

CONCERNING THE KENSINGTON RUNE STONE

When the Kensington stone was found in 1898, the credibility of its inscription seemed so doubtful, not only because the stone was found so far inland, but because of its contents, that it was condemned as a fraud by all. Photographs of the inscription were sent to many scholars both in Europe and America. Among the chief arguments of these scholars against its authenticity were the following: First, the inscription shows that the rune master was ignorant of the proper numerals and substituted a system of his own invention. Second, he tells of an expedition in which two national groups, Goths and Norwegians, participated—an unthinkable mixture in personnel. Third, the expedition is said to have taken place about 1362—a time more than three hundred years removed from the known period of the early Norse voyages to America. These, and objections about linguistic and runic forms, were quite enough to consign the inscription to oblivion, and for many years it lay forgotten.

But about ten years later the inscription had a resurrection, and shortly after this it was conclusively shown that it was the critics—and not the inscription—who were in error about the three objections cited above. They have not since been presented against it. In like manner other objections were met, and the belief in the truth of the inscription has been gaining ground. In 1932 my book on the Kensington stone was published. It met with a kind reception; nearly all the fifty-odd reviews, in fact, expressed views favorable to the authenticity of the inscription. The two prominent exceptions have been Dr. M. M. Quaife's article, "The Myth of the Kensington Rune Stone," in the *New England Quarterly* for December, 1934, which was

answered by the present writer in the following issue, and Professor L. M. Larson's paper, "The Kensington Rune Stone," in *MINNESOTA HISTORY* for March, 1936. In this latest attack there is nothing particularly new, save that Professor Larson ventures to fix the early eighties as the time when the alleged fraud was perpetrated. He thinks that the stone may "have been shoved in under a growing tree that was old enough to endure a little tampering with its roots."¹

But in presenting this theory he forgets that in such case there would be no weathering of the inscription whatever, since the inscribed characters would not be exposed to the air. He shuts his eyes to the unchallenged testimony of a number of geologists who declare that the weathering of the inscription shows that it was chiseled long before Minnesota was settled by white men. The stone was on exhibition in St. Paul, Madison, Chicago, Rouen, France, and Oslo, Norway, and was inspected by numerous geologists. Not one of them has expressed the opinion that it was recently carved. Inasmuch as there seems to be no known artificial way of producing weathering of stone, the inability of these geologists to find evidence of recent origin is highly significant. In 1909 and 1910, when the controversy concerning the stone was at its height, the stone lay on a table in the office of the Minnesota Historical Society's archaeologist, Professor Newton H. Winchell, a geologist of more than national fame. He and Dr. Warren Upham, another geologist of national distinction, had the stone in their keeping for more than a year and examined it most thoroughly. Winchell discusses the weathering of the inscription with much detail in a report submitted to the Minnesota Historical Society, and he comes to the conclusion that the inscription may be five hundred years old.² This report was con-

¹ See *ante*, p. 22, 37.

² "The Kensington Rune Stone," in *Minnesota Historical Collections*, 15: 233-237; "Newton Horace Winchell," in *Minnesota Historical Collections*, 15: 828.

curred in and signed by Dr. Upham and the other members of the investigating committee. Upham also wrote the following statement for publication:

When we compare the excellent preservation of the glacial scratches shown on the back of the stone, which were made several thousand years ago, with the mellow, time-worn appearance of the face of the inscription, the conclusion is inevitable that this inscription must have been carved many hundred years ago.³

Professor W. O. Hotchkiss, state geologist of Wisconsin and later president of the Michigan school of mines, wrote in 1910:

I have carefully examined the various phases of weathering on the Kensington stone, and, with all respect for the opinions of philologists, I am persuaded that the inscription cannot have been made in recent years. It must have been made at least fifty to a hundred years ago and perhaps earlier.⁴

We have another unsolicited testimony from the late Professor O. E. Hagen, who for many years gave intensive study to cuneiform inscriptions. These writings traced on small bricks are often counterfeited for commercial purposes, which calls for special attention to the patina and weathering of the inscription. In his study of epigraphic characteristics, Hagen developed high skill, which makes his observations on the physical phases of the Kensington inscription of special value. In a brief report on the Kensington stone, he has the following to say of the age of the inscription:

In epigraphic respects I find in the inscription no evidence that it is anything except what it purports to be. I worked over the stone for a whole day under different kinds of light and found the runes on the whole to be what I looked for from that time and the people that are mentioned in the inscription.⁵

³ First published in Hjalmar R. Holand, "First Authoritative Investigation of 'Oldest Native Document in America,'" in *Journal of American History*, 4: 180 (second quarter, 1910).

⁴ Statement given to Professor Winchell and mentioned in *Minnesota Historical Collections*, 15: 236.

⁵ See complete statement in *Reform* (Eau Claire, Wisconsin), April 29, 1926. For further particulars, see Hjalmar R. Holand, *The Kensington Stone*, 57-59 (Ephraim, Wisconsin, 1932).

This unanimous testimony of geologists and archaeologists would seem to be almost conclusive evidence of the authenticity of the inscription. Yet Professor Larson does not so much as allude to the whole question of weathering save to dismiss it as "distinctly of minor significance." Such a cavalier disposal of perhaps the most vital line of inquiry regarding the stone, at the very beginning of his article, hardly conduces, it would seem, toward respect for Professor Larson's line of argumentation on other points. But his arguments may be taken up briefly in the order of presentation.

1. Professor Larson is disposed to reject the entire evidence concerning the age of the tree underneath which the stone was found. He gives two reasons. The first is that "since no one had taken the trouble to measure the girth of the tree, there was nothing definite to remember." This objection will scarcely commend itself to his readers. The discovery of this stone was an extraordinary event in the lives of its finders, and it is reasonable to suppose that they would retain a sort of photographic impression of the stone and the tree in their minds. Dr. Knut Hoegh questioned each of the five witnesses separately to learn what this impression was. Two of the witnesses stated that the tree was "about 10 inches" in diameter at the base, while the other three said it was from "8 to 10 inches."⁶ When five witnesses separately agree so closely, there seems to be no reason for rejecting their testimony. These witnesses were all men of good character who apparently had nothing to gain by distorting the truth.

Professor Larson's second reason for rejecting the evidence concerning the tree is a statement made by Professor George T. Flom that Samuel Olson, one of the witnesses,

⁶ Larson, *ante*, p. 21-23; Holand, *Kensington Stone*, 40. Dr. Hoegh describes most favorably the trustworthy character of these witnesses. See his "Kensington og Elbow Lake stenene," in *Symra: en aarvog for norske paa begge sider af havet*, 5: 184-187 (Decorah, Iowa, 1909).

had told him that the tree was four inches in diameter. This is a rather weak objection, for Flom has no signed statement or affidavit to support his claim. When Olson was shown this report by Flom, he indignantly repudiated the assertion.⁷ It would seem that Professor Flom made his investigation in a rather desultory manner. Thus, he apparently made no attempt to interview a variety of witnesses and check their statements one against another. His report on what was said by Olson is invalidated by Olson's later repudiation. Professor Flom spoke also with Olof Ohman, who conducted him to the place where the stone was found and pointed to a tree of approximately the right size. Instead of measuring it and obtaining a signed statement, Flom merely guessed that it was seven or eight inches thick. Such a slipshod inquiry is hardly of much value in a scientific investigation. Furthermore, there seems to be no reason for rejecting the testimony of the other four witnesses because of Flom's hearsay report about the fifth. The most that an exacting judge could demand would be that both Flom's and Olson's testimony be rejected because of their disagreement. This leaves four separate testimonies which stand unchallenged and agree in substance about the size of the tree.⁸

2. Professor Larson uses considerable space in attempting to show that the explorers of 1362 could not have entered Minnesota by way of Hudson Bay because of the danger of starvation and scurvy in wintering on the shores of that bay. He cites the sad fate of Captain Jens Munk and his crew as proof. But it cannot be denied that scores, perhaps hundreds, of vessels have wintered there. Radisson mentions several vessels wintering there as early as 1680.

⁷ Olson's signed statements are printed in Holand, *Kensington Stone*, 292, and in *Minnesota Historical Collections*, 15: 222-224.

⁸ See Holand, *Kensington Stone*, 37-46, for an estimate of the probable age of the tree, based on tabulations compiled by the United States forest service from the annual rate of growth of hundreds of specimens examined. The result is about seventy years.

If starvation and scurvy would make Paul Knutson's journey impossible, then the same causes would frustrate the efforts of the Hudson's Bay Company, but we know they did not. The fact is, the hardships of life on the shores of Hudson Bay appear to be greatly exaggerated. Knud Rasmussen and six companions spent two winters and a summer on a small naked island in the extreme northern part of Hudson Bay, seven hundred miles north of Nelson River. They found an abundance of game and fish and enjoyed good health. Peter Freuchen spent almost twenty years in the extreme northern end of Greenland on the seventy-seventh parallel, fourteen hundred miles north of the Nelson River. As early as 1333 three Norsemen from southern Greenland spent a winter on a small island off the coast of Greenland on the seventy-third parallel and left an inscription there to prove it.⁹ Why then should it be impossible for a company of picked men to spend a winter in the southern part of Hudson Bay, eleven hundred miles farther south?

3. Professor Larson next discusses the difficulties of ascending the Nelson River and quotes with tacit approval a statement by Dr. Quaife that "such a journey was beyond the resources of ordinary men."¹⁰ Shall we then conclude that these explorers of 1362 were incapable of doing what thousands of later voyageurs and fur traders have done? From 1680 up to the present time employees of the Hudson's Bay Company have been going up and down the Nelson and Hayes rivers and portaging around the falls in exactly the way that the travelers of 1362 would have had to do it. In one way it would be less difficult for the latter to venture upon this river trip, for they would not be deterred

⁹ Knud Rasmussen, *Den store slæderejse*, iii (Copenhagen, 1932); Peter Freuchen, *Arctic Adventure*, (New York, 1935); Magnus Olsen, "Kingigtorsoak-Stenen," in *Norsk tidsskrift for sprogvidenskap*, 5: 189-258 (Oslo, 1932).

¹⁰ See *ante*, p. 26.

by a foreknowledge of the labors that awaited them. Once having set out, it would be a matter of meeting obstacles as they came. As these obstacles were not insurmountable, the travelers would eventually, barring accidents, reach the quiet waters above.

4. Professor Larson mentions the implication in the inscription that it was written on an island. "Unless competent geographers are ready to come forward and testify to a strong conviction that there was an island in that place in the fourteenth century," he will refuse to believe it. In another place he asserts that it probably did not exist and he calls it a "mythical" island.¹¹ I do not know how many testimonies from competent geographers will be required to still his doubts. His objections have been answered by two scientists of national distinction. One is Winchell, the other is Upham. Being geologists who were familiar with formations of glacial origin all over the state, they were probably as qualified to speak on this question as anyone. In a letter to me of August 17, 1911, Winchell writes: "The changes of physiography are such that no faker could have wrought them into such an inscription within the last 100 years." This he repeats in a more emphatic way in a statement written for publication as follows:

I am convinced from the geological conditions and the physical changes which the region has experienced, probably during the last 500 years, that the stone contains a genuine record of a Scandinavian exploration into Minnesota, and must be accepted as such for the date named.¹²

In the report of the museum committee of the Minnesota Historical Society, which was written mainly by Winchell, he also repeatedly "testifies to a strong conviction that there was an island in that place in the fourteenth century." This report was signed by Upham, a recognized authority on Minnesota geography, who separately affirmed his conviction concerning the existence of the island in the four-

¹¹ See *ante*, p. 26-29.

¹² Holand, in *Journal of American History*, 4: 180.

teenth century, in an article in *Records of the Past*.¹³ Both scientists made several local examinations of the topography.

5. The fourteen days' journey. The inscription states that ten men were left in charge of the boats down by the ocean (*hawit*). This ocean is said to be fourteen *þagh-rise*, or "days' journey," from the place where the stone was found. As the nearest arm of the ocean is Hudson Bay, which is about 1,050 miles from Kensington by way of the meandering river route, I have assumed that the distance indicated by the term *þagh-rise* is the same as that designated by *dægr sigling*, a unit of distance equal to about seventy-five English miles; in this sense the term was generally used by sailors, as is admitted by many scholars, including Professor Larson.¹⁴ Professor Larson emphatically objects to the idea that a sea term would be used in reckoning travel on land when another term, the *rast* or *röst*, was in general use. He finds evidence in this to indicate that the inscription cannot be genuine. But if the rune master had used *rast* or *röst*, this would be strong evidence that the inscription could not have been written in the Middle Ages, for the *röst* was never used in measuring distances traveled on lakes or rivers. While *röst*, meaning "rest," was in general use in land travel, it represented no definite unit of distance, but only "the distance between two resting places." This distance was variable, depending on whether one traveled in a flat or a mountainous country.¹⁵ It was

¹³ *Minnesota Historical Collections*, 15: 221, 247, 251, 253; Warren Upham, "The Kensington Rune Stone, Its Discovery, Its Inscriptions, and Opinions concerning Them," in *Records of the Past*, 9: 6 (January-February, 1910).

¹⁴ Fridtjof Nansen, *In Northern Mists, Arctic Exploration in Early Times*, 2: 170 n. (New York, 1911); William Hovgaard, *Voyages of the Norsemen to America*, 61-64 (New York, 1914); Geoffrey M. Gathorne-Hardy, *The Norse Discoverers of America*, 198-211 (Oxford, England, 1921); Andrew Fossum, *The Norse Discovery of America*, 80, 91 (Minneapolis, 1918).

¹⁵ Hjalmar Falk and Alf Torp, *Etymologisches wörterbuch* (Heidelberg, 1910); Richard Cleasby and Gudbrand Vigfusson, *Icelandic-English Dictionary* (Oxford, England, 1884).

therefore used more frequently to indicate the length of time occupied in making a journey than to designate the actual distance covered.

There was a much more definite standard for measuring distance. This in its unit form was the *vika*. The root word is *vikja*, meaning "to move," "turn," or "shift." *Vika* originally referred to the time spent in rowing a boat by one shift of oarsmen until they were relieved by another.¹⁶ The distance covered in these *vika*, or "shifts," varied in different parts of the Scandinavian countries, depending presumably on the kind of boats used locally and the number of oars. This variation in the length of the *vika* is preserved in the difference in length of the mile in the northern countries, as the Scandinavian mile is approximately the same as the *vika*. The local difference in the length of the *vika* or mile is immaterial to this discussion. The *tylft*, as the word shows, was a dozen *vika* and was the same as a *dægr sigling* ("day's sailing") reckoned by its ordinary maximum. This ordinary maximum for a twelve-hour journey was seventy-five miles or a little more, as stated above. A *vika* was, therefore, in Norway and Sweden, equal to about six and one-fourth English miles. As most of the travel was done at sea, and as *dægr sigling* was the most comprehensive for measuring long journeys, this is the term most frequently used, but there were also similar multiples of *vika*, such as *dag rodr* ("day's rowing") for sea travel. Of travel on land N. M. Peterson writes: "Often the journey's length is described in terms of *dags reise*, *dags för*, *dags leit* and *vikur för*."¹⁷ In nearly all such journeys, whether on sea or on land, it must have been necessary almost daily to compute distances by *vika*, and then add these together for a larger denomination, because of shifting

¹⁶ Hjalmar Falk and Alf Torp, *Etymologisk ordbog* (Christiania).

¹⁷ *Grönlands historiske mindesmærker*, 3:228, 240 (Copenhagen, 1845); *Haandbog i gammel nordisk geografi*, 133 (Copenhagen, 1834); J. E. Rydquist, *Svenska språkets lagar*, 6:73 (Stockholm, 1850-1883). See also the references listed in footnote 14.

winds and other troubles which cut down the ordinary maximum.

The use of *vika* as a unit of distance was not confined to ocean travel, but was used also in computing distances on fjords and inland waters. Thus Ivar Baardsson, who about 1342 was supervisor of the bishop's properties in Greenland, describes a small lake as being two *vika* wide. Another early writer says: *vatnit var halfrar viku breitt*, "the lake was half a *vika* wide." The term was used whether the journey was made by sailing, rowing, or swimming. This is illustrated by the story in *Landnama*, in which a kinsman of Erik the Red swims to an island a long half *vika* from the shore in order to get fresh beef with which to feed his visitor. The expression was used also in describing the relative position of points on land. Ivar Baardsson states that Snefellsness is twelve *vika* farther west on Iceland than Reykjaness.¹⁸

The Kensington explorers, upon leaving the sea, where for seven years they had been counting their *vika* and adding them up into *dægr sigling*, would have no reason for changing their system. They continued on the water in boats as before. The Nelson River is seven miles wide at its mouth and extends like a fjord for a long distance inland. It is broken by many rapids, but for long distances it expands into long lakes. When the explorers reached Lake Winnipeg, they came to a vast expanse of water, three hundred miles long, and so wide that for much of the time they could not see the other side. The travelers would therefore do as they were accustomed—add together the number of *vika*, and comprise them into a larger measure. As the journey in boats continued to within a few miles of the place where they left their inscription, this would leave only a very small fraction of the journey that could be reckoned

¹⁸ *Grönlands historiske mindesmærker*, 3: 251, 254, 643; *Fornmanna sögur*, 8: 32 (Copenhagen, 1825-1837); *Landnama*, 107 (Copenhagen, 1843); *Flateyjarbok*, 1: 554 (Christiania, 1860).

in *rösts*. Yet Professor Larson would have reckoned the whole journey in *rösts*!

6. The identification of the fatal camp site "by 2 skerries one days-journey [*þags-rise*] north from this stone" stands or falls with the interpretation of *þags-rise* as representing seventy-five miles. Until this interpretation is proved fallacious, nothing more need be said.

7. Professor Larson calls Paul Knutson a "fanatical explorer" because he ventured to explore the interior of the new land.¹⁹ If so, we must give the same contemptuous designation to many later distinguished explorers of America whose endeavors differed little from his. There is, for instance, considerable similarity between La Salle's last journey, of 1684-87, and that of Paul Knutson. Both followed the shores of North America, the one on the south seeking the mouth of the Mississippi, the other on the north seeking a company of apostates. Both failed in their missions and saw fit to return to their headquarters on the St. Lawrence by an overland journey. If Paul Knutson was a fanatic in venturing a thousand miles inland, what shall we say of Jolliet and Marquette, who penetrated more than twice as far? And supposing Knutson were a "fanatical explorer," what of it? His character and motives have little to do with the abstract question of whether the Kensington inscription is true or false.

8. Professor Larson proceeds to discuss the linguistic features of the inscription. He complains that:

One of the earliest arguments directed against the authenticity of the stone was that the language of the inscription gave an impression of having a distinctly modern character. . . . Any man of average education, if he hears the inscription read aloud correctly, will be able to understand every word and every line, even though he has had no previous knowledge as to its tenor and purport.²⁰

This is about as scientific as to argue that the Canterbury Tales or the old English ballads must be spurious, because,

¹⁹ See *ante*, p. 29.

²⁰ See *ante*, p. 30.

forsooth, "any man of average education" will be able to understand practically every word and line if he but hears them "read aloud correctly!" The Swedish and Norwegian *diplomataria* abound in letters from the fourteenth century which have the same "distinctly modern character." For instance, Henrik Henriksson, a priest from West Gothland, who in 1371 was King Haakon's household chaplain, wrote intimate letters to the king which phonetically are quite modern.²¹

9. Professor Larson points out that, according to Dr. Stefan Einarsson, I have committed a number of errors in my linguistic analysis. Here, at last, he has found a just cause for criticism, for I humbly admit that this charge is true. In this analysis I have quoted about two hundred and fifty excerpts from half as many fourteenth century writings to illustrate words and constructions under discussion. In so doing I have misunderstood certain words and forms and thus quoted some irrelevant passages, which I greatly regret. I am very thankful to Dr. Einarsson for pointing out my errors. He has written the best and most helpful review of my monograph on the stone, although he is far from complimentary in some respects. Some of the errors that he notes are later mentioned by Larson, and I will discuss them in their proper place, as I am under no delusion as to infallibility.

But after all is said, it is not the present writer who is on trial in this investigation, but the Kensington inscription. If Larson had remembered this, he would not have alluded to Dr. Einarsson's commentary, for this does not cite a single linguistic objection against the inscription. On the contrary, Dr. Einarsson has in several instances added weight to the linguistic evidence in favor of the inscription. In his closing summary I fail to find any indictment against

²¹ See Henriksson's letter of 1371 in *Diplomatarium Norvegicum*, vol. 6, no. 278; also his letter of 1370, written at the dictation of Queen Margaret. A facsimile of the latter appears in A. Taranger, *Norges historie* (Oslo, 1909-1917).

the inscription, but rather an appeal to other scholars to give it the study it deserves. He writes:

He [*Holand*] has actually succeeded in producing some linguistic material not easily refuted in support of the inscriptions, as for instance parallels to show that the singular of verbs could be used for the plural, or that the old dative plural was supplanted by the accusative.

In view of this circumstance and of the many other facts speaking for the genuineness of the stone, the well-documented story of its origin, its weathered appearance, the plausible connection with fourteenth-century Scandinavian history, etc.—it is my conviction that linguists and runologists would do well to take the matter under renewed consideration before rendering their final verdict.²²

10. *Oppagelsefarþ*. Larson cites Dr. Einarsson to show that I am in error in deriving *oppage* from the Old Norse *dagar uppi*, which according to Vigfusson means “day dawns upon one.” Johan Fritzner and Einarsson translate it *en overraskes af Dagens Frembrud*, that is, “one is overtaken by the dawn.” This seems to have a cognate meaning to “reveal,” “disclose,” or “discover,” but perhaps I am wrong in seeking the derivation there; I am not an etymologist. It was Gustav Indrebö, professor of Old Norse in Bergen University, who proposed the derivation to me in writing. Inasmuch as two such eminent philologists as Indrebö and Einarsson disagree on this, I am willing to drop this attempt at derivation and, like Dr. Einarsson, be content to say that the word is a loan from the German or Frisian. As Dr. Einarsson says: “There is no reason to object to this special word.”²³

11. *Mans* and *man*. Professor Larson does not state the criticism correctly when he says that I confuse the nominative singular *maþr*, meaning “man,” with *man*, meaning “slaves.” My error was that I confused the collective genitive *mans* (called *artens genitiv*), meaning “folk” or “people,” with the collective neuter *man*, meaning “people

²² *Speculum: A Journal of Mediaeval Studies*, 8: 407 (July, 1933).

²³ Cleasby and Vigfusson, *Icelandic-English Dictionary; Speculum*, 8: 404.

of one's household" and, most commonly, "slave or slaves." This collective genitive was also used in the nominative in the fourteenth century, as appears in a letter of West Gothland of 1349: *twer gother mæns siggia*. My mistake was in trying to be too explicit. This mistake does not, however, in the least affect the propriety of the use of *mans* in the inscription, as I have given many illustrations of its similar use both in Old Norse and in modern Icelandic, and also one from West Gothland, the home of one of the two groups mentioned in the inscription. Dr. Einarsson calls this last "an excellent example in proof of his [*Holand's*] contention."²⁴

12. I acknowledge with thanks Professor Larson's correction that *ö* was not the last letter of the Danish-Latin alphabet in the fourteenth century. While it appears as a separate letter variously marked to indicate different sounds between *e* and *o*, it was considered as a form of *o* until much later. I have given numerous examples from the fourteenth century showing a close similarity in form to the *ö* in the inscription, as Professor Larson admits.²⁵

13. The inscription contains seven strange characters which are not true runes. I have shown that five of these strange signs are found in manuscripts of the fourteenth century and that they must have been borrowed there. Larson admits this, but objects that this explanation is of no help, "since it does not prove that they ever were used in runic inscriptions."²⁶ The answer is that it is quite immaterial whether or not these seven characters were ever used in another runic inscription. The pertinent question is whether there is a good and sufficient reason for their use in the Kensington inscription. There are only three reasonable explanations for the presence of these strange symbols, and these three possibilities will here be discussed.

²⁴ *Svensk diplomatarium*, vol. 1, no. 4503; *Speculum*, 8: 407.

²⁵ Holand, *Kensington Stone*, 112-117; Larson, *ante*, p. 33.

²⁶ See *ante*, p. 34.

First, the inscription was written by a modern but unlettered forger who, without access to books, had learned his runes from runic inscriptions in his home vicinity. This theory will scarcely commend itself, for the inscription shows that its alleged modern author was a man of much learning in philology and history. Nor would this theory explain why he did not use the proper runes for *a*, *k*, *u*, and *y*, which may be seen on almost any rune stone. The similarity of the Dalecarlian rune for *a* does not prove that it is the work of an immigrant from that part of Sweden for, while there is a similarity, there is one important divergence—the hook on the upper right arm. Moreover, it does not explain the signs for *a*, *e*, *g*, *i*, *k*, *l*, *n*, *o*, *p*, *s*, *t*, *u* (*v*), *y*, *æ*, and *ö*, whose equivalents are all very different in the Dalecarlian runes. Moreover, the language of the inscription makes a Dalecarlian origin impossible. I have elsewhere compared a dozen words in the inscription with their Dalecarlian equivalents, and shown that there is wide dissimilarity between the two dialects.²⁷ Both in runic signs and dialectic terms, Dalecarlian usage shows less similarity to the Kensington inscription than does that of practically any other district in the Scandinavian north.

Second, the inscription was written by a forger who got his information from books. This theory is no better than the former, for if the author of the inscription got his information from books on runes, there is no reason for the presence of the seven strange symbols. A forger would naturally follow a standard runic model as closely as possible, for any divergence from it would create suspicion. The theory of a forgery can give no reasonable explanation for the absence of the proper runic forms for *a*, *u*, *k*, and *y*, as they are very common, while *æ*, *ö*, and *j* are rare. Nor would a forger of 1884 (the latest possible date, according to Larson) know anything of the peculiar Latin script forms

²⁷ See Adolph Noreen's complete table of Dalecarlian runes, in *Fornvännan*, 1906. It is reprinted in Holand, *Kensington Stone*, 120–124.

used in the fourteenth century unless he had gained access to unpublished documents stored in the big libraries, for these documents were not published in facsimile until after this date.²⁸

The third alternative is that the inscription was written approximately at the time it purports to have been, and upon that assumption all these runic irregularities can be satisfactorily explained. The use of runes had by 1362 become infrequent because the Latin alphabet had become the means of expression in writing. Probably only one or two of the explorers had any knowledge of these obsolete signs, and only the fact that runes were much easier to carve, as they are nearly all made up of straight lines, could have recommended their use. For seven years these men had toiled amid strange scenes, and their runic expert had no means of refreshing his knowledge, as there was then no existing book on runes. In such circumstances it would not be strange if he could not recall the true form of all his runic symbols. I should not be surprised if a keenly intelligent man of today would be unable to reproduce all the signs of the Greek alphabet seven years after he had studied them.

In such a dilemma the rune master would necessarily have to replace the missing characters with equivalents from the Latin script. Personally, he may have been ignorant of such scripts, but this deficiency possibly could be supplied by someone among his nineteen associates. The substitution of Latin script letters for runic signs in an inscription as late as 1362 is therefore a perfectly reasonable thing to find.

14. Battle axes. Professor Larson now turns to a review of the "corroborative finds" described by me. He cites Dr. Einarsson to the effect that K. P. Petterson, a Swedish writer, describes an ax, apparently similar to the

²⁸ H. Hildebrand's *Svenska skriftprof* was published in 1894; Kaalund's *Palæografisk atlas* in 1903, *Flatøyjarbok* in 1906. The first volume of *Diplomatarium Norvegicum*, which was published in 1847, contains two pages of facsimiles, but these illustrate the use of only one of the seven strange symbols.

one shown in figure 21 of my book, as being used for hewing the surface of log timbers — that is, a broadax.²⁹ The outstanding feature of the ax shown in figure 21 in the *Kensington Stone* is its thick blade, which is an inch in thickness a little more than an inch from the cutting edge. American broadaxes have thin blades, permitting the ax to enter the wood at an almost parallel line with the surface of the timber, thus producing a smooth surface. An ax with an edge like a crowbar (see figure 21) could only be made to enter the wood by a very heavy strike and at a very blunt angle — the very things an expert timber worker seeks to avoid. While I shall look further into this matter, I am inclined to believe that Mr. Petterson has misapprehended the uses of the ax mentioned by him. In the museum in Skara in West Gothland there is an ax which the curator, Dr. S. Welin, says “in all important features is identical” with that shown in figure 21. He adds that “the axe has undoubtedly been used as a battle-axe, especially for smashing helmets.”³⁰

Pending further study of this ax, it may be stated that the criticism adduced does not in the least affect the antiquity of the other similar battle-ax shown in figure 20 of my book. The circumstances of its discovery show that it was in its finding place hundreds of years before Minnesota was settled by white man. This ax was found within a forest which, when the pioneers came, comprised about a thousand acres. No buildings have ever been erected on this quarter section, “the land being gravelly and almost unfit for cultivation.” The following is a part of the affidavit describing the circumstances under which it was found:

The trees had been cut some years before, and the stumps were pulled up by Mr. Davidson by means of a stump-puller. Beneath one of these stumps he found a heavy axe of strange shape, the like

²⁹ K. P. Petterson, “Lantmannaredskap i Nagu,” in *Folkloristiska och etnografiska studier*, 2: 131–197 (Helsingfors, 1917).

³⁰ Holand, *Kensington Stone*, 173.

of which he had never seen before. The top of the stump under which the axe was found was more than two feet in diameter, and my husband said that it must have been several hundred years old. The axe lay quite deep in the hole, about a foot and a half below the surface of the ground.³¹

The affidavit is signed by Mrs. Martha Davidson, an elderly woman of refinement and unquestioned honesty, who had no reason for falsifying the facts of the finding.

As to the age of this type of ax, Dr. Bengt Thordeman, assistant curator of Stockholm's large historical museum, gave me the following statement:

The axe marked B. Thm [see Fig. 20] is in type practically identical with St. Olaf's axe on the reredos from Østeråker . . . now in the National Historical Museum, dated 1468.³²

As Professor Larson has no criticism to offer against the two ancient axes pictured in figures 22 and 24, nothing more need be said here.

15. The fire steel. Professor Larson writes:

As to the fire steel, one need only call attention to the statement of the finder: "The fire-steel is just the same size and form as the fire-steels which my grandmother used 65 or 66 years ago." . . . How anyone, in the face of this statement, can claim a medieval origin for the steel is difficult to understand.³³

The purpose of this distorted statement is evidently to make me look ridiculous. For some reason Larson chooses to suppress the two salient facts which make this fire steel an object of archaeological interest. One is the remarkable circumstances of its discovery. This is stated in a signed statement and a corroborative affidavit. The following is the statement of the finder, Ole Jevning, whose letter is dated Climax, Minnesota, June 8, 1914:

I have your letter concerning the fire-steel which I found. I settled here in June, 1871, and we were the first to take land around here. A short time after I settled here, I was boring holes with a six-inch post-hole auger. When I got about two feet down I heard

³¹ Holand, *Kensington Stone*, 159.

³² Holand, *Kensington Stone*, 172.

³³ See *ante*, p. 35.

something scrape against the auger and I pulled it up thinking I had struck a stone. The dirt clung to the auger and I examined it looking for the stone and found the little fire-steel. It was much rusted and there was also some charcoal and ashes. It must have been there a long time, because the place where this hole was bored was on a dry elevation. The fire-steel is just the same size and form as the fire-steels which my grandmother used 65 or 66 years ago. . . .³⁴

The important fact in this communication is that this Norwegian fire steel was found about two feet below the surface of the ground in a layer of charcoal and ashes. This implies that the steel was lost while a fire was being made. As the charcoal and the fire steel were found so deeply under the surface of a dry knoll, this indicates that a very long period—probably hundreds of years—must have elapsed since the fire steel was left there, for the fire steel *and the ashes* could only have been buried together by the slow accumulation of wind-blown particles of dust and the decaying humus of many years' vegetation.

Inasmuch as the environment in which the fire steel was found shows that the latter was left there long before any Scandinavian settlers came into the West, the next step was to learn if this could be a French fire steel left by some early *coureur de bois*. Investigation showed that this was not so. A full-size photograph was sent to the McCord National Museum in Montreal, where many early French fire steels may be seen. The museum reported that the Minnesota steel "has a distinct character of its own which is not French." Similar inquiries in all other possible directions but one proved equally negative. The one exception was the University Museum in Oslo, whose assistant curator wrote as follows:

At the same time I will state that the fire-steel which carries the same mark [Fig. 25] in its entire form with the spiral ends is of exactly the same type as the fire-steels which in great numbers have been found in Norwegian graves from the Viking Age.³⁵

³⁴ Holand, *Kensington Stone*, 167.

³⁶ Holand, *Kensington Stone*, 176, 179.

The two important facts concerning this find are therefore: first, that the fire steel was found under circumstances which preclude the possibility that it could have been brought in by the settlers of that region; and second, that precisely similar fire steels were in use in the Middle Ages in the Scandinavian countries and nowhere else. These two important facts Professor Larson in his search for truth chooses to suppress. Instead he mentions only the immaterial fact that such fire steels were in use in Norway about 1850. This only shows that the same type of fire steels have been in use from the Middle Ages down to the present, just as have the same type of awls, buckets, fishhooks, and the like. Under the circumstances, I feel urged to paraphrase Professor Larson and remark: How anyone, searching for historical truth, can thus suppress important facts, is difficult to understand.

16. The spearhead. Professor Larson makes the following observation on spears:

Spearheads were made in all lands and in a great variety of forms; individual smiths had their own patterns. One need go no farther than Webster's *New International Dictionary* (see under "lance") to find a picture of such a head, which, except for the length of the blade, has all the essential characteristics of the one shown in Holand's book.³⁶

This shows that Larson has not yet taken up the study of spearheads, for his statement is full of misconceptions. For instance, the *length* of the blade is the least typical feature of a spearhead and it often varies within one country. The *width* of the blade, on the other hand, is a fundamental characteristic. The spearhead shown in the dictionary is quite wide because it is English. It would be strange indeed if a single wide spear blade of domestic make, like the one referred to by Larson, could be found in any of the twenty-odd museums of Sweden and Norway, as the people

³⁶ *Ante*, p. 36.

of these countries preferred spears with narrow blades and long tapering points.

An even more typical difference is found in the length of the hollow shank. English spears, of which I examined more than a hundred in the Tower of London, all have long heavy shanks, because the shaft is held in place by the shank only. The same is generally true of German spears shown in the Zeughaus in Berlin, although there are other typical differences. Norwegian and Swedish spearheads are characterized by very short shanks, three to four inches long. The spearhead found near Whitehall, Wisconsin, has a shank three and three-eighths inches long on the outside, permitting only one and three-eighths inches of the shaft to enter the shank.³⁷ For this reason the shanks of Scandinavian spears were provided with two opposite strap-like extensions of steel, about seven inches long, which held the shaft firmly by means of clinched bolts. These straps are never seen on English and French spears, and rarely on German. It is a mistake to suppose that "individual smiths had their own patterns." The shape of a spear, which in the Middle Ages was almost as much a part of a man's attire as a necktie is now, was prescribed by well-established national customs.

With the possible exception of one, the five implements all belong to the same remote period as the date on the Kensington stone. They were found under circumstances which preclude the possibility of their having been brought in by early settlers. Their place of origin is southern Sweden and Norway, just as the members of this expedition are said to have come from southern Sweden and Norway. They were discovered in the same part of America as was the stone, and no similar finds have been reported elsewhere. They are just such things as these explorers needed on their journey. On the assumption that they belonged to the ex-

³⁷ Holand, *Kensington Stone*, 168-170, 177.

plorers of 1362, their presence is easily understood. On any other assumption, each of these finds is just as puzzling as is the stone to its critics.

In closing this survey one can perhaps do no better than recall Professor Larson's final words:

The theory that the forgery was intended as a hoax disposes of all the problems that the inscription has raised. A forger who is preparing a fraud of this sort does not have to be consistent on all points. In his syntax, in his choice of runes, and in his dealings with geographical facts he is likely to allow himself much freedom, always being careful, of course, not to wander too far afield.³⁸

Quite so! Yet by his own line of argumentation, Larson would have us believe that this forger, although "always being careful . . . not to wander too far afield," nevertheless "chiseled the inscription on an island that probably did not exist . . . three hundred miles from a sea which apparently was as mythical as the island"; that he used seven runic characters which could only serve to bring his production under fire, where any rational being would have used standard forms; that he perversely employed "at least a dozen important deviations" from fourteenth-century literary usage; that in spite of everything, he "could probably make some claim to scholarship"! Truly, a strange creature, this hypothetical forger of Professor Larson's!

Instead of crediting him with "some claim to scholarship," one wonders if this forger is entitled even to a claim for ordinary common sense or sanity. Why couldn't or wouldn't any forger have written "41 days' journey" instead of "14 days' journey," supposing he knew only these two symbols, and thus brought at least a glimmering of sense to his critics? Why couldn't he have followed any ordinary textbook on runes and Old Swedish syntax? Why didn't he make his inscription short, instead of one of the longest on record, and thus avoid most of the pitfalls his critics insist he fell into? Why did he bury his amazing

³⁸ *Ante*, p. 37.

production, face down, under a tree on a stony hillside, where it might never be found? Why, with all the physical work and scholarship involved, didn't he use even a child's intelligence in perpetrating his fraud?

Dark questions, these. Professor Larson blandly ignores them all. And then, to cap it all, we find that by some curious legerdemain, the alleged forger—this industrious scholar and blundering nitwit—is able to create on his runes a weathering that fools completely the ablest geologists of his time and region! A truly astounding creature!

HJALMAR R. HOLLAND

EPHRAIM, WISCONSIN



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