

THE CONVENTIONAL RADIOCARBON LABORATORY IN UPPSALA AND ICELANDIC ARCHAEOLOGY

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ABSTRACT

Incorrect statements made in a paper by an Icelandic archaeologist *re* my research on the reliability of the ^{14}C results obtained from Icelandic samples are discussed and proper references given. Some dates are listed in a table.

Introduction

A paper on radiocarbon dating and Icelandic archaeology (Vilhjálmur Örn Vilhjálmsson 1991) was presented at *The Fifth Nordic Conference on the Application of Scientific Methods in Archaeology* held in Stockholm in 1990 and later published in the first part of the proceedings of the Conference. It contained so many erroneous statements that I feel obliged to accept the suggestion to write a comment for the second part of the proceedings. Vilhjálmsson has also published a paper - said to be essentially the same paper in Icelandic. I wish to discuss a few of the items in his paper related to my work. It is noteworthy that he has only two references to my papers. The first is to an extended conference abstract published in 1983. The results were presented in the corresponding lecture, given in 1982. The second is to a paper which he says is in press although not yet submitted in July 1992. That paper was still in the initial stage of preparation when Vilhjálmsson wrote his paper. It is noteworthy that I also presented, for example, in a lecture at the fourth conference in the actual series, held at Haugesund in Norway, many results on the lower ^{14}C activity of the atmospheric carbon dioxide over the Arctic islands than over northern Sweden, as measured on samples from Abisko. My paper, to be submitted in 1992, is intended to relate, in a popular way, the principles and problems of radiocarbon dating. It is based on a lecture which I was invited to give in April 1990 in Reykjavík as part of the series of *Ása G. Wright Memorial Lectures*. For the discussions, I brought my first draft (working material) with me to show to the editor. I do not know how Vilhjálmsson has got hold of that material. He has not asked for permission to use it and

I have not given him any. The title has been changed twice since then, a few minor errors corrected, some more references added, and the language improved. It seems as if Vilhjálmsson has read at least part of the paper. He has made a lot of statements in his paper (1991) *re* my work indicating that he has not read my draft and earlier papers as carefully as he should have done before making comments on other papers. Many of his statements are made without any references and there is simply no basis for them.

Reservoir effects

Vilhjálmsson (1991:104) writes that "Olsson has...given a more detailed explanation for the alleged influences of inactive CO_2 ... (Olsson in press)". What I have done is to give results indicating a lower activity over Iceland and other Arctic islands than over Sweden. Vilhjálmsson writes: "Olsson has collected relatively few samples near only 2 thermal and volcanic sources in Iceland. When dated, all of them gave quite higher ^{14}C values than expected." The fact is that, in Iceland in 1978, I collected 49 samples and several samples in 1971. Together with Icelandic friends, I collected even more samples in 1989 and 1991. I also received some samples from Icelandic friends. Several samples have not yet been measured. In the manuscript to be submitted in 1992 and in my lecture presented in April 1990, I included results, in diagrammatic form, on samples from Heimaey, Deildartungukver, Namaskárd, Namafjáll, and Krafla and in the text results on samples collected for me in connection with the volcanic eruption in 1973 on Heimaey. Results are also given for samples from Öfaerufoss, Kviárjökull, Dyrhólaey, and Thorsmörk. The samples gave lower ^{14}C activities than

contemporaneous Swedish samples. This was very pronounced for two atmospheric carbon dioxide samples collected on top of the new lava on Heimaey and the sample closest to the outlet of hot water at Deildartungukver. Vilhjálmsón has unfortunately not included any reference to Sulerzhitzky (1970) when on p. 104 he gives references to authors presenting a low ^{14}C activity due to volcanic eruptions. Vilhjálmsón claims that the effect is "limited to only a small radius around the source". Small is a very imprecise word in this discussion. Indeed, in the literature, results are given indicating that the effect can be detected at distances kilometres away from the source.

It is difficult to understand what Vilhjálmsón (1991:105) means when he writes: "...has influenced I.U. Olsson in her quest for explaining her hypotheses and unexpected ^{14}C results from Iceland". I wished to evaluate the reliability of the ^{14}C method - in this case for Iceland, since several facts indicated special problems. As early as March 3, 1980, I wrote to S. Thorarinsson that it seemed as if we had to question the information given in the Landnámabók. Similarly, K. Eldjárn and I agreed in a discussion on August 27, 1974, that I could very well question details in the Landnámabók.

Vilhjálmsón (1991:105) writes, without giving any reference for his statement: "Olsson has accepted the traditional date". The fact is that I carefully avoided publishing any statement, except that the ^{14}C dates have been regarded as being too old to judge from the traditional chronology based on the Landnámabók and that the conclusion based on this discrepancy must be that, for Iceland, the traditional chronology and the ^{14}C based chronology are both more or less in error (for example, Olsson 1980:48, 1983a, 1987a:22, 1987b&c, 1989a&b, 1990:214, 218, 221). I found it appropriate to investigate the possible sources of error in the ^{14}C dates. Very early, I thought of carbon dioxide trapped in glaciers as a possible contribution to a decreased activity. In 1971, however, on a geological excursion to Iceland, I started to consider a volcanic contribution, but later I realised that the global carbon dioxide cycle must be evaluated. I had measured many recent samples from Svalbard and Sweden and found that the excess activity due to nuclear-bomb tests remained slightly lower on Svalbard than over northern Sweden (Olsson 1989c). The initially large difference between the activity of samples from high northern latitudes and those from low northern latitudes and the southern hemisphere could, however, hardly be seen any longer. Since Lerman et al. (1970) had released a few dates on old, pre-industrial samples, indicating that the activity was lower in the southern hemisphere than in the northern there seemed to be a great possibility that the

reason for the results from the Icelandic samples was the same - a large surface-water area in comparison with the land area.

Because of the possible sources of error, I have, since 1971, collected Icelandic samples close to hot wells, in volcanic areas, close to the coast, far from the coast and in front of glaciers. I have also collected samples on the Faroes to avoid volcanic activity and glaciers, as well as on Greenland to avoid volcanic activity and to get samples from the very vicinity of glaciers. In addition, I have got samples collected by others on Iceland, Svalbard, the Faroes and Greenland. It will thus be seen that I have long agreed with Vilhjálmsón (1991:107) that the activity has to be "studied systematically and not at random". Vilhjálmsón, however, says that samples from the coast of Svalbard and the Faroe Islands cannot necessarily be compared with samples from Iceland. My opinion is that in this work it is very essential to compare results from different localities in the Arctic.

In my lecture given in April 1990 in Reykjavík, which was intended to be a survey of the radiocarbon method and my present standpoint on the question of the reliability of Icelandic samples, I did not give the details of the collection sites in order to give the audience a chance to get a fair picture of the problem. Many details will be published in the forthcoming scientific paper.

Collaboration

In his paper, Vilhjálmsón (1991) has written thrice that I have withheld a result from an archaeologist. On p. 107 he has even taken this, his own statement, as an example of a laboratory not showing "a sufficient will for cooperation". He did not suggest that the archaeologist and others receiving dates should get into contact with the laboratories before publishing the dating results to show their will for co-operation. This is especially remarkable in the related case, considering the conditions agreed on from the start and during the dating work. Unfortunately, no contact was made by the archaeologist for several years before the book was published, although it contained some appendices on investigations made by other scientists. The sample Vilhjálmsón refers to, was dated after the other datings were finished, to yield more information on possible errors when material such as driftwood is dated. To avoid the archaeologist being blamed for omitting two results, not available when the book was published, I wrote, in my draft for my lecture in Reykjavík, that one result had not yet been given to the archaeologist and another was not yet ready in April 1990. The second dating was made to prove that it is

U-No.	^{14}C age BP	$\delta^{13}\text{C}$ ‰ PDB	Material	Species acc. to subm. forms	Locality	Subm. by
U-2517	970 $\pm$ 60	-26.8	Charcoal	<i>Betula</i>	Smidjuskógur, Króksdalur	S.Th.
U-2718	960 $\pm$ 65	-24.7	Charcoal		Hrísheimar, S. of Mývatn	S.Th.
U-2827	900 $\pm$ 60	-26.1	Charcoal		Thraelagerdi, S.W. of Mývatn	S.Th.
U-2899 U-4299	1015 $\pm$ 70 1190 $\pm$ 100	-27.2	Charcoal	<i>Betula</i> *	Hrafneseyri at Arnarfjörð, S-15	G.O.
U-2900 U-4300	1070 $\pm$ 65 1325 $\pm$ 120	-26.4	Charcoal	<i>Betula</i> *	Hrafneseyri at Arnarfjörð, S-46	G.O.
U-4014	1090 $\pm$ 80	-26.3	Charcoal	<i>Betula</i>	Undir. Hellisbjargi, Papey	G.H.
U-4327	1110 $\pm$ 65	-25.2	Charcoal	<i>Betula</i> and/or <i>Salix</i>	Adalbol, Hrafnkelsdalur	S.Th.
U-4328	1030 $\pm$ 65	-25.0	Charcoal	<i>Betula</i> and/or <i>Salix</i>	Faxagil, Hrafnkelsdalur	S.Th.
U-6227	650 $\pm$ 65	-28.2	Charcoal	<i>Betula</i> *	Búdasandur, Hvalfjörður	G.O.
U-6228	345 $\pm$ 70	-25.2	Bone	Cow	Grunnasundsnes, Stykkishólmur	G.O.
U-6229	315 $\pm$ 80	-25.3	Bone		Nyjibær, Vestur-Eyjaflahreppur	G.O.

* Probably, according to the form.

Fig 1. Icelandic archaeological samples dated in the Uppsala Conventional Laboratory (except the two series from Reykjavík and Herjólfssdalur). G.H. = Gunnar Hoppe, G.O. = Gudmundur Ólafsson, S.Th. = Sigurdur Thorarinnsson.

important to remember the statistical laws in discussing results. Both results have now been given to the archaeologist and they will be included in the final version of the paper in the series of *Ása G. Wright Memorial Lectures*.

Wood species

In his abstract, Vilhjálmsón (1991:101) claims that "some radiocarbon laboratories do not ask for, or execute, a detailed wood-analysis prior to dating". He takes this statement as one of a number of "alternative explanations for the discrepancy", referring to the fact that "some of the ^{14}C datings do not match the conventional date for the settlement of Iceland". What has the practice of some radiocarbon laboratories got to do with the problems discussed when he stresses that the conventional radiocarbon laboratory in Uppsala has performed so many datings (Vilhjálmsón 1991:105)? On the forms from my laboratory to be submitted together with samples, I ask for the species. Vilhjálmsón (1991:106) writes that Nordahl (1988:113) has written that all the samples from Reykjavík were birch, but he also writes on the same page that "No extensive wood-analysis on the samples from Reykjavík have been published". I asked Else Nordahl to arrange for wood analysis of the samples, and this was done. Dr. Åberg did that on all her wood samples, except the first sample I dated (U-4030). From the tables in my *Ása G. Wright*

paper, it will be seen what samples from Iceland listed therein have been wood-analysed. It should be recalled that the statements made by Vilhjálmsón (1991:106) in this matter are misleading. He has no reason to say that "Five out of nine dated samples from Heimaey have, though, been determined in an acceptable way". The samples determined by Åberg were also carefully examined. I had frequent contacts with Åberg about the Icelandic samples. My opinion is that the archaeologists working with Icelandic material should be thankful for his skilful and dedicated work. There are some errors in the thesis by Hermanns-Audardóttir (1989). The information given by her on the wood analyses is not the same throughout her thesis (p. 45 and p. 126).

I have also in letters and printed papers stressed the importance of submitting wood-analysed samples - at least in certain cases (for example Olsson 1974, 1976, 1983b and 1989a&b). In the 1974 paper I even published the result of U-4014 and wrote that the sample consisted of charcoal of *Betula*. Vilhjálmsón writes in his table that it consisted of charred *Larix* and *Betula*. K. Eldjárn was worried about the first dates from the Stockholm laboratory on samples from Papey and he discussed the matter with G. Hoppe, who in turn discussed it with me. I recommended the selection of birch samples and after that the results were improved. Åberg separated the Papey sample, dated as U-4014, into *Larix* and *Betula* and I got the *Betula* part from Hoppe.

According to Tauber (1968:321), the sample used for K-1245 consisted of charred *Sorbus*, although Vilhjálmsón writes "charred birch". According to Nordahl, her sample used for U-2674 consisted of barley, but Vilhjálmsón writes "barley and other seeds". Several other errors are introduced by Vilhjálmsón in his table. Some, but not all, however, may be due to errors already published by some other archaeologist. I have therefore, in the *Ása G. Wright Memorial Lecture*, given the correct results and pertinent information for the samples from Reykjavík and Herjólfssdalur, Heimaey. The other dates from my laboratory on Icelandic archaeological samples are listed in this paper (fig 1), as far as I have been able to trace them without too much effort.

Laboratory process

How could knowledge of the expectations of the archaeologists affect the physical measurements? If the island-effect theory is correct, we do not need to worry about "Dissolved carbonate...to come in with the westerly and northerly winds", as Vilhjálmsón (1991:105) does.

The working conditions in my laboratory were, for a long time, such that I had to charge for average material costs; a price which would be equal for all samples but did not cover my own or my technicians' salaries. I had no money for wood analyses, since these are not necessary for all samples, and people can sometimes do them themselves or may wish to arrange these analyses themselves.

Conclusion

Vilhjálmsón has not a single result on geophysical or geochemical samples to support his hypothesis that I am wrong in reporting an island effect in the Arctic. I have more than a hundred sample results, given in diagrams, in the *Ása G. Wright* paper, indicating a slightly lower activity on the islands discussed than in northern Sweden. The consequence is that any calibration must be made considering the regional deficiency of ^{14}C of the atmospheric carbon dioxide. The result following this geophysical approach was used to discuss the dates, from the Uppsala Conventional Laboratory, of Icelandic archaeological samples from Reykjavík and Heimaey. Extensive wood analyses were made on the samples dated in Uppsala with very few exceptions. In his paper Vilhjálmsón demonstrated how dangerous it is for an archaeologist to criticise a natural scientist without sufficient knowledge and without reading the appropriate papers and to base his criticism on speculations. The intention declared when this series of Nordic meetings was initiated and started in

1976 with a conference in Uppsala must not be forgotten. We said that we needed a forum for the discussion and explanation of new methods and improvements and the exploration of hitherto unknown limitations of already established methods.

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