegen, 1968: 19, 32-34). The age of the tree under which the stone was buried is also not certain, since the year rings of the stump were never counted, and numbers mentioned say everything from 12 to 30 or 40, and even 70, years, variable numbers that are highly suspicious of a dubious cover-up story (Wahlgren, 1958: 32-35).

With the publication in *Ariel*, news of the runestone reached Olaus Breda, Professor of Scandinavian languages at the University of Minnesota. Breda received a transcription of the runes, and quickly declared the stone to be a fake, on the basis of finding the language to be "a jumble of Swedish and Norwegian in late grammatical forms with here and there an English word, but all spelled in runic characters" (Blegen, 1968: 22). Similar opinions were expressed by Professor Crume at Northwestern University (in Evanston, Illinois), and also by Professor Bugge at the University of Christiania, in Oslo, Sweden (Wahlgren, 1958: 8-9).

The stone, having been initially rejected as authentic by the scholarly world, was kept at the Ohman farm, where it was apparently used as a stepping-stone by the barn. And there the tale could have ended, dying a natural death, had not, a decade later, Hjalmar R. Holand resuscitated it. A former student at the University of Wisconsin-Madison, Holand had an M.A. in Scandinavian philology and thus a certain interest in, and knowledge of, runes.

In 1907 Holand visited the Ohman farm and promptly acquired the stone. Subsequently the runestone began to receive renewed and greater interest, becoming the focus of Holand's life-long campaign, in articles and publications (in total six books), to prove and defend the authenticity of the Kensington Rune Stone's inscription. The apogee and extent of Holand's success came with the exhibition of the stone at the Smithsonian Institution in Washington, D.C., from February 17, 1948 until February 25, 1949. At the same time however, the Smithsonian Institution as such did not endorse the stone as genuine, and a leading runologist continued to express his doubts about its authenticity (e.g. Jansson, 1949; Moltke, 1953).

Since then the stone's supporters defended and maintained its authenticity, and in the 1990s, once again, the stone received renewed interest. A sizeable amount of positive attention to the stone was aroused by the amateur philologists Richard Nielsen, a consulting engineer, and Scott F. Wolter, geologist, resulting, amongst other things, in a monograph, *The Kensington Rune Stone – Compelling New Evidence* (2005), with strong claims about the authenticity of the inscription and its content.

The Kensington Runes

So what is it, then, about this inscription, that makes it so controversial? In short, almost every single word and rune on the stone has been, and continues to be, discussed.

First of all, there are several words that do not fit into the vocabulary of Old Swedish of the fourteenth century, but that give an impression of being from a much later date. For example, the word *opPagelsefarþ* or “voyage of discovery” (line 2). Modern Sw. *upptäcksfärd* is of fairly recent date, the verb *uppdaga* recorded in the sense of “to discover” only in the beginning of the nineteenth century, from Low German *aufdecken*, which is itself a sixteenth century loan translation from French verb *decouvrir*. The Kensington word, *opPagelse-* (for Sw. *upptäckt* “discovery”), has never been recorded in Swedish sources for any period, and is in every sense a “modern” word (Wahlgren, 1958: 113-114).

Speaking for the defence, Nielsen and Wolter suggest that the thau-rune <þ> may have an additional value /t/ or /th/, thus a word *uptakilse* meaning “take up; ascension”. The complex *optagelsefard* on the Kensington Rune Stone would then mean something like “journey to appropriate new land” (Nielsen and Wolter, 2005: 134). They do not, however, explain the interchange of <g> and <k>, between *opPag-* and their suggested reading *uptak-*. In addition, one is forced to point out that this would be the only instance on the Kensington Rune Stone where the thau-rune would have this additional or alternative value, since in all other instances it is used for /d/.

Other words on the stone, such as *rödhe* “red”, *vore* “our”, *rise* “journey” and *vedh* “by”, are all reminiscent of Norwegian spelling, and, in other words, look more like English, e.g. *ded* “dead” or “death”, *from* “from” (Wahlgren, 1958: 110; Sköld, 2005).

Secondly, there are several forms of words that look very odd. Among other things, Medieval Swedish still had person and number inflections on verbs, but on the Kensington Rune Stone only singular verb forms appear. This is indeed a feature of some Swedish texts from the fourteenth century, although these are very rare. Old Swedish of the kind supposedly used on the Kensington Rune Stone would also have had case inflection, with special forms of nouns after prepositions. The preposition *ve(d)* “by” took the dative case, and thus we expect a form *vid hafinu* for “by the sea”, rather than the “modern” version *ve(d) havet* (Wahlgren, 1958: 112-113).

In addition, the runes used to inscribe the text do not resemble any other runic inscriptions from Scandinavia, from any given period. More compelling evidence are the

dotted vowel signs, for the Swedish vowels <Ä> and <Ö>. These signs were introduced into Swedish orthography only in the eighteenth century, inspired by the German book printing types for the Umlaut sounds. In the Medieval period, the sounds later represented by <Ä> and <Ö> were written <Æ> and <Œ>, as are still used in modern Danish (Wahlgren, 1958: 91-92).

Nielsen and Wolter (2005: 136) cite the double-dotted <Ö> in a Swedish manuscript from 1475, and suggest that its use on the Kensington Rune Stone is consistent with its orthographic use on Gotland in the fourteenth century.

Another possible authentic feature is the presence of the “dotted R” rune (that is, an <R> sign with one or two “dots” added, whether inside the sphere or on the back or front leg). In Sweden, this sign was used, and attested, in two inscriptions from Lund, Skåne, and Ukna Church in Småland, dating to AD 1200 and 1300 respectively, and both discovered in the 1930s. The sign has since been found in a number of inscriptions from Gotland, all from the fourteenth century. In these inscriptions, the rune marks a palatal *r*-sound, i.e. an *r*-sound pronounced with the tip of the tongue on the roof of the mouth. According to Nielsen and Wolter (2005: 41-58), the “dotted R” rune appears on the Kensington Rune Stone in its etymologically correct positions, and is considerable proof that the Kensington Rune Stone inscription could only have been carved during medieval times since this rune was not known in the late nineteenth century.

Holand's line of defence (1932: 96-103), when it comes to the languages of the Kensington Rune Stone, was to suggest that a “mixed” dialect could have arisen spontaneously in a company of Swedes and Norwegians on a long journey, far away from their homeland. This might explain some of the anomalies in the inscription, such as loss of inflections and mixture of vocabulary, although the defenders of the stone are forced to pick and choose among features presented by various Swedish dialects in order to arrive at this unique dialect of the Kensington stone.

There are two alternative sources of information, which must be taken into consideration, one of which is known to have been available in the Ohman household in 1898, whereas the other might have been.

The encyclopaedia *The Well-Informed Schoolmaster* by Carl Rosander (or: *Den kunskapsrike skolmästaren, eller Hufvudgrunderna uti de för eft borgerligt samfundslif nödigaste vetenskaper*), in its third edition of 1882, is documented as being part of the library of the Ohman household. The first part of this book covers the history of the Swedish language, with information on Viking Age runes and tongue, as well as information about Old Swedish and older Swedish dialects. For example, Rosander mentions the variable spelling of the Middle Swedish period (from 1300 to 1523):

“... because of the influence so many different languages had on the as yet unconsolidated Swedish so that at the end of the period one finds one after the other, for example **rike, riike, rige, riche,**

jak, jeg, jac, jach, ok, oc, og, och and so on ...” (Rosander, 1882: 63, translated by Nielsen and Wolter, 2005: 211).

Thus there is a possible explanation for some of the odd spelling on the stone. In addition, some of the odd words in the Kensington inscription are featured, quite specifically, in this encyclopedia, for example, the preposition *äptir* “after” (side, line 2) and the plural form *mans* “men” (side, line 1), in the section of dialected spelling (Rosander, 1882: 62). Both of these words do not at all correspond with what is known of the Swedish, or the Norwegian, for that matter, of the fourteenth century.

In short, almost all of the words *can*, in theory, be explained as genuine in a minute comparison with contemporary fourteenth century Swedish data, whether on inscriptions or on manuscripts (see Holand, 1932: “Appendix B: The Alleged English Words”; Hall, 1994: 19-46; Nielsen and Wolter, 2005: 133-164). However, when the existence of the Rosander book in the family library is taken into account, all these claims become unsustainable. Very little content beyond that of page 64 in Rosander is required to identify all of the anomalies or oddities of the Kensington Rune Stones’ inscriptions.

A second source of information that could have been available to the potential forger comprise the so-called Larsson runes. Carl Edward Larsson (1867-1950) was a tailor journeyman, from Dala-Floda, Dalecarlia. In a letter dated 1885, he explains the different types of “secret scripts” used by travelling wayfarers at the time, in Dalecarlia, but perhaps also used in other parts of Sweden (see Sköld, 2003 and 2005). Larsson’s letter mentions two alphabets, the oldest of which was founded on a general knowledge of authentic runes, although somewhat modified to fit the Swedish of the time. When this became too well known, Larsson writes, a second script was developed, *lönstilen* (“the secret script”), and elaborated with, among other things, a few dotted signs. As it happens, the runes on the Kensington Rune Stone include signs from both of these two Larsson script examples, in a somewhat jumbled order, with signs taken from both of the alphabets, as used by the carver. Only the “dotted R” rune is missing from the Larsson letter, but it is possible that the Larsson runic rows were probably only two versions of this kind of secret alphabet, and display a layman’s knowledge of runic letters, and that different models might have been developed in various parts of the country in the late nineteenth century, for secret or special messages.

The Stone Itself

Carving something onto a sedimentary stone, such as the Kensington stone, causes the exposure of calcite deposits, which later oxidize. This oxidation resembles a white powder. The older the carving, the more the calcite deposits will have oxidized, and the less “white” the cuts on the stone will be. Eyewitnesses at the time of the discovery of the Kensington stone would later testify that Ohman re-chiselled some of the letters, in particular those that had been covered in mud, in order to increase their legibility.

This means that the letters showed signs of fresh calcite oxidation, causing great difficulty for any modern scientist to scientifically determine the relative age of the original acts of incisions or carving.

In 2000, Wolter was employed by the Minnesota Historical Society to conduct a modern analysis of the Kensington Rune Stone. Among other things, he compared the white lineation marks of the tree roots left on the stone, partly still visible, with eyewitness reports, and with drawings made by Ohman shortly after the find. Wolter’s conclusions completely agreed with the original reports, and concluded that the stone must definitely have been buried under the tree for a considerable amount of time (Nielsen and Wolter, 2005: 30).

To determine the relative age of the cutting of the letters themselves, the team made use of two sources of information available to the modern scientist: the analyses of iron oxide deposits developed from the decomposition of the minerals biotite and pyrite; and the weathering of mica grains. Both are features of sedimentary stones, and are usually worn away at surface areas.

Wolter localized the lines of letters on the Kensington stone, on the side, that were *not* re-chiseled in 1898, and compared the level of oxidization and weathering in these with the inscriptions of other stones, in order to arrive at an approximate, relative date for the cutting of the inscription onto the Kensington stone. Two inscriptional sources used in the comparison were of a similar geological type, and also from the area of Kensington, and thus were subjected to the same kind of weathering conditions. The first was the so-called *AVM* stone, an inscription with thirteen carved letters including the three signs <*AVM*>, short for *Ave Virgo Maria*. This was discovered in May 2001 not far from the place where the Kensington Rune Stone had been found, and was initially thought to be connected to the latter. However, later on professors and students from the Indiana University and Southeastern Louisiana University admitted to have chiselled it in 1985, during a course on runes (Williams, 2004: 40). The letters of the *AVM* stone showed rust-coloured halos around actively oxidizing, exposed pyrite crystals in the cut letters, whereas none were observed on the Kensington Rune Stone, where they had all since long weathered away.

The second set of data for the comparison comprised a group of seventeenth century gravestones from the Kensington area. The weathering of the mica grains, a common feature in sedimentary rocks, in the inscriptions of the Kensington tomb stones also proved to be less worn away than in the runic letters. This led the team to the conclusion that the Kensington Rune Stone must be *older* than the seventeenth century (Nielsen and Wolter, 2005: 47).

The Kensington Stone in Its Cultural Context

While the comparative geological data that Wolter analysed were convincing, the problems with the inscription itself, and the incriminating evidence of Ohman’s ownership of the Rosander encyclopaedia, provide evidence for an alternative conclusion about the authenticity of the Kensington Rune Stone.

There are also other kinds of disadvantageous local evidence, such as the rumours among Kensington townspeople and Ohman's neighbours, recorded over time, that hinted at an alleged conspiracy. In 1970 the Gran testimony appeared. Walter Gran was the son of John Gran, a neighbour of Ohman's. On his death bed, Walter Gran described, and it was recorded on tape, that his father at some point had acknowledged being a part of the team that manufactured the stone, along with Ohman himself. This was a very serious threat to the credibility of the stone's authenticity, and Hall (1994: 12-15) and Nielsen and Wolter (2005: 179-186) did their best to discredit the oral testimony on these tapes, by describing it as nothing more than a person's recollection of rumours from many years earlier. Gran himself did not witness the forgery of the stone, they said, and one cannot be sure that Gran's father himself told the truth, because he too, might have made up the story.

At the same time, in light of the eventuality that the stone *could* prove to be a forgery, the defence were keen to distance Ohman himself from any accusations of being the stone's forger, describing him as a simpleton farmer (Holand), or "an intelligent man who worked hard all his life" (Nielsen and Wolter). This was no defence given his possession of the Rosander encyclopedia – Ohman was obviously a well-read man with a great interest in knowledge, and he was never asked to *deny* authorship of the inscription, nor did he ever claim that the inscription was genuine.

So was it then, perhaps as Wahlgren suggests, a family hoax that simply went a bit too far? However, Ohman was in an awkward predicament once Holand had gotten his hands on the stone and began publishing on it, especially if he had to publicly admit to anything about the stone (Wahlgren, 1958: 177-178).

Several aspects about the Kensington Rune Stone – the language, the runes, the unexpected presence of Vikings in inland Minnesota – suggest that the stone is a hoax. If so, it would have been produced in an immigrant environment in a new land, where there was a need for firmer attachment to the land by way of a cultural and historical connection, and perhaps via the appropriation of the soil itself. And it seems that forged "Viking" inscriptions or artefacts are surprisingly common in the North American spectrum (see Wallace, 2003), and the AVM stone is one modern illustration of this phenomenon. The Wolter analysis, on the other hand, could prove that the cutting of the letters is much older than the end of the nineteenth century.

The Fibula Praenestina

Presented to the public in early 1887, eleven years before the Kensington Rune Stone appeared, the Fibula Praenestina, a small gold brooch with a brief inscription in Latin, has suffered a similar fate as its runic cousin, although in a number of different ways.

Unlike the Kensington Rune Stone, which surfaced in a non-archaeological community, the Fibula Praenestina was presented, directly, to a scholarly audience, at the German Archaeological Institute (Deutsches Archäologisches

Institut, *DAI*) in Rome, in January 1887. The main presenter, Wolfgang Helbig (1837-1915), at the time second secretary of the *DAI*, was a recognized archaeologist with a long record of well-received publications. His accomplice was the philologist Ferdinand Dümmler (1859-1896), who was also the first scholar to publish about the language of the inscription.

The Fibula Praenestina is rather small, not longer than 10 cm. Its total weight is only 37 grams. The model is rather plain, much less extravagant than other contemporary fibulae, from other parts of Italy. As with the case of the Kensington Rune Stone however, the Fibula Praenestina immediately received a lot of attention given its inscriptions, and the date attributed to these. The text on the fibula is very short, in total 28 letters divided into four words, reading (in retrograde):

MANIOS MED FHE: FHAKED NUMASIOI ("Manios made me/had me made for Numerius") and in Classical Latin: *Manius me fecit Numerio*.

This is a standard dedication or gift phrase with the object itself "speaking", a common feature used in the archaic Greek world, as well as in early Italy.

According to Helbig the fibula originated from Praeneste (modern Palestrina, about 3.5 km east of Rome), from a tomb described as of a similar type to the Bernardini Tomb, that was excavated in 1876, and comprised a number of objects with strong Etruscan/orientalizing features from the early seventh century BC. Therefore the inscription on the Praenestina Fibula would be a very particular "first": the oldest preserved Latin inscription, and it was, almost immediately, recognized as such. The famous and great and venerable professor of Latin, Theodor Mommsen wrote of the inscription with awe: "*Daß ist wirklich uraltes Latein, und glücklicherweise verständlich*" (cited in Guarducci, 1980: 420).

However, there were just as prompt suspicions about the authenticity of the fibula from Palestrina. As with the Kensington Rune Stone, these suspicions were caused by the language of the inscription, which in this case was judged "too good" to be true. In 1887, Giacomo Lignana (1827-1891), professor of linguistics at the Università di Roma, wrote, in an open letter addressed to Helbig:

"... ed appare in un certo modo come una qualunque combinazione fatta secondo i risultati degli ultimi studii della grammatica storica del latino (... and it looks, in a certain way, like combination of the most recent finds within the study of the historical grammar of Latin).

And from that point onwards, the authenticity of the fibula, just as in the case of the Kensington Rune Stone, has been almost continuously questioned.

"Manios Made Me"

Several aspects in the language of the fibula's inscription are perfectly explicable in terms of Old Latin, in comparison with the Classical language: the rounding of final -os

> -us (*Manios* > *Manius*); the loss of final -d (*mēd* > *mē*); rhotacism of intervocalic -s- and vowel raising in interal syllable (-*ASI*- > -*ERI*-); and monophthongization (-*OI* > -*ō*). Most of these changes can be dated to the third or second centuries BC, and were known in 1887, jointly attested in the so-called *Duenos* inscription (so named after the phrase *DVENOS MED FECED*, “Duenos made me”), dated to around the late sixth or early fifth centuries BC. This was found in 1880 and was, before the discovery of the fibula from Palestrina, the oldest known Latin inscription.

Three aspects of the fibula however stand out as particular, in that they were not, in 1887, found elsewhere in any examples of known Latin epigraphy. All three are incorporated in the verbal form, <*FHE:FHAKED*> (pronounced *fefaked*) “made”. To begin with, the form is a reduplicated perfect. The original system of past tenses found in early Indo-European languages such as Ancient Greek and Sanskrit is that of the perfect (completed act the result of which remains in the present moment), normally a reduplicated form, next to the aorist (a punctual event at some point in the past), which can be formed in several different ways. The Latin and its closest related sibling languages on the Italian peninsula, the Sabellian language group, show an altered, simpler system consisting of a “(resultative) perfect” only. In the process of reducing the original pattern, one of the two available formations was preserved, differently with different verbs. Classical Latin *fēcit* is a long-vowel aorist to the stem **deH₁*- “to do, make”. A reduplicated perfect *fefaked* of this stem is attested in Oscan, a Sabellian language, as *fefacid* (3.sg. perf. subj.) and *fefacust* (3.sg. fut.). Both forms appear on the *Tabula Bantina*, a bronze law tablet discovered and also interpreted in 1791, and interpreted in full, more or less correctly, in 1885.

The hypothetical location of a reduplicated perfect Latin *fefaked*, next to Classical Latin *fēcit*, has been much discussed by philologists, and is considered possible by most, e.g. Meiser (2003), de Simone (2006), and Mancini (2004, 2009). An overlapping of Latin *fefaked* with *fēcit* of the Classical language could be a remnant of the old pattern where both tense stems were still available, before the final reduction of the system to its Classical shape.

Secondly, the orthographic habit of using double points <:> between words is also known from other inscriptions. Although the use of a triple point <::> between the reduplication syllable and the stem in a perfect form was unknown at the time of the presentation of the fibula. In 1889 a Faliscan inscription was discovered (a close neighbour language to early Latin), with the same use of the triple point in a perfect form, *PE:PARAI* (cf. Latin *peperī*). However, this was interpreted only several years later, in 1908.

Lastly, the form *FHEFHAKED* features a digraph <FH> (that is, a combination of the digamma, <ƒ>, and the letter for aspiration <H> for the /f/-sound. In 1887, this digraph was known in inscriptions from the Etruscan and Venetic speaking areas in the north of Italy, as in the Etruscan name *FHELEQU*, and the verbal form Ven. *FHAGSTO*

“made” (cf. Latin *faxit*). In both instances, the use of the digraph for the /f/-sound was interpreted in 1888, after the presentation of the fibula.

All in all, many of the features of the language of the fibula from Palestrina were known in 1887. The *Duenos* inscription incorporated several of the archaic aspects of the older Latin language, and may have served as the point of origin for a potential forger, particularly the phrase *DVENOS MED FECED* “Duenos made me”. Note that the first two words comply completely with the formula on the fibula, *MANIOS MED*. When manufacturing a “first”, the forger would however want something extra that would out-do the *Duenos* inscription, and the choice could have fallen on the verb.

Of the three features already mentioned, the reduplicated perfect is paralleled by Oscan forms. For someone with some insight into the Italic spectrum, this might have provided potential input for the manufacturing of an “Old Latin” verbal form. The remaining two elements, the triple point after the reduplication syllable and the use of the digraph <FH> for /f/, were *not* known in 1887. The supporters of the fibula find the later date of the reading of Fal. *PE:PARAI*, and the Etruscan and Venetic use of the digraph, verifications of the fibula’s authenticity. The opposite view has it that both features might have been cleverly designed (see below) by someone well versed in contemporary philological discussion. Anticipating all publications in the direction of Faliscan, Etruscan and Venetic, the forger managed to get his inscription “authenticated”.

The Pedigree Problem

As specified by Helbig’s first report, the fibula originated from excavations conducted at the seventh century BC settlement at Praeneste. However, there is no scientific documentation concerning either the origin, or the discovery of the fibula, but only secondary reports.

According to Helbig, the fibula was found in 1871, in a tomb in Praeneste “of the same type as the Bernardini Tomb”, where a “friend” had found it and given it to him (Helbig, 1887, cited in Gordon, 1975: 2). In 1871 there were no on-going excavations at Praeneste. Two large tombs had been excavated in 1855 and 1861, and the Bernardini Tomb, to which the fibula has been attributed, was excavated in 1876. If 1871 is the correct date for the finding of the fibula this means that it was extracted in secret, and also kept secret until 1887 when Helbig presented it. Was it perhaps kept secret to disguise the fact that it was stolen? and was it appropriated later on by someone who did not want his name mentioned? and was the original find date really 1871? Was this too invented so as to disassociate it from the Bernardini Tomb? And was Helbig unaware of the whole truth?

A few years later, in 1900, Georg Karo, a student of Helbig’s, told a different story to Luigi Pigorini, founder and also director of the Museo Nazionale Preistorico Etnografico (which today bears his name). Pigorini was keen to have Karo provide a reliable Praenestine pedigree for the fibula, so that it could be placed in his museum. Karo wrote that

the fibula originated from the *Tomba del Tesoro* (which he later implied would be the Bernardini Tomb) at Praeneste, and that it had been stolen by the *caporale*, the excavation foreman, who sold it to an antique dealer, Francesco Martinetti (1833-1895), the “friend” who later showed the item to Helbig. Karo also wrote that Helbig had wanted to keep the whole thing secret since it involved pillaging and stealing, but since everyone that had been involved were now dead, it was deemed safe to tell the truth (Gordon, 1978: 34-35).

However, more and particular allusions to forgery were made. Giovanni Pinza (1872-1940), a palaeo-ethnologist by trade, wrote a number of critical articles and mentioned, in several forewords to publications, his doubts concerning the authenticity of the fibula (Gordon, 1975: 5-10, 1978: 35-36). Towards the end of his life he became even more specific, and he said, in a 1932 interview, that the famous Roman goldsmith Augusto Castellani had told him (Pinza) specifically not to bother with the fibula since it was a fake. The name of the manufacturer of the fake was communicated to Pinza, who did not pass it on.

At around the same time Karo questioned the story of the origin of the fibula. In a review in 1925, ten years after Helbig's death, Karo wrote that the doubts that the fibula had actually originated from the Bernardini Tomb, were not wholly without substance, and that Helbig's statement about this was “... *selbstverständlich kein Beweis* ...” (Karo, 1925, cited in Gordon, 1975: 11).

In spite of all of these contradictions, for the major part of the AD twentieth century the fibula was regarded as more or less authentic, perhaps because this was easier than proving it to be a forgery. Most scholars considered the question of fibula's authenticity as an unsolvable problem, and the matter was never gone into in any great detail.

The Second Wave of Interest

In the 1970s, American philologist Arthur E. Gordon studied the problem of the Fibula Praenestina's pedigree, investigating the various stories told about it, and, in particular, Pinza's version. Gordon (1975) found most aspects of the language of the inscription convincing, in particular the triple point and the digraph, and wondered whether Pinza's constant attacks on Helbig's reputation, and on the authenticity of the fibula, were expressions of mental instability, partly caused by museum director Pigorini's enmity towards him. Among other things Gordon (1975: 10) quoted Pinza, in a preface from 1923, that recorded his strained relationship with museums and excavations, and explained that he was not as strong as he used to be after having done his war service.

This was, Gordon himself admits, a roundabout explanation only, which cannot be considered scientific proof of the fibula's authenticity. All the same, Gordon was supported by several scholars, most of them considering the digraph <FH> the singular strongest piece of evidence in support of the fibula's authenticity. There was also the simple fact that the fibula was donated to the Italian state, to the Museo Etrusco di Villa Giulia, after its foundation in February 1889. This, Italian philologist Colonna writes

(1979: 123), is certain evidence of the fibula's authenticity, since Helbig would not have run the risk of discrediting his name and reputation by donating a fake artefact to such an important institution.

The Facts of a Fake?

In the late 1970s, the Italian archaeologist Margherita Guarducci began her own study of the fibula. She undertook a detailed scientific study of the object itself, and enlisted the help of a number of scholars from various backgrounds – art historians, experts on restoration techniques, graphologists etc.

Guarducci's expert team detailed several problems with the fibula itself, some visible to the naked eye: the fibula was unique when it came to its shape, and its gold was not as fragile and as brittle as one would expect of a gold item more than 2,500 years old. An analysis of the microstructure of the artefact revealed that the gold granules did not fit with the structure of an ancient artefact, and when examined through a microscope, the artefact's surface proved not to be worn in the way that an object that has been buried in the ground and then dug out, tends to be worn. Instead, there were traces of the use of some kind of abrasive acid, applied both *before* and *after* the cutting of the inscription's letters, indicative of a late nineteenth century attempt at forgery that was used to make the artefact look much older (Guarducci, 1980: 469-470). And, if the fibula was a certain forgery, then the inscription was also bound to be a modern concoction, albeit a very intelligent one.

Guarducci (1980: 456) suggests that the reduplicated perfect must be the mere copying of the formula seen in the Oscan forms, and suggests that the triple point is an attempt on behalf of the forger at “Etruscanization”. As for the digraph <FH> for the /f/-sound, her explanation is a bit more cumbersome. To start with, in the *Duenos* inscription the letter <F> marks the /f/-sound, and <V> stands for both vocalic /u/ and the semi-vowel /w/, as in the Classical language. A forger, wanting to convince an expert that it was an “older” text would want something different. At least two Roman grammarians, Priscianus and L. Annaeus Cornutus, attest to an earlier use of <F> for /w/, saying that the letter <V> was only later adopted for the consonantal sound in addition to marking a vowel (Guarducci, 1980: 457-458). A late nineteenth century forger could thus conclude that the scribes of early Latin must have written the sound /f/ differently. Also, Emil Szanto, in an article from 1890 (cited in Guarducci, 1980: 458-459), analysed the Greek dialectal habit of combining signs for aspirated stops with <H> as a way of reinforcing the aspiration. If this was a recurring point of discussion among philologists during the 1880s, it would not be going too far to ascribe to the forger of the fibula, the use of the idea that an ancient writer might well have combined the digamma with <H> for the f-sound.

The Tale of a Forger?

Having thus established that both inscription and object are most likely modern constructions, Guarducci set out

to find the culprit. And the prime suspect was ever so easy to ascertain: where the Kensington Rune Stone was most likely cut by its finder, Ohman himself, it is similarly simple to identify Helbig, the fibula's finder, as conspiring to manufacture the fibula.

And it seems that Helbig might not have been the renowned and honest scholar he was later made out to be. At the beginning of his career, Helbig's teachers and mentors were not enthusiastic about his potential. Among other such comments, Helbig's teacher in Bonn, Otto Jahn, was very reluctant to nominate him for a Rome travel grant, writing in 1861: "*Nach meiner Kenntnis kann man ihm nichts anvertrauen, wozu Selbstständigkeit, Ernst und Sorgfalt erforderlich ist ...*" (cited in Guarducci, 1980: 487-488).

Despite this, in October 1862 Helbig arrived in Rome on a scholarship, and eventually, more than twenty years later, he had risen to a position of power in Roman archaeological circles, becoming the second secretary of the *DAI*. This had occurred shortly after the fibula's presentation to the public in 1887, when the first secretary of the *DAI*, Henzen, died, and Helbig was promoted in his place. As it happened Helbig did not last long in this position, since the authorities in Berlin were disappointed with his achievements and requested him to resign. In October of that same year he left the *DAI*, and spent the rest of his life in retirement. Through his marriage to a Russian princess, Nadejda (Nadina) Schakowskoy, Helbig became a wealthy man and he did not suffer economically from this retirement.

As a scholar and an archaeologist, Helbig had also developed a business "on the side", tracing antiquities on behalf of foreign interests, and buying them from excavations. And it is here that a Mr Martinetti, a Roman art dealer, who worked with Helbig for many years, becomes an interesting part of the fibula story. Martinetti also worked as a discreet supplier of high-class antiquities to foreign buyers. Between them Helbig and Martinetti bought, sold, passed on, dealt with and handled, a large number of artefacts. Among those artefacts that passed through Martinetti's workshop, at least two were later revealed to be forgeries: a Praeneste coffin, itself authentic, although with a modern incised decoration, sold to the British Museum in London (1864); and the so-called "Boston Throne", reportedly from the Ludovisi Collection and sold to the Boston Museum of Fine Art (in 1894) (Guarducci, 1980: 481-486). Helbig was involved in the sale of both of these objects, and in fact, has been associated with a third forgery. In 1895, after Martinetti's death, he was involved in selling a false *Diadumenos* to Carl Jacobsen's Glyptotek in Copenhagen (Guarducci, 1980: 509-525).

For two men with such pasts, and connections, faking an object such as the Praenestina Fibula would not have been very difficult. Martinetti would have had the contacts necessary for the production of an ancient-looking object, and Helbig, an established philologist, and would have had access to current discussions about possible new interpretations of old inscriptions that were soon to be published. He possessed all of the knowledge necessary

to manufacture the inscription on the Praenestina Fibula, one that incorporated both known and, at the time, "unknown" linguistic archaisms and features.

Guarducci (1980: 539), concludes her monograph on the Praenestina Fibula with the laconic words: "*Cade, cosi, if mito della Fibula Praenestina, dopo aver ingannato per quasi un secolo studiosi di tutto il mondo e provoacto agli studi numerost danni*" "thus falls the myth of the Fibula Praenestina ...".

Third Time Lucky

Guarducci's monograph was avidly received, and there were many sighs of relief that the century long mystery had indeed finally been solved. In the words of Ridgway (1981):

"Miss Guarducci has cleared the air. Few others could have bridged the fatal gap between archaeology and epigraphy so authoritatively – certainly not Sherlock Holmes, whose only recorded excursion into antiquity concerned the Chaldaean affinities of the ancient Cornish language. Which, like Manios and his fibula, may now be safely discarded."

Like a villain in a Conan Doyle/Sherlock Holmes story, Helbig fits the description of the felon and perpetrator perfectly: possessing the expertise to commit the forgery, with the connections and position of power and respect to authenticate it, and also the motif: to advance within his scholarly field, more specifically, to advance within the *DAI*. Gordon (1982: 67) joined in, describing Helbig, a recognized scholar and at the same time behind such an excellent hoax, as having the character of a kind of "Stevenson's Dr. Jekyll and Mr. Hyde".

However, not all scholars found Guarducci's arguments and conclusions convincing. Hans Krummrey, director of the *Corpus Inscriptionum Latinarum (CIL)*, wondered (1982: 589), if it was possible to attribute this supposed misdemeanour to Helbig, if it was simply based on the fact that he had, reportedly, committed other crimes? Can we be absolutely certain that Helbig knew of, and was compliant about, the forgeries? Or was he duped by his friendship with Martinetti? So much so that he did not question the provenance of the items they sold? Throughout the affair with the false *Diadumenos*, Helbig maintained, when asked by Jacobsen, that he considered it to be authentic (Moltesen, 1981).

German legal historian, Franz Wieacker argues that the evidence outlined above would not provide a conviction in a regular court (1984: 375): "*Wir suchen ein Mörderer erst, wenn feststeht oder doch wahrscheinlich ist, daß ein Mord überhaupt begangen würde ...*". He finds Guarducci's technical examinations inconclusive, and her reasoning strikes him as too harsh, or too prejudiced against Helbig. Wieacker also comments (1984: 398) that such an unfavourable description of the German scholar could only originate from an Italian view-point, since, in his own country, Helbig remains a highly esteemed figure: "*Dieses*

Bild [of Helbig] könnte wohl nur ein italienischer Betrachter liefern ...”.

A decade later, other evidence was produced. Edilberto Formigli was part of Guarducci's 1979 expert team, and at that time he found nothing to convince him of the fibula's authenticity (Guarducci, 1980: 466). In 1992 he decided on a new analysis of the fibula, and given the new techniques available, his results were somewhat different than those he had made earlier. His examination of the individual parts and shapes of the fibula found that the object in itself was not as unique as Guarducci had described it. There are many varieties of fibulas from the late eighth and seventh centuries BC, and a piecemeal study of these reveals nothing out of the ordinary about the Fibula Praenestina. The method of production of the fibula maybe hitherto unknown, and although it is similar to forgery techniques of the nineteenth century AD, it can just as well be explained as authentic seventh century BC (Formigli, 1992: 338-339). He concludes that it is simply more difficult to explain the fibula as fake, than it is to accept it as genuine.

Formigli also suggests (1992: 340-342) that the traces of abrasive acid found by Guarducci may well be the result of badly done nineteenth century restoration. Similar traces are visible on other objects, known to be authentic. More recently the Formigli team seem to have ultimately proven that the Fibula Praenestina has been authentic all along, through the use of electronic microscope and microprobe analysis (http://roma.repubblica.it/cronaca/2011/06/10/news/fibula_praenestina-172553841?ref=HREC2-9 [accessed 21 November 2011]), they believe it to a genuine ancient and authentic artefact.

Similar things are also claimed for the inscription. In 2007 in her book, Annalisa Franchi de Bellis argues that since the fibula, based on Formigli's 1992 analysis, is most likely authentic, the same must be true for the inscription. A supposed forgery is very unlikely, she writes (2007: 121), since it would have been virtually impossible for a nineteenth century forger to presuppose the various features in the text that were later verified by subsequent finds, and forgers are not generally known to pave the way for later scientific discoveries.

Passing A Verdict

As I have demonstrated in this article, both of these two cases are continuously revived, but with different cycles of interest and evidence, and with an alternating arguments about authenticity, favouring either side, defendant and plaintiff.

The Kensington Rune Stone was immediately rejected as a fake, by an almost unanimous scholarly field. This it remained up until the 1910s when Holand launched his full-scale authentication campaign. In the 1950s and 1960s, Wahlgren and then Blegen published crushing, negative summaries about evidence against its authenticity. Then, from the 1990s onwards Hall, followed by the Nielsen and Wolter team, have led the renewed defence of the authenticity of the Rune Stone, and the

publication of several papers in favour of the truth of its inscription.

The Fibula Praenestina was, at first, mainly positively received, and widely believed to be authentic. Professor Lignana's doubts about it were almost unnoticed. However, Pinza's many critical comments about the fibula, during the 1910s and 1920s, slowly undermined its credibility. A final verdict proved to be impossible, and the subject was left in limbo for almost half a century, until the mid-1970s, when Gordon took it back to the courtroom, trying to salvage the fibula's authenticity by accusing Pinza of mental instability.

Guarducci, in the early 1980s, did the complete opposite, denouncing Helbig as the fibula's forger, and demolishing his reputation. There was nothing similar to the “rescue team” of the calibre of Nielsen and Wolter when it comes to the later fate, and ruling on, the fibula. A conglomeration of philologists place the Praenestine forms into what they find is a sustainable Early Latin linguistic system, in support of the archaeologists who prove the artefact's authenticity.

As we have seen, the two objects were given very different initial receptions. The fibula was presented in a scholarly context, at the *DAI* in Rome, and thus was credible and published about from the very beginning. On the whole, philologists tended to support its claims to authenticity, whereas archaeologists and art historians were quick to reject this, finding the perfect forging culprit in the figure of Helbig.

The Kensington Rune Stone surfaced in a rural environment, and its inscription took a few months to get the attention of the academics who quickly repudiated it. And here we have the opposite situation: runologists find several faults with the inscription, but those who see more to the story of the discovery, believe Ohman credible, and favour the authenticity of the stone.

Despite all of the differences between the two artefacts and the two cases, the similarities in the treatment of both are striking. In the case of both inscriptions, courtroom debates and evidence identified a chosen forger or prime suspect, in the one case, Helbig, in the other, Ohman. Other potential forgers and assistants were not pursued, or implicated, or hunted down, in the same way. With both alleged forgeries, questions about the capacities and motives of the alleged forgers needed investigation: were they both skilled or shrewd enough to perpetrate a fake and present it as “true”? Or were the evaluations of the evidence of the inscriptions themselves and the language used in them, enough evidence of authenticity to exonerate them?

It is, in short, easier to consider a person guilty of a crime, or for that matter innocent, based on a description of his character and person, than to detail and evaluate all of the numerous pros and cons about the crime itself, if any crime was committed. This is why Guarducci devotes most of the second half of her monograph to the respective canniness of Martinetti and Helbig, and this is why Nielsen and Wolter spend more than three chapters describing Ohman and his surviving family as intelligent, hard-working and honest.

The consequences of finding for the authenticity or “truth” in the case of the Kensington Rune Stone are much greater than the same finding’s consequences for the fibula. If the Kensington Rune Stone is an authentic “first”, this means ante-dating any Viking presence in Minnesota to a much earlier date than has hitherto been proven. This also means that the inscriptions are “true”, that is, the great number of singular changes in Old Swedish of the fourteenth century, with regard to vocabulary, phonology, morphology and orthography, found only on the Kensington Rune Stone, that are nowhere else in evidence, are authentic as well. This contradicts other additional evidence that the text itself is also too long to be authentic. The occasional longer inscriptions there are on Swedish runic stones served a very different purpose, and were carved under different circumstances, in the midst of leisure and prosperity in the homeland.

If the Fibula Praenestina is genuine, as Formigli has recently argued, the consequences are less than the consequences of the authenticity of the Kensington Rune Stone, as nothing of really great note depends on it. The presence of Latin people at Praeneste is not a matter of dispute, and Latin literacy at that time can be proven by other artefacts, although these are a century younger than the fibula. All the odd features of the language of the inscription can be explained, with not that much difficulty, as evidence of a Latin dialect. And the text itself fits into the pattern of Latin ownership, gift or dedicational inscriptions.

In the end both supposed crimes, if this is indeed what they are, were committed more than a century ago, and their cases can only be described as effectively “cold”. If one finds the evidence of forgery compelling, then it has to be understood that there is little hope of ever confirming the real culprits. In both cases there are echoes of forgery and conspiracies, by contemporaries close to the main suspects, although there are no primary sources comprising confessions for either assumed fraud.

Perhaps the only verdict that can be made is that made by Lothar Wickert who, unable to get around the problem posed by the fibula in any other way in his history of the *DAI* in Rome, makes the exasperated conclusion that if Helbig really *did* administer the forgery of the fibula, he must have been such a genius that he ought to be awarded the Nobel Prize, posthumously (Moltesen, 1987: 237-238). And even if the language on the Kensington Rune Stone might now not be as convincing evidence in comparison to that now available regarding the fibula, with all the strange forms and signs there are, at least Ohman rose to the challenge of confusing contemporary scholars, and many more that followed during the century to come.

The Kensington Rune Stone is currently on display at the Runestone Museum, Alexandria, Minnesota, USA, where it is exhibited as genuine (<http://www.runestonemuseum.org/>). The Fibula Praenestina is kept at the Museo Nazionale Preistorico Etnografico “L. Pigorini”, in Rome, where it is described as being genuine, and its inscription as being an early example of the Praenestine dialect of Latin (<http://www.pigorini.beniculturali.it>). Although not

specified anywhere in the museum’s information, the inscribed fibula on display at the Museo Nazionale Romano Terme di Diocleziano, also in Rome (<http://archeoroma.beniculturali.it/node/97>), is a copy.

Notes

- 1 The contents of this article were first presented as a paper at the INSCRIPTA Epigraphy Network Autumn Meeting, October 29-30, 2010, Swedish Institute in Rome. Many thanks to Dr Anna Blennow, who read and substantially commented on an early version of this text.
- 2 <sklar> can be translated as Sw. *skylar*, Eng. *shocks/shelters*. The word appears in central Swedish dialects and north-eastern Norwegian of the late nineteenth century (Sköld, 2005: 7-9).

References

- Blegen, T C** 1968 *The Kensington Rune Stone. New Light on an Old Riddle*. St. Paul, MI: Publications of the Minnesota Historical Society.
- de Simone, C** 2006 Falisco *faced* – Latino arcaico *vhevoked*: la genuità della fibula prenestina e problemi connessi. *Incontro Linguistici* 29: 159-175.
- Franchi De Bellis, A** 2007 *La fibula di Numasio e la coppa dei Veturii. Quaderni dell' Istituto di Linguistica dell' Università di Urbino* 12. Alessandria: Edizioni dell'Orso.
- Formigli, E** 1992 Indagini archeometriche sull'autenticità della Fibula Praenestina. *Mitteilungen des deutschen archäologischen Instituts. Roemische Abteilung*. Band 99: 329-343.
- Gordon, A E** 1975 *The Inscribed Fibula Praenestina – Problems of Authenticity*. Berkeley, Ca: University of California Press.
- Gordon, A E** 1978 Further Remarks on the Inscribed Gold Fibula Praenestina. *Epigraphica* 40: 32-39.
- Guarducci, M** 1980 *La cosiddetta fibula prenestina – antiquari, eruditi e falsari nella Roma dell' ottocento. Atti della Accademia Nazionale dei Lincei. Memorie. Classe di Scienze morali, storiche e filologiche*. Serie VIII – Volume XXIV, fascicolo 4. Rome: Accademia nazionale dei Lincei.
- Guarducci, M** 1984 *La cosiddetta fibula prenestina. Elementi nuovi. Atti della Accademia Nazionale dei Lincei. Memorie, Classe di scienze morali, storiche e filologiche*. Serie VIII – Volume XXVIII, fascicolo 2. Rome: Accademia nazionale dei Lincei.
- Hall, R A, Jr.** 1994 *The Kensington Rune-Stone. Authentic and Important*. Ithaca NY: Jupiter Press.
- Holand, H R** 1932 *The Kensington Stone. A Study in Pre-Columbian American History*. Ephraim WI: s.n.
- Jansson, S B F** 1949 “Runstenen” fran Kensington i Minnesota. *Nordisk Tidskrift* 25: 377-405.
- Krummrey, H** 1982 Die Fibula Praenestina als Fälschung erwiesen? *KLIO* 64: 583-589.
- Lejeune, M** 1966 Notes de linguistique italique XXI: Les

- notations de F dans l'Italie ancienne. *Revue des Études Latines* 44: 141-181.
- Mancini, M** 2004 Latina Antiquissima I: esercizi sulla Fibula Praenestina. *Daidalos* 6: 1-30.
- Mancini, M** 2009 Il preterito latino tra continuità e discontinuità: facio, feci, fefaked. In Ancilotti, A. and A. Calderini (eds.) *Atti del I e del II Convegno Internazionale sulli Antichi Umbri. IRDAU-JI. La città italica*, vol. 1, pp. 69-96. Perugia: Jama.
- Meiser, G** 2003 *Veni Vidi Vici. Die Vorgeschichte des lateinischen Perfektsystems*. München: C. H. Beck.
- Moltesen, M** 1981 En forfalskningshistorie. *Meddelelser fra Ny Carlsberg Glyptotek* 37: 51-69.
- Moltesen, M** 1987 *Wolfgang Helbig. Brygger Jacobsens agent i Rom, 1887-1914*. Copenhagen: Ny Carlsberg Glyptotek.
- Moltke, E** 1953 The Ghost of the Kensington Rune Stone. *Scandinavian Studies* 25: 1-14.
- Ridgway, D** 1981 The Forgers and the Fibula. *Times Literary Supplement*, 19 June 1981, no. 4,081, p. 691.
- Rix, H** 1985 Das letzte Wort des Duenos-Inschrift. *Munchener Studien zur Sprachwissenschaft* 46: 193-220.
- Sköld, T** 2003 Edward Larssons alfabet och Kensingtonstenens'. *DAUM-Katta* 13: 5-11.
- Sköld, T** 2005 Kensingtonstenens språk. *DAUM-Katta* 15: 5-12.
- Untermann, J** 1993 Gr. $\epsilon\theta\eta\kappa\alpha$ = lat. feci, gr. $\eta\kappa\alpha$ = lat. ieci? In Meiser, G. (ed.) *Indogermanica et Italica. Festschrift für Helmut Rix zum 65. Geburtstag*, pp. 461-468. Innsbruck: Wiss. Buchges.
- Wachter, R** 1987 *Altlateinische Inschriften: sprachliche und epigraphische Untersuchungen zu den Dokumenten bis etwa 150 v. Chr.* Bern: Lang.
- Wahlgren, E** 1958 *The Kensington Stone. A Mystery Solved*. Madison: s.n.
- Wallace, B L** 2003 Vikings in North America – New and Old. *Viking Heritage Magazine* 4: 2-7.
- Wieacker, F** 1984 Die Manios-Inschrift von Praeneste: zu einer exemplarischen Kontroverse? *Nachrichten der Akademie der Wissenschaftlichen in Göttingen. I. Philologisch-historische Klasse*, Nr. 1: 371-399.
- Williams, H** 2004 The "AVM" Stone from Minnesota (Kensington II). *Nytt om Runer* 17: 40.