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Since the publication of the article in ANTIQUITY, June, 1951, Professor Johannes Broendsted’s observations from his American Journey (as a guest of The American-Scandinavian Foundation) has appeared in Aarbøger f. Nordisk Oldkyndighed 1951 with an extensive summary in English (observations on weapons supposed to be of Scandinavian origin, on the Newport Tower and on America’s Runic Stones). In this article Broendsted publishes an article on the Kensington Stone by K. M. Nielsen, who, too, denies the genuineness of the inscription.
THE KENSINGTON STONE
By Erik Moltke
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In 1898 a Swedish-American farmer, Olaf Ohman, cut down an aspen tree on his farm. He found enveloped in its roots a large flat stone which he only just managed to get out without ruining his axe. When his ten-year-old son had brushed some of the dirt from the stone, Ohman discovered that one of the faces and one of the edges were covered with strange engraved figures. It was soon decided that these had something to do with runes. This is where the saga of the Kensington stone begins: from Ohman's farm to a shop window in Kensington, to Prof. O. J. Breda in Minneapolis, to Prof. George O. Curme, back to Ohman's farm condemned as a blatant forgery. Here it lay despised as "a stepping stone near his granary for eight years, without further notice." It was "rediscovered" by Hjalmar Holand, bought by him, and he devoted his life (three large books and innumerable articles) to attempting to prove that the inscription was genuine. Finally in its Jubilee year 1948 it was given the place of honour in the National Museum at the Smithsonian Institute in Washington as one of the finest pre-Columbian monuments of America.

American and European newspapers during the Jubilee year were full of articles about the stone, partly because two leading Scandinavian scientists had been called to America to give an opinion. They were the Swedish runologist Sven B. F. Jansson, who declared subsequently in a talk on the American radio that the stone was false, and the Danish professor of Archaeology Jogs. Brǿndsted, who brought back an excellent copy of the inscription but otherwise left the conclusive word to the students of runes and language.

In 1910 a little book was published: - The Kensington Rune Stone: Preliminary Report to the Minnesota Historical Society by its Museum Committee. The result of the investigations by the committee was that the inscription was genuine, pre-Columbian. Nevertheless the report ends with a letter from Prof. N. H. Winchell, the philological expert on the committee. In it he is sorry that he is going away, but he says: "I have examined your report carefully, have visited Kensington and neighbourhood, and have read most of the papers and articles relating to the rune stone. I have always agreed with the great authorities of Norway and Sweden, Magnus Olsen, Moltke, Moe, M. Hogstad (i.e. Haegstad), Bugge, Noreen, Schrick (i.e. Schuck), Montelius, in thinking that the language is too modern, besides being faulty; and a more careful study of the words has not changed my opinion." To these outstanding names, and especially the runologists and philologists Sophus Bugge, Magnus Olsen and Adolf Noreen, may be added the names of the greatest runologists of Sweden and Denmark, Otto V. Friesen, Elias Wessen and Ludvig F. A. Wimmer. Now two younger runologists have severely questioned the authenticity of the stone, namely the present writer,\(^1\) and also Sven B. F. Jansson in Nordisk Tidskrift 1949. A couple of years before the appearance of these articles the above-mentioned journal Danske Studier for 1946-47 published an article on the inscription by the Danish Emeritus Prof. in the Eskimo Language, William Thalbitzer, who upholds the authenticity of the inscription. Recently a paper by S. N. Hagen has appeared in the American Journal Speculum (July 1950). Hagen says: "As far as I know, no linguist or runologist has come forward with the reconsideration suggested by Elmarsson (disagreeing with Holand who is deficient in certain elementary fundamentals). While we are awaiting a study of the inscription by a competent scholar, I offer a few observations..." Hagen supports the authenticity of the inscription, for example in the following words: "This inscription should be a perfect joy to the linguist because it is such a delightfully honest and unsophisticated record of its author's own speech. A forger would have tried to imitate a language other than his own. It is clear that this author tried to imitate no language but his own. In branding this beautiful inscription as a forgery, scholars have thrown away not only an important historical document but also a faithful record of medieval Scandinavian speech." Finally I am acquainted with an article in the Swiss Journal Atlantis (Sept. 1950). It is written by one of the former supporters of the inscription, Prof. Richard Hennig, Dusseldorf, and although he knows both Jansson's and my article, he concludes his statement with the following pompous words, reminiscent of the runologists of the Hitler period: "The authenticity of the Kensington Stone has been proved and thereby the presence of Scandinavians in America fully 130 years before Columbus is no longer in doubt."\(^2\)

1. In the newspaper INFORMATION, of 9th Feb. 1949, on the basis of the material brought back by Brǿndsted, later published in the journal DANSE STUDIER, 1949-50, with a linguistic examination of the inscription by Dr. Harry Andersen.
2. German original: "Die Echtheit des Kensington-Steins ist erwiesen und damit die Anwesenheit von Skandinaviern in Amerika volle 130 Jahre vor Columbus nicht mehr zu bezweifeln."
As far as the most recent authors are concerned, one may well say that Thalbitzer's philological arguments have been so hard hit by the specialised criticism of Jansson and Harry Andersen that they have no leg to stand on. In spite of Hennig's determined judgment he produces no new argument, but confines himself to an enumeration of all Holand's old chestnuts. As for Hagen, his article shows that he is a scholarly Scandinavist— from time to time he puts his comrades in arms, Holand, gently and kindly on the right road, when the latter displays his ignorance of the elementary rules of Old Nordic grammar. He makes a really honest attempt to confute the philological arguments against its authenticity, but his deep attachment to "this beautiful inscription" gives its peculiar forms of speech such a wide margin as to leave the thinking reader with the impression that had the inscription been in Chinese, Hagen would have let it pass as good Latin! But more of this below.

It is striking not only to the Scandinavian scholar, but to any ordinary reader of the Kensington inscription that this living document can be very easily read when transcribed in the Latin alphabet. But this is not the impression one gets from the language of the 14th century

**FIGURE 20. The Inscription on the Kensington Stone**

(We were) 8 Goths and 22 Norwegians on a journey of exploration from Vinland to the west. We had (our) camp by two skerries one day's journey from this stone. We were out to fish (fishing) one day. Arrived home we found 10 men red with blood and dead. A V M (A Ve Maria), free (us) from evil. (We) have ten men by the sea to look after our ships 14 days' journey from this island. (In the) year 1362.
which is somewhat incomprehensible for the ordinary Scandinavian reader. That is why Hagen looks in vain for writings by Scandinavian specialists about this stone. They do not merely believe and feel that this inscription is impossible, but know it for certain in the same way as an Englishman would know at once that an inscription in modern English with a few old-fashioned forms added (which moreover combine words of different gender and case) could not belong to the 14th century. To show how every Scandinavian scholar regards the inscription I will quote what Professor Jon Helgason, professor in Icelandic at the University of Copenhagen, said to me when he read my first article on the Kensington Stone: “In my opinion the inscription on the Kensington Stone is such that no philologist with any self-respect could in any decency write about it; any more than an archaeologist would trouble to publish a grave-find of the Iron Age if he found a telephone book under the urn.” In my heart of hearts I agree with Jon Helgason. On the other hand there has been so much fuss made about this inscription that a stop must be put to it.

In the language of the 14th century we expect to find, e.g., “mi hafðum” instead of the modern “mi hade” (we had), which is on the stone, or “mi varum, kommum, funnum” instead of the stone’s “mi var, kom, fann.” In general we expect to find inflected endings in both verbs and nouns. However, we do find—quite sporadically, it is true—entirely modern uninflected forms as well (e.g. “kom” for “kommum”—we came, etc.). The only 14th century inscription which shows complete consistency in this matter is—that on the Kensington Stone.

There are a few other small details which—to put it mildly—are offensive to the ears of a Scandinavian philologist, and I shall mention them because Hagen has now made a noble attempt to justify them. If the interested reader takes a look at the copy of the inscription he will find that “from this island” is “from pëno ðh,” in another place he finds that “from” is also “fro.” Hagen writes: “In an older stage of the inscriber’s dialect both fra and fram (with short a) must have existed side by side; but since both forms had exactly the same meaning, fram must (!) have yielded to pressure from the more dominant (!) fra, thus becoming fram. When a became open ð, fram would have become (from) just as ‘fra became (fro:).’ This argument is really strikingly effective also because the same sound shift did not take place in the word skjar (which Hagen translates by shed) and ahr (year), just as the development fram > fram > from > frem must be designated as striking. As far as pëno is concerned (this), it is the only inflected word in the inscription. Unfortunately this form, which is a newly developed form of the neuter in the dative singular, here qualifies a word (uninflected) (ðh) which is feminine, and elsewhere in the inscription is connected with a masculine word pëno sten (which is nevertheless not in the dative); in genuine 14th century language these combinations would be: þessum stein and þessi eyju. Finally the last of the three small words is completely impossible in the language of the 14th century, ðh, with h to indicate length, since this phenomenon is considerably more recent than the 14th century. Hagen is extremely off the track when he tries to explain this h as the best proof that the inscriber of the stone was a specially fine phonetician, since this word in Danish certainly, but never in Norwegian or Swedish, had an “aspirated off-glide.”

We could keep on like this; but we will spare the now impatient reader further philological explanations. Let us pretend that the language is in order; without a qualm we will take it for granted that the inscriber, who, as the diagrams show, knew both the runic alphabet and the Latin alphabet (to some extent), has forgotten everything he learned at his monastic school about the rules of spelling which he must have known at one time and that he wrote as he spoke. This is what the supporters of the inscription would have us do, and we will forget that this supposition is not able to explain the linguistic anomalies either, even if we are willing to admit that language is a strangely lively fish which it is given to few armchair philologists to grasp. We will for the time being turn our backs on the language and consider the runes, to see if the solution to the problem may not lie with them.

Runes ceased to be used in Denmark and Norway about 1300, but they survived in Sweden on grave-stones and on household goods, not to mention the runic calendars which continued up to the 1700’s. But it is actually true of all medieval runic inscriptions whether of Denmark, Norway, or Sweden, that there is an even development to be traced from the earliest Viking times right up to the latest runic inscription, not only in the forms of the runes themselves but also in the language. The Kensington inscription does not fit into the unbroken chain of Scandinavian runic inscriptions. Look at the alphabets and see for yourself that almost half the stone’s runes have shapes which are not to be found in the runic alphabet of the Middle Ages.

It is however a fact that runes, as mentioned above, were retained right down to the 18th century in the Swedish runic calendars in the possession of the common man in many Swedish districts. And if we examine these we find that those of the 16th and 17th centuries employ very degenerate forms of the runic alphabet—forms which are not unlike those on the Kensington Stone. Therefore there can be no doubt that the person who engraved the Kensington Stone constructed his alphabet on the basis of the alphabet of such a Swedish runic calendar, and this is completely corroborated by the symbols which he used for numbers. They are those usual in runic calendars (known since the 14th century) but completely unknown in the general run of runic inscriptions. The engraver has not slavishly followed the alphabet of his runic calendar but has invented new symbols. In the formation of these new symbols he has offended against the system of the runic alphabet in ways we shall not go into here. The patient reader interested in the Kensington Stone will have already noticed that it is now in rather a precarious position. But it has not received the coup de grace. Here it comes. In his eagerness to have as complete an alphabet as possible the engraver of the Kensington Stone has invented a j-rune. He ought not to have gone as far as that. The fact
is that the letter "j" is a development within the Latin alphabet (like v). Both these letters were invented by the French philosopher Petrus Ramus in the 16th century. He took the letters jod and vau from the Hebrew alphabet and supplied the Latin alphabet with these two sadly needed consonants. In Scandinavian and German jod kept its name, while vau became vee in Scandinavian and English. Only the Germans have kept the old Hebrew name vau (fau), just as striking is the letter o, with the two dots above it, on the stone. This trick was introduced into Sweden about the time of the Reformation. In other words we have before us a rune-stone which used symbols—j and o—which were not invented until c. 1550, and the stone is dated 1366!

Now all the linguistic objections are on a firm footing: now we really understand the forms like vi hade instead of wi hafdam; now we realise how a word like opdagelsesaerd, impossible in the 14th century, can be found in the inscription (opdage, discover, meant oplyse, enlighten). With our eye on the American language we nod in recognition to words like ded (dead) and from.

But is it possible that there may be still a skeptical maintaining that Petrus Ramus—by thought transference—got the idea for the letter j from the author of the Kensington Stone inscription. Leading mineralogists have really accounted for the weathering and patina of the stone. But Professor Brøndsted's examination has a crushing reply to this. He has proved that an h carved in the stone by Holand about 40 years ago has already taken on a certain patina. And that in spite of the fact that in these 40 years the stone has not been acted upon by the wind and weather but has been kept in a sheltered room. Farewell, Kensington Stone of 1362, farewell Paul Knutsson expedition which perhaps never even started and which very likely never got to America; at any rate farewell to all the fruitless labours of scholars of nearly every branch of learning.

Apart from the battering Sven B. F. Jansson gave the Kensington Stone inscription both in general and in detail in his above-mentioned article, it is interesting to note that he shows how Holand in his massive books seems to have suppressed important material which argues against the authenticity of the stone.

From information I have received from Prof. J. A. Holvik, Moorehead, Minnesota, it appears that not even the archives of the Minnesota Society were properly examined by Holand. Prof. Holvik here discovered a document which must be considered as no less than sensational in this connection. It concerns a sheet of paper covered with runes and is apparently the engraver's rough draft for the inscription on the Kensington Stone. Holvik, who published it in the paper The Concordian, no. 10, Nov. 18, 1949, accompanies it with the following words: "This sketch of the Runestone inscription was sent in a letter by J. P. Hedberg of Kensington, to Swan (Swen?) J. Turnblad of Minneapolis. This letter is dated January 1, 1899. Mr. Hedberg calls the sketch an exact copy of writing on a stone brought to him by Olof Ohman. Both the letter and the sketch were filed in the archives of the Minnesota Historical Society in August 1925. (Holand's first book on the inscription was published 1932.) I found it there last month. A detailed comparison of the individual characters and spelling of each word with those on the Kensington Stone shows that this sketch is not a copy of the inscription on the stone. It is an original preliminary draft of the story later inscribed on the stone. With reference to Figure 22, which is a reproduction of a photostat copy sent to me by Holvik, I emphasize in agreement with him that in Figure 22 the word "from" is written fro in the first line and from in the fourth; the stone has here only fro. (2) Figure 22 has the word "red" written with h-rune (røde). The stone has råde. (3) Figure 22 spells the word "blood" as bjud, the stone has correctly blod.

3. This j is one of the details that exposes Holand's lack of knowledge in most elementary things, since he is pleased to refer to a couple of Norwegian diplomas which in his opinion show many examples of j. These are in fact not j's but long i's which are known to have been used from the Middle Ages up to more recent times as a sort of graphic (but not phonetic) variation, a principle which is shown for example in such discoveries as "vi" (we) written "wij," or the Roman numeral viii (VIII) as viij, without anybody, who has some knowledge of old writing, dreaming of talking about a letter j (a consonant—or semi-vowel) without a distinct sound value.

4. Hagen attempts to explain these words plausibly, but unsuccessfully.

5. Holand succeeded in giving a sort of historical background to the inscription. He produced a letter from the Swedish king Magnus Eriksson dated 1354. In this the king commands the Norwegian noble Paul Knutsson to fit out an expedition to Greenland, there to restore declining Christianity. Holand now imagines that the Paul Knutsson expedition arrived in Greenland, but, not finding the Norsemen there because they had either been killed off by the Eskimos or else had emigrated to Vinland, took the expedition to Vinland (America) to find the backsliders and drag them back to the Kingdom of God by the hair. Hence the inscription on the Kensington Stone is a record of what happened to some members of the expedition and a warning of what Fate had in store for the rest.
The sketch is older than 1899; at some time before this year (and no doubt in the autumn of 1898, when the stone was found), it was given to a man (Hedberg) by the finder of the rune stone (Ohman), who publicly had declared that he knew nothing about runes. Nothing is intimated of how far the sketch was undertaken by Ohman. But the spelling of tfhde compared with tghde (on the stone) cannot have been a transcriber's error by a man who knew nothing about runes, and that is evidence that the sketch is not a copy of the stone's inscription. This is stressed and strengthened by the two other examples. Notice also that tep (dead) was first written tpo, but corrected to tep. No reader, whether layman or not, could transcribe the stone’s regular and correct o-rune in blod by the complicated o-rune. But if the paper is not the copy, then it must be the original!

In this connection I must draw attention to a book, some of whose pages have been copied photostatically and the copies sent to me by Prof. Holvik. These are extremely interesting. The Book is called: Den kuns-

kapsrike Skolmastaren eller Hufvedgrunderna uti de for ett borgerligt samfundslyb nodigaste Vetenskapen, by Carl Rosander (The Well-informed Schoolmaster or the Fundamentals of Popular Science), 1893, new edition 1893. On the title page is Ohman’s signature and “Kensington 2.3, 91.” On pages 63ff is to be found an account of the Swedish language and its development. An example of the Lord’s Prayer of c. 1300 ends with the words: froelsae os af illu; here can be found the spelling ok and og (and), here even can be found h to lengthen vowels, and remarkably enough the spelling rohd (red).

How these remarkable coincidences are to be explained is naturally difficult to say. But it must be admitted that the consensus of opinion among Scandinavian scientists is that if there was in America at the end of the last century a Scandinavian with no training in philology but who had dabbled a little in books on popular science, and if he had had the idea of making a runic inscription, then that inscription would take on the same appearance as the one on the Kensington Stone.
In the Bulletin of the Massachusetts Archaeological Society, Volume 12, Number 3, April, 1951, Howard R. Sargent published, under the title, "A Polynesian Adze from Martha's Vineyard," an adze alleged to have been found at Oak Bluffs. He also figures another Polynesian adze after Moorehead from a Rhode Island collection. The Moorehead adze is, without question, from the Cook Islands while the Oak Bluffs specimen may be from the Cooks, the Marquesas, or possibly, Tahiti.

Members of the Archaeological Society may be interested to know that the occurrence of Polynesian adzes and other material from Oceania in collections of New England Indian artifacts is not at all unusual. In the Rhode Island archaeological collection in the Museum of the American Indian: Heye Foundation, there are four especially fine Cook Islands adzes that were labeled, "Rhode Island Indian," until a few years ago, when I pointed out their proper provenience to the curator there. One lot of archaeological material from Essex County which came into the Peabody Museum of Salem in the mid 1930's contained a rather nice Hawaiian adze blade. On several occasions, in the miscellaneous archaeological collections of historic societies, I have seen Polynesian adzes mixed in with the American Indian material. Almost always they are with material from coastal southern New England. Nor are adzes the only Polynesian things that get mixed in with local collections. A few years ago in the little private museum in Dover, New Hampshire, I was looking over a case of New Hampshire Indian specimens. Right in the middle of the Indian collection was a perfectly good bonito hook from the Marquesas Islands. Marquesan fish hooks are not common even in collections which specialize in Oceanic material but here was an excellent example of one. Polynesian things, for the most part, are so distinctive that, not only can they be readily recognized by one experienced in handling them as being Polynesian, but usually they can be identified with a particular group of islands and, sometimes, with a particular island. Thus, Marquesan bonito hooks superficially resemble those from the other Polynesian islands but the details of construction are sufficiently distinct so that one can always tell whether a bonito hook is from the Marquesas, the Hawaiians, the Cooks, the Societys, or some other group. One of our Marquesas bone tikis, a tiki being a stylized carving of a human figure, was taken from a well in Wiscasset, Maine.

Probably other than adzes the most common extraneous material from the Pacific are strings of beads from Micronesia. On numerous occasions these have come into museums, with collections of local Indian things, labeled as wampum. On one occasion I had a string of Marshall Islands beads come in thoroughly documented as being Mohegan Indian from Connecticut. There is some resemblance between Marshall Islands beads and the so-called grave or disc wampum of the local Indians. In general, however, the Marshall Islands beads are smaller and usually alternate shell discs with discs made of tortoise shell or coconuot shell. Fijian clubs that are occasionally decorated with wampum like beads, and clubs from other islands, have sometimes been given as Indian clubs or tomahawks. Even tapa cloth, which could hardly be more distinctive of the eastern Pacific, has been brought in as American Indian. Recently, a canoe model was sent me from southern Massachusetts where it had been dug out of a cranberry bog. The owner said that the local tradition was that small canoe models of this kind were built and raced by negroes from Africa who worked in the cranberry bogs in the early days. That may be so, but the model sent me was a typical one from the Samoan Islands.

Sargent, in his article, correctly infers that the presence of his adze and other material of this kind can be explained by the whaling industry which, for over a century, brought a wide knowledge of the Pacific to New England. This point, however, needs a little elaboration. It is well known that Polynesian material is scarce anywhere and the fact that it occurs in New England more frequently than other places is due to the early contacts of this region with the Pacific. These contacts, however, were not confined to whalers. Polynesian material still shows up in shops and private homes throughout Massachusetts, Connecticut, and Rhode Island. South of Boston most of this material probably came back on the whale ships. The islands of the Pacific were visited by these whalers, on their long three and four year cruises, for water, fresh vegetables, and for repairing their ships. North of Boston whaling was not an important industry but Salem and Boston vessels were engaged in extensive trading with the northwest coast and with the islands of the Pacific where they obtained sandalwood and beche-de-mer for the China trade. It was also from north of Boston that many of the first Missionaries to the Hawaiian and other islands were sent out by the American Board of Commissioners for Foreign Missions. Thus, the occurrence of Polynesian specimens in considerable quantity in New England is due to the local demand for whale oil, the Chinese demand for sandalwood and beche-de-mer, coupled with the fact that it was usually impossible for a vessel sailing to the northwest coast to obtain a full cargo of sea otter skins for China. The Christian zeal for spreading the gospel to the heathens also had some bearing on this.

Archaeologists who wish to check suspicious adzes in their collections will find good illustrations and descriptions of Polynesian adzes in "Arts and Crafts of the Cook Islands," (Bulletin 179, Bishop Museum) by Peter H. Buck, and other bulletins of the Bernice P. Bishop Museum. There is also a good account and many figures of Polynesian adzes in "The Moa-Hunter Period of Maori Culture" by Roger Duff, Wellington, 1950, pages 138-198. The bibliographies of these works will also lead those interested to more detailed descriptions of adzes from particular island groups. Adzes and other
During the spring of 1951 members of the Northeastern Chapter collected chips, stone artifacts, pottery and such from the Bull Brook Camp site, the location of which had been known for some time. Previously the late Mr. Roy L. Esty had collected a number of artifacts there. In early June 1951 Eldridge and Vacaro showed the site to Messrs. Byers and Johnson of the Peabody Foundation at Andover. Because of the interest of the artifacts which had been collected, it was decided to publish the following account. Some of the data included here has been supplied by Byers and Johnson.

The Bull Brook site, M13/36 in the Society's site catalogue, is located on a spit of land less than one half mile wide forming a divide between Bull Brook and Muddy Run, the two upper tributaries of the Rowley river. The spit is composed of a deep layer of water-laid sand in which occasional boulders are to be found. The surface of the area upon which the artifacts are to be found is flat, having a relief of less than an estimated five feet. It is about 40 feet above sea level. This flat surface is covered with sparse vegetation, grasses and scattered trees growing in a deposit of sandy humus some eight inches thick.

A number of years ago when the site was first discovered the northeasterly end was numbered M13/36 and surface indications were that the area was restricted. Since that time a large sand pit and rock crusher have been located on the spit and stripping operations have revealed evidence of occupation distributed in a general southwesterly direction for more than 500 yards. The artifacts are concentrated in small areas, some of them on the sloping sides of the spit.

The bulldozers operating about the sand pit have stripped off the layer of humus and the excavations for sand have removed all of the original M13/36. From all that has been observed to date, however, it seems likely that further work, though it will probably produce interesting and significant artifacts, will not add to our knowledge of the stratigraphy of the site. As far as is known all the artifacts are found in the humus or in the transitional zone between the humus and underlying sand. Because of the manner in which the sand was laid down, it is impossible to conceive of a way in which anything but its present surface could have been occupied. The inferred process of deposition is that during successive occupations the materials which were left behind came to rest on a surface of the humus which had been laid nearly bare of vegetation and partially eroded by the ordinary activities of the people. Upon being abandoned, the artifacts were covered up by the vegetation and by deposits of the very light humus which drifted about the site. Successive occupations deposited tools and chips in this humus in the same manner so that their location can give little or no clue to the stratigraphy. It is possible that very careful mapping of concentrations of artifacts might provide significant data, but the size of this task and the uncertainty of success most precludes this as a possibility. This kind of work is rendered further uncertain by the probability that the area has been cultivated at some time since it was occupied.

In spite of these discouraging remarks the site is worthy of record. The fluted arrowpoint excavated by Vacaro from a surface laid bare by a bulldozer is the best record of such a specimen from Massachusetts to date. Adding to this a similar arrowpoint found by Mr. Esty heightens the interest. Unfortunately, this arrowpoint cannot now be specifically identified in Mr. Esty's collection. One fragment of a fluted arrowpoint and one small arrowpoint not so easily classified as a fluted point were kindly loaned for study by Mrs. Esty. They are labeled "Folsom Points" and on a slip in the box "Ipswich" is written. They may have come from this site. The presence at this site of "sheared" tools, though apparently not unique in New England, is also of significance. In view of this we offer the following description of the tools which have come to light during the past few years.

**ARROWPOINTS**

One arrowpoint, Fig. 23, no. 37, Fig. 24, no. 7, is of particular interest. The size (2 1/2" x 1" x 1/4"), shape and the channel which is to be found on both faces places it within the general group called Eastern Folsom. It is of the smaller variety. The arrowpoint is somewhat crudely made of a hard, fine-grained "cherty" stone. Apparently the edges were retouched by the removal of narrow, horizontal flakes before the channel flake was removed. In a few instances, however, especially near the butt, the flake scars do not reach as far as the channel scar. On one face the concave base has been retouched following the removal of the channel flake. On the other face the channel flake came off in two pieces, apparently due to a single blow or application of pressure at one spot. This channel scar is more irregular than that on the other face. The edges near the butt have not been polished. One of the corner points at the butt has been broken off, but it is impossible to say whether or not this occurred while it was being made.

The other arrowpoints in the collection require only brief comment. All the forms shown, Fig. 23, nos. 28-36; 38-43, are commonly found in Massachusetts. No. 42
had a notched or bifurcated base, but one corner of this has been broken and it does not show clearly in the illustration. With the exception of the arrowpoints made of quartz, nos. 30–33, these arrowpoints are most frequent in non-pottery horizons, but they occur, actually in probably significant percentages in association with various kinds of pottery some of the latest of which may be the New England counterpart of New York Owasco. The four quartz specimens are of interest because Ipswich is close to the northern boundary of their occurrence, especially in the forms seen. This kind of arrowpoint, along with others, is found in large relative numbers south of Boston, Massachusetts, in Rhode Island and eastern Connecticut. Nos. 30 and 32 have been called Lanceolate and are slightly more frequent in earlier pottery bearing horizons. The triangular form, no. 31 is a rather crude example of a type found in the same general horizon. No. 33, trianguloid, stemmed, has a similar distribution.

In all twenty-three arrowpoints from the site may be described. In addition to the sixteen illustrated, two are duplicates of nos. 32 and 35, although not made of the same material. One broken piece has a slightly concave base and crude side notches. Another is a broken arrowpoint, the base of which has been retouched for use as a scraper. The three other pieces are probably sections of long, narrow forms. These specimens belong in the same general horizons as those which are illustrated.

As a whole, excepting the eastern folsom specimen, the arrowpoints belong in a group which is as yet only vaguely defined. The group is not the most ancient known in New England for it appears to have been most commonly employed by the people who were just learning to make pottery. Cultural affiliations and the time horizon of eastern folsom arrowpoints are as yet unknown. Here we only record the discovery of this specimen in a location which defies stratigraphic interpretation. We do not know whether it antedates the other specimens, or whether it was made by people who also used the more commonly distributed types.

**SCRAPERS**

Figure 23, nos. 16–26, illustrates a series of scrapers such as are commonly distributed in Massachusetts. Excepting no. 19 this particular group is made up of relatively thick pieces of chert or other fine-grained rock. All of them have one flat or slightly curved face. The edges at the top of the specimens as illustrated have been dressed by the “vertical” chipping characteristic of scrapers of this sort. On some of the specimens a “side” has also been retouched in similar fashion. No. 19 is made of a thin flake the edge of which has the characteristic vertical chipping. It is singled out here because of an impression that thin scrapers of this sort may be characteristic of cultural horizons, especially to the north and east. No. 26 has been made of a particularly refractory piece of stone. Part of the upper surface is flaked off, probably due to frost. The vertical chip-

ping, although probably done by pressure is irregular perhaps because of a flaw in the stone. The sides of the specimen are dressed. Apparently the specimen was first “roughed out” and then the edges sharpened by removing smaller flakes.

Nos. 17 and 27 are forms of scrapers which are either rare in Massachusetts or not frequently described. One face is flat. The convex faces seen in the illustration have parts of the original surfaces of the pebbles from which the tools were made. Various edges have been retouched so as to produce a form of vertical chipping. There is a question whether the blunt point of no. 27 (possibly a similar point was broken off no. 17) was purposeful. We are inclined to think it was not, for the smooth end of the point may be the outer surface of the original pebble.

**SHEARED SPECIMENS**

To our knowledge the occurrence in New England of tools sharpened by a technique called “shearing” by Barbieri has not been previously recorded. The technique is described as follows. “Some material, notably obsidian, flakes into very thin, fragile edges. Such edges develop in other materials during the process of pressing off thin flakes. They have not sufficient solidity to produce a sizable flake, but break off short at the slightest pressure. So the workman shears them off by running the side of his flaking tool across the thin edge at an angle nearly parallel with the face of the specimen, pressing firmly as the tool sweeps along the edge. This shears off the useless thin zone and produces a slightly blunted edge against which the flaking tool can find a proper ‘bite’. The working edges of these scrapers, which are incurved ones, have been trimmed by this trick of shearing, giving them the appearance of having been very finely and evenly flaked. Except for the percussion strokes which blocked them out and shaped them, these implements show no other workmanship. They could be made easily and quickly, and probably were soon discarded.”

The stone industry of New England is notorious for the crudeness of its product. Much of this is due perhaps to the type of stone selected by the aboriginal artisans. The commonest materials are coarse-grained rocks such as the porphyritic rocks, poor grades of quartzite, jaspers and the like. Chaledony, flint and rocks of similar properties are present although not abundant in the region. However, tools made of such stone are relatively rare except at sites which apparently were near some source of supply.

The distribution of the shearing technique in New England may well be somewhat modified by the kind of stone used by the Indians. Although distributions suggest it, they do not prove the idea that Indians of the region had developed habits in using certain rocks, especially varieties which appear to us to be more refractory than other kinds immediately available. Only

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infrrequently did the people use the cherts and other “better” stones. This employment of coarse-grained rocks is reason for some uncertainty in identification of the shearing technique. It may have been employed and its distribution may be rather general, but we cannot be sure at this writing. For example, no. 12 in the illustration is a flake about 1/8 of an inch thick. The upper edge has been finely retouched in a manner suggesting shearing. However the irregularities due possibly to differential fracturing of the crystals are reason to doubt the identification. Specimens which are sheared are numbered 9, 10, 11, Fig. 24, 5, 3, 1. In the drawings an attempt has been made to indicate the characteristic regularity of the tiny flake scars and the lack of serrations along the edge. It is also evident that the flaking is usually at a steep angle analogous to the vertical chipping on thicker scrapers.

Tools numbered 1, 13-16 are probably sheared. Nos. 1, 13 and 14 are 1/8 inch or less thick, but the retouched edges are somewhat more irregular than those of nos. 9, 10, 11. The rounded end of no. 14 appears to have been dressed by pressure retouch before having been sheared. There is some doubt concerning nos. 15 and 16 both because the chips from which they were made are thicker and because the retouching on the edges has removed larger irregular flakes than is usual. It is to be admitted that segregating small differences of this sort may be unwise, but until we are more familiar with the results of the technique as applied to local rocks, it is perhaps of value to point out such minute variations.

This description and record of a number of sheared tools from a single site leaves an impression that the specimens have unusual characteristics. Actually nothing is further from the fact. It is probable that the principle reason for the present lack of record of shearing in the Northeast is the fact that the very fine retouch is not easy to identify and, furthermore, it is usually found on chips which are ordinarily discarded. Inspections of collections of chips from various places in the Northeast are bringing to light an increasing number of examples of this type of retouch. For example, Dr. E. E. Tyzzer found on the Smith Farm in Lynnfield, Massachusetts, jasper flakes which had been retouched in this manner. At this site there were also a number of triangular arrowpoints suggesting that the site was of relatively recent date. Other examples have come from sites scattered about New England, and to the east. For example, T. L. Stoddard, Jr., of the Peabody Foundation’s Northeastern Survey, excavated a number of specimens from sites in New Brunswick. A few examples have been found in the collections from the Nevin Shellheap, Blue Hill, Maine. It is not possible to judge the significance of the observation that the technique has not yet been identified in the collection of more than 3000 artifacts from the Titicut site, Bridgewater, Mass. In view of our slight knowledge of the vertical and geographical distribution of the technique in the region, it is not possible even to suggest the time when it was employed or the cultural affiliations of the people who used it.

Nos. 6 and 7, Figure 24, 4 and 6, are unusual in this region. Mainly by shearing, a sharp point has been formed on one edge of these very thin flakes. Although quite small, the retouching on the “sides” of the projecting point can be easily seen. No. 8 may have been a similar tool. The base of a small projection is present on the straight edge. The curved edge at the bottom in the photograph has also been sheared.

SMALL BLADES

The blade illustrated, Figure 23, no. 2, Figure 24, 2, is a type which is uncommonly reported from Massachusetts. It is roughly prismatic in cross section. The end has been trimmed to a sharp point by the removal of small flakes. Technically this tool is not a burin or a graver, but it could be used as such, particularly the latter. The other three specimens, nos. 3-5, are flakes of similar nature, although they are not as thick as the first one. They are not retouched in any way and it may only be suggested that they are either tools or evidence of the presence of a blade industry. It is entirely possible, of course, that these are accidental, having been flaked off during the process of dressing down a block to make an arrowpoint or other tool.

GORGET

A broken gorget is illustrated, Figure 23, no. 51. This is a crude example of this kind of ornament in which two holes may be identified.

POTTERY

The potsherds illustrated, Figure 23, nos. 44-46, 49-50, 52-57, have been selected to represent varieties seen in the whole collection of more than twenty pieces. Some of this pottery, nos. 49-50, 52-54, is mineral tempered, with cord wrapped paddled inner and outer surfaces. Nos. 45, 46 are rim sherds. No. 46 has a flat rim in which punch marks have been made. The other rim is rounded. The outer surface of both these sherds appear to have trailed lines on them. The pieces are too small to identify these as decorations. The inner surface of these sherds has been smoothed, the technique cannot be identified. No. 54 is a sherd of a thick, mineral tempered pot, the inner and outer surfaces of which had been smoothed so that no tool marks are to be seen. One imprint of a cord wrapped stick may be seen on the outer surface. Nos. 56 and 57 are sherds showing dentate, rocker stamping.

EUROPEAN SPECIMENS

A fragment of a clay pipe, no. 47 and a large glass bead, no. 48 were found on the site. The pipe fragment is too small to attempt identification. The bead is certainly an old one and of a type traded to the Indians during colonial times.
FIGURE 24.

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