Records Provided by Dr. Runo Löfvendahl of the Swedish Heritage Board, Stockholm, Sweden on His Rejection of Wolter’s Claims Regarding the Age of Weathering on the KRS

Complied by Dr. Richard Nielsen, May 2011.

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THE KENSINGTON RUNESTONE – A TENTATIVE SUMMARY

Based on what is known of the KRS, and the exchange of views between us, I will summarize my present interpretation below!

You have, as far as I understand now, following interpretation of the KRS, as you consider solely two possibilities:

The stone with inscription is genuine, i.e. from 1362, as indicated by the text.

The stone is a hoax, cut by Olof Ohman.

Consequently, you start to prove that the stone cannot be cut by Ohman, and thus claim that he has indirectly shown that it is authentic. You exclude all other possibilities, because of the impossibility that the inscription once cut by an unknown Scandinavian could accidentally be found by another Scandinavian - Ohman. You also support this by comparing the mica weathering of the KRS and tombstone(s) from Maine, showing that the KRS inscription is at least 200 years old. In one of your later e-mail you even indicated that the inscription appears to be over 500 years old (e-mail 04.10.08). It would be interesting to know how you arrived at this age, and the error of this age estimate; what is the 95% probability (or 68% probability) that the stone is above 500 years, and how large is the error?

Our (the Swedish group) starting view was that the message could have any age between 0 and 900 years. Your examinations have shown that the inscription was hardly cut by Ohman, because of the difference in pyrite weathering between the “improved” and original runes. You have collected written testimonies that the majority of the runes were retooled after the stone was found. This is not especially obvious after all human interference. The tree hugging the stone when it was found also indicates that the inscription was at least 25 years old, when Ohman found it. As Ohman had owned the area only since 1890, the inscription could not possibly be made by him. Some of us questioned if the white lines on the back of the stone were caused by mycorrhiza on the roots of the tree.

Initially, I certainly found this idea plausible. However, after biochemists in Uppsala and Umeå studied pictures, and thought that it was impossible for these lines to form so rapidly, I got more hesitant. The lines certainly have a form corresponding to tree-roots, but I should like more info on this. If formed by tree-roots in so short a time (circa 20-25 years), this would certainly be no rare phenomenon.

There are still questions surrounding Ohman’s “improvement” of most of the runes, and I feel that more information can be extracted, both in micro topography and weathering chemistry of the stone and the runes. Suitable methods might not be accessible presently, but in the future they certainly will.
What about the mica weathering? You have studied the mica weathering on the “split” side of the KRS with mica weathering on 200 years old tombstones from Maine. The comparison is shrouded with approximations. The tombstone mica, in this case exclusively (?) biotite, has been atmospherically exposed since 1806. The microenvironment of the KRS since the carving of the runes is not accurately known, but it is possible that it has been situated underground most of the time since the cutting. The main form of mica in the KRS is muscovite (although you, based on prof. Ojakangas’ point-counting, maintain that biotite is also present). It is not obvious if you compare biotite with muscovite, biotite with biotite or considers this question irrelevant. This is a disputable point in your “proof”. Although you maintain that the comparison is adequate (the atmospheric weathering is slower below ground than above, the climate being comparable), and all differences will indicate that the comparison, if not exactly comparable, should result in added underestimates of the age of the KRS. It is a pity that you did not study the belovelground parts of the tombstones, and the mica weathering of these.

Hence, the mica weathering is presently the central question, and the only scientific indication to extend the age of the stone with an unknown number of years. The major problem is that with reductive action (with reductive action I mean that something is withdrawn from an object, in this case some of the stone surface is cut away, forming the runes). There is nothing left or added, giving any possibility to arrive at an age, based on remaining surface. This is a central point, which has caused much controversy, not least lately. Besides the KRS, I have followed at least three other objects/questions, where this is a major issue, namely the use of Schmidt test hammer to date mine and quarry walls in Sweden (Meurman 2000), the age and function of the stone ship “Ales Stenar” in southern Scania (data on this object can be found through a search engine such as Google) and the age of the Foz Côa petroglyphs in northern Portugal (Zilhão 2003; Bednarik 2003). In all these cases different views were presented. The opposing groups in all cases arrive at strongly differing ages or question the methods used by the opponent(s). In the case of Foz Côa, the age ratio between conflicting views is more than 20:1. This point[s] to a major shortage in tools and methods to solve the central question of atmospheric exposition age of rock surfaces.

The endeavor to use mica weathering as a tool to solve the KRS question is interesting, but presently undeveloped. The magnitude of weathering of the mineral must be quantified accurately. This also has to do with the difficult question of linearity of weathering with time for example. The problem is also that we do not agree of the possible age of the “split side” with the three finishing rune rows. Weathering is also a composite process, embracing chemical, physical and biological weathering. Chemically, biotite weathers faster than muscovite and physically they might have similar (?) weathering kinetics. There are however, so many unknown factors, that we feel it difficult to support any definite conclusion. It might be added that another mineral, calcite, might be a clue to the weathering (and age), as some runes are cut into this mineral. This mineral shows the strongly different microenvironments influencing the different surfaces of the KRS. Obviously calcite has formed during two different episodes (in situ and embedded in moraine), and has probably also weathered differently on different surfaces of the KRS. The runes cut in calcite seem to be shallower and less distinct than the others.

Our starting point might also be strongly different from another point of view. You had the aspiration to prove that the stone is false or genuine. Personally, I have started to distrust proof of authenticity. In principle, these types of objects are unsuited for proof of authenticity, and I rely on Popper’s falsification criteria as a better alternative. As falsification is the main point in Popper’s method, it cannot be used to prove that a theory is correct or true; it can at best show that a theory cannot be rejected as false. Also, I distrust in extending indications to proofs. In many cases, the goal might be to formulate better questions and find more reliable.

Runo Löfvenhahl
January 17, 2005.
2. **The Norwegian R. Martin’s “Swedish Study of the Kensington Runestone.”**

Martin (2007) quotes Löfvendahl’s final views on the dating of the KRS by geology, “...*Neither his [Wolter] investigations nor our own impressions can date the inscription. As you can see, we don't have enough material or results to write a reasonable paper. The runological content is still more important to the dating issue than any available natural-science criteria. I'm sure the last word hasn't been said yet: sooner or later someone will figure out a better method to pinpoint the date. But a method for direct dating of rock-carvings is hard to conceive! ...I have no problem with confessing my uncertainty in this case. I do, however, feel that it is suspect*.”
3. Undated memo from Dr. Runo Löfvendahl written in the summer of 2003.

Quoting Wolter (2011: Figure 2) from his “Report of Digital Microscopic Examination” to the Runestone Museum March 2, 2011, “This memo was written by Swedish Geologist, [Dr.] Runo L[ö]fvendahl, after reviewing and generally agreeing with the report on the geological investigation of the Kensington Rune Stone dated October 9, 2003.”

RN Note: This undated memo is from Dr. Löfvendahl sent in the summer of 2003 before the KRS arrived in Stockholm with me on SAS on a flight from Chicago on October 20, 2003. It is not surprising that Wolter does not identify the date because it does not mention the report dated October 9, 2003. Furthermore it obviously does not agree with many points sent in the material in the summer of 2003 by the RSM to Dr. Löfvendahl to review.
This report was relayed to the Runestone Museum (RSM) on August 13, 2004 as part of the Runestone Museum contract supplying the Swedish Historical Museum (SHM) the KRS for Display in the Fall and Winter of 2003. The Swedish Heritage Board was a consultant to the SHM for the geological review of the two KRS Studies (Those of Dr. Paul Weiblen and Scott Wolter).

Background:

Following comments are based on Wolter’s report dated 2003.10.18 and ocular studies of the Kensington rune-stone (KRS) at following occasions:

- 2003.10.25 by Laila Kitzler Åhfeldt (LKÅ), Carl-Magnus Mörtth (CMM), Kalle Dahlberg (KD) and Runo Löfvendahl (RL)
- 2004.01.26 by Laila Kitzler Åhfeldt, Risto Kumpulainen (RK), Kalle Dahlberg and Runo Löfvendahl
- 2004.01.28 by Laila Kitzler Åhfeldt, Mats Johansson (MJ) and Runo Löfvendahl
- 2004.02.10 in Hudiksvall by Scott Wolter and Runo Löfvendahl

Further, we have also consulted prof. em. Jan Lundqvist at the University of Stockholm, Dr. Reiner Giesler at the Swedish University of Agricultural Sciences in Umeå and Dr. Maria Malmström at the Royal Institute of Technology in Stockholm. RL also discussed the KRS with Scott Wolter in Stockholm 2004.06.02.

The KRS is a disputed stone with runic inscriptions, the authenticity of which is controversial. In the following we discuss Wolter’s conclusions and especially pinpoint those conclusions we find open to questioning. Here, we will refrain from discussing the text itself, the runic forms, word-forms and their derivation as well as the pentadic numbering system. Already directly after the stone was found in 1898, Scandinavian runologists such as Bugge and later for example Moltke and Jansson questioned the authenticity of the inscription. This conclusion is upheld to the present day by Gustavson and Knirk. This view is presently strongly questioned by Nielsen and others, who favour the interpretation that the text is authentic.

Geo-scientific studies of the KRS have been few. However, already early in the 20th century, Winchell (1909) made a thorough study, hesitantly arriving to the conclusion that the stone was genuine, a work from the 14th century [RN Note: Winchell’s conclusion depended on the water level in 1362 creating an “island” and the presence of two rocks in a lake a day’s march north from the “island” as details unknown to a modern carver. However, neither conclusion was correct since “island and peninsula” in Swedish and Old Swedish are the same word and the translation for rocks was wrong]. The last years, further studies have been reported by Wolter (2003), supporting Winchell’s view. Based primarily on mica weathering, Wolter concludes that the inscription is more than 200 years old.

It has been our endeavour to study the stone and compare our observations and interpretations with Wolter’s report and define those key points in his report that might be open to questioning. We have studied the stone during 8 hours (Löfvendahl additionally 6 hours together with Scott Wolter). This is far from enough, but forms the basis of our present views. Our starting point has been to find weaknesses or uncertainties in Wolter’s report, which might overthrow or question Wolter’s conclusions. Thus, we apply Popper’s falsification criterion (Popper 1983) as the guiding principle. One may ask: are Wolter’s indications and conclusions possible to test? We will try to show that this might be possible, and point to indications and conclusions that can be falsified or questioned. The central point is that a theory cannot be proved by induction; it can only remain acceptable by being non-falsifiable. This endeavour has the
Questions unrelated to the sides of the stone:

**Geology** – The stone consists of a metagreywacke regenerated in greenschist facies metamorphism. The grain size distribution is bimodal, with up to 0.5 mm large crystals of quartz, feldspar, muscovite and rock fragments in a fine-grained matrix of chlorite, micas, quartz, feldspars, opaques and calcite. The opaque minerals are ilmenite, often altered to leucoxene, and iron sulphide, probably pyrite according to microscopy by Ojakangas (Wolter, Table p. 34). No biotite was found by Weiblen (2001) with microprobe analyses (127 point analyses) but reported by Ojakangas (in Wolter p. 34) from point counter microscopy. Chlorite and muscovite are the major minerals according to Weiblen. Microscopy by (RK) shows that muscovite of two generations has two main orientations more or less perpendicular. As we do not know the orientation of the slide in all three co-dimensions, we cannot transform this orientation to the stone. The rock splits in thin parallel planes.

**Shape of the stone** – The KRS has an almost rectangular shape. The front and rear sides are parallel, and should most probably be formed by a single process, i.e. fracturing of the original rock. The rear side was later subject to glacial shaping and scratching. Additional fractures semi-parallel to these are closely spaced (distance less than 2 cm) healed joints located on the lower part of the front side. Consequently, all these surfaces seem to be formed contemporaneously and constitute a common fracture system in the rock where the KRS was an integral part. A closer study of these fractures shows that they cut the mineral assemblage and consequently form smooth surfaces. We do not know the development of the fracture systems and their relative ages, but interpret that the fractures were formed at a depth of a few kilometres. We also know that mountain chains in general are dissected by a prominent vertical fracture system perpendicular to the axis of the chain. Alternatively, fracture systems may form in steeply dipping strike-slip zones. Any other possible fracture system in the original rock was probably less pronounced and less pervasive. This is indicated by the structure of the “split side” which is rougher than the front side. It seems likely that this surface is parallel to one of the “poor” cleavage orientations observed under the microscope.

**Layering** – This parameter was not discussed by Wolter. The larger clastic grains are parallel-oriented, probably representing the original layering (RK). Another evidence for layering is a few mm wide chlorite-filled fractures cutting the length of the stone discordantly. Under artificial oblique illumination, there seems to be a variation in grain size, from one side of the chlorite-filled fractures to the other, i.e. a possible indication of original layering.

**Pyrite weathering** – This is one of the central criteria Wolter refers to when concluding that the runes of the KRS were not cut just before it was found, but are at least 18 years older. This conclusion is based on comparison with the AVM stone, a fake rune carving cut in 1985. In this stone one small crystal of pyrite (it is unclear if the mineral phase was positively identified as pyrite or iron sulphide) in the bottom of a cut rune had started to dissolve (Wolter 2003, figs. 32 and 33). However, in contrast to the KRS, a part of the crystal is preserved in the AVM-stone. In the KRS, bottom of sign 213, line 10, representing the rune “r” (numbering according to Wolter) two tiny holes are surrounded by rust halos. This indicates an age of the runes making it impossible for the finder to have cut the runes. We will discuss this indication later in the chapter on the “front side”. The comparison is not free from problems, as it is not exactly known how the KRS was stored in the period 1899-1907. There is some information that the stone was left outdoors, while other information suggests that it was stored in a shaft or leaning to the wall of a
shaft. Wolter (personal communication to RL) has collected information that it was placed inside the wall of a semi-open barn, sheltered from rain. This seems to preclude exposure to rain during the period 1899-1907.

**Mica weathering** – This is the central issue in Wolter’s report, but also very difficult to evaluate. Weathering is a complex process, in this case including at least chemical and physical weathering. As far as we know, nobody has used mica weathering for dating purposes. There is some literature on chemical mica weathering (White & Brantley 1995; Kalinowski & Schweda 1996; Malmström et al. 1996; Maurice et al. 2002). Kalinowski & Schweda (1996:378 and fig. 6) have studied and compiled rate constants for the dissolution of biotite, phlogopite and muscovite, given by different research groups. All these data show that biotite and phlogopite have higher rate constants than muscovite at atmospheric pressures and temperatures. At low pH the difference is large, up to two powers of ten; at neutral pH the difference approaches or exceeds a factor 10. In this case, weathering at atmospheric exposure (the Maine gravestones) and slightly below ground in moraine (KRS) are compared. The total time of contact between mineral and mobile, under-saturated rainwater is the major weathering factor. Secondary factors are temperature and pH of the fluid; the chemical composition of the fluid is also important. Physical weathering might be very important in the case of micas, as the mineral structure with thin, platy crystals forming “books” can rapidly be detached and be transported away after repeated freeze/thaw cycles. We will discuss these factors later.

**Calcite** – The parallel fractures, shaping the front and back sides of the KRS, are healed by secondary calcite and some chlorite (Wolter, figs. 5 and 8). This calcite covers the lower part of the front side, where the rune lines 7-9 start. Wolter interprets this calcite as precipitated at the original site, where the stone experienced low-grade metamorphosis. We agree with this interpretation. Another type of more yellowish calcite is found at the bottom of the stone. This type Wolter believes crystallised when the stone was deposited in the moraine. This conclusion is based on the fact that other boulders in the moraine have calcite coating (Wolter, fig. 5). This explanation also seems reasonable.

**Cleaning** – The stone has been cleaned repeatedly. Unfortunately, these measures were never documented in written form. When the stone was dug up in 1898, it was dirty and the runes difficult to interpret. According to some sources (Wolter 2003:21-23) the runes were “improved” by scratching their bottoms with a nail. As a consequence the bottoms of most runes are brighter in hue than the sides. According to Wolter, such a conclusion is supported by earlier photos, for example Steward’s; (see Wolter, p. 23-25 and fig.6). We will discuss this later.

Copies of the KRS were cast through the years, which might have influenced it’s surfaces. The first was made circa 1937 according to Langdon (1956). This was probably a gypsum cast, and engine oil was used as “releasing agent”. The stone was afterwards cleaned with petroleum ether according to Langdon. Later, copies were constructed by the Smithsonian Institute in the 1940s, by B. Wallace in 1965 and in silicon by the Runestone Museum in 2002. According to Wolter some of the colour contrast of the runes was weakened after the latest process. The brightness of the bottom of the runes is a major issue in the discussion of the KRS.

**The front side:**
The front side constitutes at least five relatively planar, close to parallel surfaces, approximately 10 mm thick. Towards the tapering bottom end of the stone, the surfaces are more discordant. It is not clear what they represent. Most of these more or less parallel fractures are filled with secondary minerals, mainly calcite and chlorite. The tapering bottom end is coated with yellowish calcite. The rune lines 7-9
start in calcite. The runes cut in calcite are less distinct and shallower than the other runes. They thus look more weathered, which is natural if the stone was atmospherically exposed during some period. This period must have been very short, as dissolution in humic climates of thin calcite coating would be reasonably rapid.

**De-lamination (cleavage) in front of lines 2 and 3** – This area is called “Oh shoot” (cf. Wolter, fig. 5). It is suggested that this area spalled off when the carver started cutting the second row. This conclusion is based on the indication that the first rune in line 2 seems to be discounted by the carver, who made a word divider and started with an “o” (the word “opdagelsefærd”). The abandoned rune seems to have one weak secondary staf, indicating that the intention was to cut an “o”. From line 4 onwards, the rune lines start directly at the edge of the front side.

**Winchell’s chip** - In 1909-1910 Winchell chipped off a small piece low down on the front side close to the calcite precipitate. The imprint of this chip might be found down at the edge as a rounded depression, probably shaped with a rounded hammer. This depression might be studied further, for example for weathering studies, if localised with certainty.

**Cutting and cutting technique** – Our specialists on stone cutting and carving (MJ and KD) agree that the carver used a chisel (Swedish -bredmejsel) and (probably) a hammer when cutting the runes. A 5 mm wide chisel seems to be the major tool. Further a pointing chisel (Swedish -pikmejsel) was used for word dividers. It is highly unlikely that chisels with widths of 15 or 25 mm were used. When cutting the chisel was slowly moved along the runes with straight staves until they were finished. The length of the major straight rune staves is 26-29 mm. The linear runes were often cut obliquely along one edge, but not the other. A professional carver would cut both edges obliquely. Where straight lines intersect, the runes have a tendency to split. This indicates that the carver was not familiar with this type of material, but not generally inexperienced. He seems to have used too much power in each blow, causing the runes to split. It is also obvious that the carver was cautious when cutting runes close to the edges of the stone. Runes with curved lines were cut with either a 5 mm chisel or pointing chisel. One of the specialists also suspects that a wider chisel, circa 8.5 mm was used, but this is uncertain. Some runes have a wider “foot”. Our specialists agree that the carver was no novice, but in possession of some cutting knowledge. The carver has carefully prepared the cutting, probably he painted the text on the KRS with chalk or similar pigment before cutting. The exfoliation in front of lines 2 and 3 might have transposed the text to the right, but this needs confirmation by carefully defining the sizes of the runes and their width and spacing along the whole text. There is no large variation in the sharpness of the runes along the text, so the carver had obviously access to a grindstone or some good substitute. The stone is so hard that the egg of the chisel had to be sharpened regularly. One word divider (sign 135 according to Wolter’s numbering) is round and regular, and might be formed by a drill. Unfortunately, the bottom of some of the word dividers are filled with a white precipitate (white flakes), so their real bottoms are not visible. In most cases the bottom of the word dividers is distinctly brighter than the walls, which Wolter attribute to a later cleaning with a nail or a similar tool. However, difference in illumination is amplified with artificial light, so we feel uncertain of this conclusion. We estimate that the whole operation (including pre-marking) would take 3-6 days. We have tried to find out if the chisels were mass-fabricated or hand-made, and if American (inch/foot) or European (metre) standards were used. We had no progress in searching for this. According to MJ, chisels were local and hand-made well into the 20th century in Europe and probably also in the U.S.

**Improved runes** – Wolter regards most runes to be improved and slightly deepened, for example sign 51, line 3, an “n”. Many runes and above all word dividers have brighter bottoms than sides and
Peripheral parts. In some cases this is caused by gypsum flakes attached to the stone. Excluding these, there are also several runes where the bottoms are brighter than the sides and exfoliated marginal parts. This is most prominent under artificial light. It is, according to our experience, common for quartz-rich rock types to show bright surfaces in the bottoms of carved figures, which can remain so for thousands of years. A good example of this is the rock-carvings of eastern Scania, such as Simris (RL). When studying these in daylight, the colour contrast is weakened. This contrast is also weakened after the silicon moulding according to Wolter (oral comm. 04.01.28). We hope that LKÅ can illuminate this with future studies of the 3D-registration. Scratching with any pointing tool would deepen the bottom and hopefully be discernible. According to MJ scratching with a nail would give a rougher and not so regular furrow. Thus, MJ believes that a chisel was used if the runes was later “improved”. We are not able to the take any definite stand in this question presently. The Holand “H” has been cut with a pointing tool, but is sinuous and of lower quality than the other runes. This sign is not distinctly brighter than the others.

The “split side” with runes:
Wolter regards this side as wedged by the carver, before starting to cut the text. The indications of this are the “imprints” of wedges along three edges of this side (Wolter, figs. 17,18). We are not convinced that these are true wedge imprints. After the imprints had been checked closer, it is obvious that they are concave and thus no imprints from an ordinary wedge. A wedge is flat and tapering towards the egg. It would leave a flat impression, often traceable further down the split surface. The imprints on the KRS are concave. Such imprints would be the result of a rounded hammer or other convex metal tool.

Wolter (Fig. 10) has suggested that the stone was originally lying horizontally, with the front side downwards contacting the bedrock. In that way the back side was worked by the overlying ice, which was sliding over it, forming striae and other glacial traces. Later, the stone was teared free from the bedrock. Alternatively, the stone was attached to the bedrock surface on its so called split side (RK; Fig. 2) In this case the split side might originate naturally, when the stone was broken loose by the action of the ice. The front side of the stone might in this case be oriented in the direction from which the ice sheet moved. This surface is wedge-shaped, while the top end of the stone is perpendicular to the flat sides of the stone, and would correspond to the distal (lee) side of the roche moutonée. The front side of the stone is almost planar. The fractures existed long before erosion exposed the surface. The rough surface of the split side is different from the front side, and might have broken off later than the other surfaces. We would not reject the idea that this surface might have formed during the latest glaciation by the action of the ice. But is it possible that this surface remained intact during transport in the ice? According to Jan Lundqvist, it would not be surprising if this perpendicular surface would remain intact when transported in the ice for tens or even hundreds of kilometres. We do not rule out the possibility that this surface might be several thousands of years old, and not contemporaneous with the carving. This conclusion is also supported by the loss of muscovite mica on the split surface. The surface is not flat, but distinctly curved. The roughness of the surface is probably the consequence of a weak schistosity, and that the surface “jumps” between a number of weakly developed schistosity surfaces. The surface would be brighter if formed in a rock with pervasive schistosity. As the surface is no prominent fracture, it is coarser in texture. The lower part is also warped. This would mean that all surfaces were exposed to weathering during the same period of time, if all parts were similarly exposed (mainly lying embedded in moraine).

Mica weathering – The mica in the KRS is mainly muscovite (white mica), with a lesser amount of biotite (black mica). Only Ojakangas (Wolter, p. 34) reported the presence of biotite. As a reference, Wolter, (figs. 61-63) studied biotite mica from gravestones in Maine. A comparison between the progress of
mica weathering of the “split side” and the front side (Wolter, figs. 51 and 52) shows that all mica is gone from both surfaces, although the surfaces are structurally dissimilar. Comparing biotite weathering on the Maine gravestones with that of muscovite on the KRS, the latter has progressed further, indicating that the KRS was subject to weathering for a longer period of time. From this Wolter concludes that the KRS “split side” is contemporaneous with the cutting of the runes, and that the original runes are older than 200 years, i.e. older than the worked gravestones. Is this comparison well founded, i.e. have muscovite and biotite similar weathering kinetics? To elucidate this question, we are relying on laboratory studies, foremost by Kalinowski & Schweda (1996). They have compared different researchers data besides examining these micas themselves, all summarised in their fig. 6. These data show that, at room temperature and pressure, biotite dissolves 1-2 powers of ten faster than muscovite (or phlogopite). A discussion with Maria Malmström supports the conclusion that biotite dissolves chemically distinctly faster than muscovite at pH-values below 7, and probably also at higher values. This refers to chemical weathering. With micas also the physical (mechanical) weathering might be important, especially at atmospheric exposure, with repeated freeze/thaw cycles. If, on the other hand, the KRS was lying partly to completely embedded in moraine, it would pass few freeze/thaw cycles, and thus experience less physical weathering. As the biotite originates from the atmosphere-exposed parts of the gravestones, the weathering might be speeded up compared to the buried KRS. With this comparison it seems reasonable to expect that biotite weathering would be 1-2 powers of ten faster than muscovite weathering under the discussed conditions.

There is also another difference between atmosphere-exposed and below ground environments, a factor studied by Swoboda-Colberg & Drever (1993). Their conclusion was that below ground chemical weathering is slower than laboratory studies by a factor 200-400. They speculated that the major discrepancy was the restricted contact between mineral and percolating water solutions in the moraine. These studies indicate that comparison between atmospherically exposed and below ground reaction kinetics is not very illuminating, especially if different minerals are compared. We find it quite surprising that all mica have disappeared from the supposedly young “split side”, and hence want to question its suggested Medieval Age. Either this surface is much older, or the KRS has been atmospherically exposed for a longer period of time. This latter possibility seems very remote to us. This conclusion is strengthened by the fact that no traces of lichens or other biological colonisation are found on the KRS. The presence of calcite on several sides of the KRS suggests that atmospheric exposition and weathering was very short-lived. The Minnesotan atmosphere would probably dissolve the relatively thin calcite coating in one or a few decades.

“The pyrite holes” – In the bottom of sign 213, line 10, an original “r”, there are two small holes surrounded by rust halos (Wolter, fig. 28). According to Wolter, pyrite crystals once filled these holes, but are now completely dissolved. The 18 years old inscription of the AVM-stone also has a pyrite of similar size in the bottom of a rune. In this case a part of the pyrite still remains. Wolter thus concludes that the original inscription of the KRS stone must be more than 18 years (the age of the AVM runes). His conclusion might be corroborated further, if it can be shown that the hole was left of a crystal with cubic or other crystal form of pyrite. We have also discussed the pyrite question with Maria Malmström, who have studied pyrite weathering in sulphide mine dumps. Firstly, one has to be convinced that pyrite is positively identified, as also other iron sulphides exist, such as marcasite (though pyrite is generally the most common). The major factors causing pyrite weathering is oxygen and water. The oxidation is catalysed by bacteria, foremost Thiobacillus ferrooxidans, but also other species. These bacteria are generally active in strongly acid environment, but some are also thought to be active at neutral pH. The oxidation is not believed to be especially pH-sensitive. Oxidation might also take place indoors at high RH (relative humidity). We regard it as important to definitely find out if the small holes in the KRS-sign
213 are the remains of original pyrite crystals. We are not convinced of Wolter’s conclusion that it is proved that the original cutting of the KRS is at least 18 years. As Henrik Williams has pointed out, the bottom of the Holand “H” cut in 1907 could serve as a “non-weathered reference”, with intact pyrite and muscovite in the bottom of this sign.

Will any other mineral help to estimate the age of the KRS surfaces and their cutting. We think that calcite can be of help. The rune lines 7-9 start on a surface coated with calcite. The depth and distinctness of the runes cut in calcite seem to be less than those cut in the original graywacke minerals. It might also be interesting to compare the structure of calcite in sharpness of cutting and the surrounding surface of calcite. The weathering of calcite is several powers of ten faster than that of the discussed silicates biotite and muscovite. Perhaps the 3D-evaluation of the rune-forms by LKÅ will help in this endeavour.

The Holand “H” – This sign, cut in 1907, is uneven obviously cut with a pointing tool. It is of low technical quality, and not distinctly brighter than the other signs. A closer study of the freshness of the minerals in this sign will be of value.

The rear side:
This surface was ground during the last glaciation, with both coarse, rough striae and thin, straight ones. The surface has a distinctly different appearance than all the others. It is stained, with alternating, even, dark polished surfaces and rough, coarse, brighter ones. Wolter’s idea for the original localisation of the stone (fig. 10) seems reasonable, but other explanations cannot be ruled out. The alternating hypothesis that the “split side” was attached to the underlying rock is possible. This would mean that the surface could be glacial, i.e. more than 15 000 years.

The light streaks – Wolter regards these (Figs. 12 and 13) as formed by mycorrhiza growing from the roots of the poplar tree that embraced the stone when it was found. He believes that the mycorrhiza of the roots excreted iron from the stone, giving the light colour of the lines. The specialists we consulted (Dr. R. Giesler, SLU Umeå and collegues) are of the opinion that 100s of years of water contact would be needed to extract iron from the stone surface, to give the present pattern. It is obvious that ectomycorrhiza of many trees genera can extract nutrients from bedrock surfaces (Rosling 2003:part 4, figs. 2-5). Ectomycorrhiza would form filiform, μm-thin mycelia, and not compact cm-wide zones as upon the KRS. Furthermore, iron is no important nutrient for the trees, in the same quantity as calcium, magnesium and potassium. The whitish lines will most probably be formed by prolonged contacts with channelled water, and not by ectomycorrhiza, that will be creeping over the surface with the root-tips, not remaining stationary in the same place for many years.

Damages – There are a number of scratches and cut marks running discordantly over the glacial striae, none especially prominent. There are also some pigment blots, both red and blue, and a few semitransparent, brownish-yellow grains, which we were unable to characterise and fully analyse.

Short side II:
There are four dark lines (Wolter, figs. 3 and 4) representing healed fractures filled with dark minerals, probably chlorite.

Bottom side:
Yellowish white precipitate of calcite was probably formed when the boulder was deposited in the moraine after the latest glaciation terminated, just as Wolter suggests. This end is distinctly tapering, may be indicating that the stone was standing with this side dug into the ground.

Top side:
This side is quite dark, especially the exfoliated surface close to the “split side”. White flakes of gypsum are distributed over it, obviously added with the first casting of the stone in the 1930s.

**Additional information requested from Scott Wolter**

1/ Is the iron sulphide positively identified as pyrite or just analytically as Fe and S in the KRS and AVM stones?
2/ Is the bottom of the Holand “H” completely fresh? Is pyrite found in this sign? Are there some oxidised spots in it? Micas?
3/ Are the whitish lines on the back side homogeneous, or do they consist of a net of intertwining, white lines?

**Conclusions**

1/ The light lines on the back side are not useful for age determination (since we doubt that they are the result of ectomycorrhiza leaking from the roots of the tree found embracing the stone).
2/ The origin of the “split side” is unclear. We now prefer to interpret it as natural, as the imprints devised by Wolter do not correspond to wedges, but possibly some convex tool, leaving a concave imprint. We feel that the different appearance of the split and the back side for example might be a consequence of the internal structure of the stone and the exposition to the ice and its erosion.
3/ The suggested improvement of the runes by retooling is uncertain. The problem is that later copying has partly obscured the brightness of the suggested fresh appearance of the suggested retooling. Further, our experience with Swedish rock-carvings and rune-stones tells us that it is natural that the bottoms of the carved figures appear brighter than the sides. The reason is that almost all power in each percussion is taken up by the peak or egg of the percussion tool, resulting is crushing/micro-fracturing of the hardest minerals quartz and feldspar. The colour difference can remain visible for hundreds, even thousands of years, if the surface is not strongly weathered.
4/ The pyrite weathering needs a deeper study, especially to define crystal form of the mineral, grain size, and the extent and distribution of transported and precipitated rust. We are aware of the difficulty in studying these with a satisfying resolution in situ.
5/ The mica weathering, i.e. comparison of biotite/muscovite above/below ground, is difficult to apply. We see it as very disputable to compare muscovite weathering below ground with biotite weathering above ground. May be, a study of the fate of biotite on the gravestones from Maine below ground can give some inking related to this question!
6/ Are the geographical references in the runic text of any value? Especially interesting is the fact that the text says that the stone is on an island, that there are skerries (or does this word mean anything else) a day’s journey north and that the sea is a 14 day’s journey away?

**What more can be done**

1/ Archaeological excavation of the find locality. We know that there are indications that the stone was transported to the find site, but all the same there might be a chance to find something of interest at the site, if a thorough examination was done.
2/ A careful sign by sign study might give further information about cutting technique, about the possible retooling, the homogeneity and size of the signs and later alteration of them.
3/ Mica weathering and a comparison biotite and muscovite above and below ground. What about the importance of chemical respectively physical weathering under natural conditions? How important are freeze/thaw cycles for the mica deterioration?
4/ Search for similar boulders; Winchell found one boulder out of 500 with the same rock as the KRS after a search. This boulder has not been localised, and is probably lost (Wolter, pers. comm. to RL). A similar glacial boulder as the KRS would allow study with all possible techniques, even destructive ones. It would be especially interesting to quantify the alteration of mica minerals and study pyrite oxidation.
5/ A close up study might also allow a better description of the tools used when cutting the runes.
6/ Age of the “split side”. Is it distinctly younger than the other surfaces. Why do the surfaces look different.

References
5. Rebuttal of Elements in Wolter’s “Science, Archaeology and the Human Condition” by Dr. Richard Nielsen

A. Introduction: Scott Wolter has reacted in the May 2001 MES Newsletter to letters send on his Venus Alignment and Hooked X articles, both in ESOP Vol. 26 (2008) to the editor of *Epigraphic Society Occasional Papers* (ESOP), which were printed in Vol. 27 (2009). See Correspondence www.richardnielsen.org. Quoting Wolter (2010:13), “The responses by Richard Nielsen and Mr. Frankki are nothing more than carefully crafted personal attacks hidden beneath the veil of an ‘academic response.’...I intend to support my claim of a personal agenda behind these rebuttals. “However, the Editor, Don Buchanan, does not allow personal attacks in letters or articles in ESOP as Wolter alleges.

Wolter covers many topics in his essay, but I will respond first to incorrect comments made about Dr. Runo Löfvendahl. The report, Löfvendahl (2004), disagrees with Wolter’s results on KRS geology and Wolter creates an imaginary scenario that Runo actually told Wolter he agreed with him. Secondly, the claim by Wolter that his geology report on the KRS has received peer-review by geologists is roundly disputed. Thirdly, presented is some of the lack of evidence in Wolter’s reports that would not even allow a proper review.


C. Anecdotes about Dr. Löfvendahl: Quoting Wolter (2010: 14), “When I returned to Sweden four months later…Runo [Löfvendahl] took the train 2 ½ hours north of Stockholm, to the Hudiksvall Museum and away from his colleagues. Runo and I spent seven hours alone looking at the Rune Stone and discussing the various geological aspects. When we were finished, he verbally agreed with all of my findings. We then sat down in the lobby and wrote down only the findings of fact that we agreed on. We made no interpretations and drew no conclusions. I then signed and dated the paper and then slid it across the table to Runo and said, ‘Sign it.’ With moistening eyes, he pushed the paper back and said, ‘I can’t.’ Why couldn’t he sign it? Is there some type of directive in Sweden that doesn’t allow a scientist to conduct honest research on controversial American runestones?”

In Nielsen and Wolter (2006: 344-5), Wolter has a different story about this meeting in Hudiksvall, Sweden on February 10, 2004, “…When we discussed our work on relative age dating
of mica using tombstones, I agreed with his point that it is a new method that needs further study for full acceptance. ...It was nearly 6:00 p.m. when we finally felt like we had covered everything. I asked Runo if we could write down some [of] the points we had agreed on, which left us little time before his 7:00 train left for Stockholm. We felt good about the highly productive time we spent together... but [Runo] also said that he would issue a final report.”

Martin (2007) quotes Löfvendahl’s final views on the impossibility of dating of the KRS by geology, “...Neither his [Wolter] investigations nor our own impressions can date the inscription. As you can see, we don’t have enough material or results to write a reasonable paper. The runological content is still more important to the dating issue than any available natural-science criteria. I’m sure the last word hasn’t been said yet: sooner or later someone will figure out a better method to pinpoint the date. But a method for direct dating of rock-carvings is hard to conceive! ...I have no problem with confessing my uncertainty in this case. I do, however, feel that it is suspect”.

D. Where are the peer reviews? Quoting Powell (2010): “Wolter doesn’t see the necessity of publishing his work in a peer-reviewed journal.” Quoting Wolter (2010: 13), “...both archaeologist Larry Zimmerman and runologist Henrik Williams imply that my work has not been properly peer-reviewed [,] which is completely false. My work, in fact, was peer reviewed, in writing, by several senior geologists and material scientists. This is hardly credible. If the peer reviews are in writing why have they not been produced to Eric Powell and others?

Quoting Wolter (2010: 14), “When asked, I gave Mr. Powell four of those names [of those who gave a peer review] and to my knowledge he only interviewed one person. He did speak with Professor Emeritus of Geology at the University of Minnesota-Duluth, Richard Ojakangas, yet he is not mentioned in the article, presumably because he had positive things to say about my work. Apparently the opinion of archaeologist Larry Zimmerman is more important than Dr. Ojakangas’ scientific peer-review of my work?”

Eric Powell tells me that of the four only Prof. Ojakangas returned the call, albeit after the deadline for the article. In the History Channel Film, “The Holy Grail in America”, no geologist endorsed Wolter’s geological results. Prof. Ojakangas told me he was interviewed for the film, but not on the subject of peer review? ” Prof. Ojakangas confirmed to me recently, “Obviously I am not eligible to be a peer reviewer, and do not recall saying anything about his work in writing.” Zimmermann (2006) simply confirms the fact that no scientific paper has been written on the KRS geology by Wolter and it was not an opinion about quality of Wolter’s work, as Wolter avers.

The Maine State Museum (MSM) received Wolter’s e-mail on 23 January 2006 as part of his request to examine the Spirit Pond Rune Stones in his office. Quoting Wolter (2006b), “The first point is relative [The MSM geologist] comment about an apparent lack of peer review of the geologic work we performed and reported in our book. The fact of the matter of is that my report has been peer reviewed, in writing, by eight senior geologists and geological engineers.
These individuals are Professor Emeritus John Green, Professor Emeritus Charles L. Matsch, Professor Richard Ojakangas [all three are debarred from giving a former student a review], Professor Emeritus G.B. Morey, Professor Emeritus Paul Weiblen [Neither Professor Morey nor Weiblen received any report], Dr. Bryant Mather (now deceased), geological engineer Terrance Swor P.E. and Senior ACI [American Concrete Institute] International Fellow, Richard Stehly P.E.” [These two men worked with Wolter, which disqualifies them from giving a peer-review].

E. Geological Reports: Quoting Wolter (2010: 15), “The fact is my geological work is published in two reports and two books, and anyone in the world is free to review that work and comment on it.” There should only be a single scientific paper with opposing evidence to the theories advanced with the backup data from Ames-Iowa fully incorporated. This must be published in a geological journal with blind peer-review. Popular books and unpublished reports cannot suffice. In Nielsen and Wolter (2006) and Wolter (2003 and 2004 a & b) the results given in Weiblen (2001) and in Löfvendahl (2004) (both these latter two references are found on www.richardnielsen.org) that both show an absence of biotite in the thin sections were ignored. Weiblen (2001:10) has shown that the potassium layered silicate encountered in the electron microprobe transverse of 127 analyses is the clay, illite, rather than muscovite mica. The X-Ray diffraction in Weiblen (2001: Fig. 15) confirmed this. Löfvendahl’s investigation on the same thin sections by point count found no biotite either, which confirmed Weiblen’s results. Prof. Ojakangas (2002) did find biotite in an undocumented KRS thin section (Nielsen and Wolter 2006: 33), but this does not guarantee biotite is part of the initial KRS chip mineral components. The EDX results were claimed on Figure one to have proven the presence of biotite in the KRS chip (Nielsen and Wolter 2006: 37). Obviously, no age of weathering determination on the chip can be made without first proving the presence of biotite.

After the RSM regained custody of the core research material in late 2008, it was later discovered by Prof. Emeritus Paul Weiblen and I that the core research records were lacking the claimed EDX records that would prove that muscovite and biotite existed in the chip’s fresh fracture. Fig. 1 is now unsupported without the published data on EDX and the important accompanying technical data to allow replication. Replication requires exact location of the biotite and muscovite mica minerals sampled, biotite and muscovite comparison standards, instrument settings used in the process, and chain of custody on the samples to name a few of the deficiencies prohibiting replication at present. This required data was entirely absent from the record submitted to the RSM.
Figure 1: “This SEM image shows numerous blade-shaped biotite and muscovite mica minerals identified by EDX on the freshly fractured surface on the back side of the chip sample” (Nielsen and Wolter 2006: 37).

**F. Summation:** As a co-author of Nielsen and Wolter (2006) and with a stake in the geologic results I have an obligation to state the research record as I know it today and not remain silent. The current evidence shows that the geology section in Nielsen and Wolter (2006: Chapter 2) regretfully has no scientific support. The only written review, Löfvendahl (2004) (See www.richardnielsen.org), rejects outright Wolter’s age of weathering theories due to the lack of required tests for muscovite weathering time required under the agreed protocol, Popper’s Falsification Principle (Criterion), during the Swedish examination in the winter of 2003-4 in Stockholm. I agree with the Swedish assessment that is no test evidence to support muscovite mica disappearing from the KRS chip in 500 years (Nielsen 2009b: Appendix D) (See www.richardnielsen.org). When compared to the geological report by Prof. Emeritis Paul Weiblen (2001) (See www.richardnielsen.org) of the University of Minnesota, Wolter’s work lacks the detailed scientific data and information required for replication in a scientific article, in which he be the first one claiming to have determined the age of weathering of minerals in rock. For this to stand scrutiny the evidence must be totally unambiguous. Prof. Weiblen (2001) has written the best article on KRS geology written to date and his report content ought to be fully emulated by Wolter in my opinion.

Unfortunately, Wolter has consistently made unsupported claims related to his studies, including that they have been peer-reviewed by no less than five professors of geology, but he refuses to produce any documentation to prove this to me or anyone else. He appears to believe that repeated assertions will prevent the facts from surfacing, but eventually his
inadequate approach on these issues will be revealed to all who are willing to look for the facts. Of course this type of behavior can be a big setback for the credibility of KRS research, both here and abroad. I personally can spend no more time on correcting the record on KRS research. Several years of work has already been wasted. I have done this now to clear the way for others to take up new geological research on the KRS along the lines recommended by Weiblen (2001) and Löfvendahl (2004), but unfortunately ignored these many years.

Wolter ought to produce now in writing his additional geological research data and the reviews of his work he claims he has in order to put these matters to rest. He should then write the required scientific paper with a full discussion of the geological evidence brought forth in Löfvendahl (2004) and Weiblen (2001). Until this is done Wolter has no credence for his oft repeated claim to have demonstrated that the KRS is at least 200 years old.

I regret it has been incumbent on me to respond to Wolter’s article in the May 2005 newsletter of MES on behalf of Runo Löfvendahl. Correction of the misinformation given about Löfvendahl in Wolter (2010b:14) (MES May Newsletter) should be Wolter’s first priority.

Publication of the identified missing KRS core proof material should be his only other priority. I sincerely hope that Wolter now takes upon himself to address these very formidable tasks. He has the opportunity to make a sincere attempt to “right the ship” that is riding on his geological studies. It is better that he rights it, rather than leave the task to others to carry out.

Richard Nielsen

References
http://scienceblogs.com/aardvarchaeology/2007/05/swedish_study_of_the_kensington_to.php


