

THE UNIVERSITY OF CHICAGO

CHICAGO 37 · ILLINOIS

INSTITUTE FOR NUCLEAR STUDIES

May 2, 1951

Professor G. P. Harnwell, Chairman
Randal Morgan Laboratory of Physics
University of Pennsylvania
Philadelphia 4, Pennsylvania

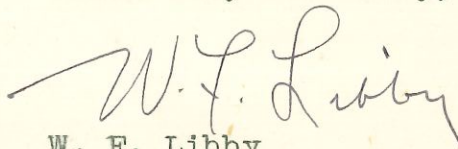
Dear Professor Harnwell:

The situation with young men taking degrees in radiochemistry this year is extremely tight, and it would be possible to place five men for every one we have; and as though this were not a sufficiently difficult situation, the draft deferment adds complications. I am completely unable to suggest anyone else to you. I shall bear it in mind and if any possibility develops I shall certainly write you immediately.

If you find anyone I would be pleased to teach him what we know of the technique, but I must say that I plan to be in California all summer.

I remain

Yours very sincerely,



W. F. Libby

WFL/lm
cc: Dr. Rainey

Prof William Libby

Dr Hans. E. Suess

Institute for Nuclear Studies

University of Chicago

Chicago, Ill.

do call Chicago arrange for visit.

visit Chicago for work before June 26 [1951]

write Rad. Counter Lab.

list

→ counter: ^{circuits} Rad. Counter Lab 8-10 sample counters

→ rim

→ 3 mech pumps

→ 2 diffusion pumps

3 manni gauges

argon & ethylene cylinders

→ oxygen tubing 60

→ stainless steel tubing Central Steel & Wire

→ analytical balance

crude balance

3 nichrome hot plates

brass burner

September 17, 1951

Dr. W. F. Libby
Institute for Nuclear Studies
University of Chicago
Chicago 37, Illinois

Dear Dr. Libby:

I hope it is still convenient for you to have me visit your laboratories this fall to learn about your carbon fourteen age determination process. I am wondering when you would like to have me arrive. Unless another date suits you better, I shall plan to come to Chicago on 8th. *of October.*

Most of the orders which I placed for the University of Pennsylvania in June, including the iron for the shield, have been filled, so, at the moment, I am optimistic about our progress.

Sincerely yours,

Elizabeth K. Ralph

THE UNIVERSITY OF CHICAGO

CHICAGO 37 · ILLINOIS

INSTITUTE FOR NUCLEAR STUDIES

September 24, 1951

Miss Elizabeth K. Ralph
Randal Morgan Laboratory of Physics
University of Pennsylvania
Philadelphia 4, Pennsylvania

Dear Miss Ralph:

Mr. Libby asked me to thank you for your note of September 17, and to tell you that he is looking forward to seeing you here on October 8. This date is entirely satisfactory to him.

It occurs to me that you may not know that we have moved our offices from Kent into the new Research Institutes Building. The address is 5640 Ellis Avenue, Room 442. The phone is Midway 3-0800, Extension 2481.

Sincerely yours,

Lucille McCormick

Lucille McCormick
Secretary to W. F. Libby

c/o Dr. W.F. Libby
5640 Ellis Ave., rm 442
Chicago 37, Ill.
October 11, 1951

Dear Dr. Stephens,

Yours and Professor
Harnwell's hunches about
Libby's equipment not being
ready were correct, for
his counting equipment is
not yet completely rebuilt.
When it was moved this
summer, the iron shield
fell on the counters and
circuits, so they had to
be rebuilt. The chemical
processing equipment, however,
is in operation, so I have
been observing one of Libby's
technicians running samples
through. Dr. Libby went

to Washington on Wednesday²
and thought it a good
plan for me to run
one of ours through while
he was gone. I called
Dr. Coon in the Museum
and he sent a sample
which arrived this morning.
Unfortunately, I didn't meet
Dr. Coon before I left. I had
asked Dr. Rainey about
samples, but, evidently,
Dr. Coon has charge of
them.

I acid-treated the
sample today and hope
to have half of it converted
to radio-chemically pure
carbon by early next week -
it's a lengthy process.

I visited Radiation
Counter Labs. yesterday
morning. It is possible

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for them to complete our equipment within a month - they have the materials, but I suspect they won't because they are going to move their plant in the interim.

Dr. Suess' equipment (He ordered it for the Geodetic Survey at the same time that we ordered ours, but he had a D.O. number) is almost completed. If they don't find too many "bugs" in his, ours will follow fairly quickly and we will benefit from their experience with his. They have made quite a few changes in the screen-wall counter to make it easier to manufacture and also to use. Most of the changes are good, but I am

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insisting that they use silver solder (as specified in Libby's drawing) which they don't like. They have made Suess' with soft solder. I suspect that they haven't used low lead-content brass, but I see no reason to add lead in the form of solder. They want to chromium plate the counters. Libby thinks that would be good. I may be unduly suspicious about the solder, but I have ordered one with plating and one without. If you think this is a mistake, please let me know. I don't think that RCL fully appreciates the lowness of the radioactivity that they

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will be used for.

RCL has arranged the counters so that they take up more space than originally planned, so I will have to make the iron shield two inches higher and longer. I have included a list for Miss Schafnacker. I hope that is all right with you. I have also added some iron tubes - they can be used only once and don't need to be stainless. I had already ordered six from that company. When I last talked to a steel company, the D.O. number requirement had been extended beyond October 1st so I'm hoping that these can still be ordered without one.

Ann Arbor, Michigan
and Columbia are both
now operating their radio-

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carbon equipment, and Dr. Libby has suggested that I visit their labs.

Dr. Anderson, who worked on this at the start with Dr. Libby, has been back here this week before leaving for Copenhagen to assemble equipment for them. I've been helping him fill a counter, so I've learned something about that process.

Dr. Kulps at Columbia has succeeded in getting a lower background count than Dr. Libby's. I've heard that he is using stainless screen wall counters, so I'm glad that we have ordered only two from RCL.

Sorry this is so long.

Sincerely,
Beth Ralph

November 14, 1951

Dr. W. F. Libby
Institute for Nuclear Studies
5640 Ellis Avenue
Chicago 37, Illinois

Dear Dr. Libby:

I want to thank you very much for the generous amount of your time that you gave me during my visit in Chicago and for letting me observe and work with your equipment. As you know your assistance was invaluable for the success of our project here.

I hope to have our equipment assembled within a few weeks except for the counters and circuits which Radiation Counter Labs haven't yet delivered.

Sincerely yours,

Elizabeth K. Ralph

University of Chicago
Institute for Nuclear Studies
Chicago 37, Illinois

June 24, 1952

Memorandum to: Miss Hilde Levi
Miss Elizabeth Ralph ✓
Messrs. H.R. Crane
E.S. Deevey
J.L. Kulp
H.E. Suess

From: W. F. Libby

Subject: Carbon Analyses and Acid Extractions

Some time ago we began using the nitric acid extraction to reduce the ashes in our carbon samples. After running several samples we noticed a certain inconsistency in the results which led us to make carbon analyses of the samples. Our troubles appeared in the form of abnormally low count rates, modern woods running some 10% below the values we had secured previously with the hydrochloric acid extraction. After considerable thrashing around we performed carbon analyses on the samples. The results are shown in the following table.

Carbon Analyses of Nitric and Hydrochloric
Acid Extracted Samples

HNO ₃ Reextracts	HNO ₃ Originals	HCl Originals
77.95 + 2.65	91.81 + 0.99	80.42 + 14.56
85.22 + 1.99	91.28 + 1.54	88.6 + 4.76
86.10 + 1.41	90.20 + 1.17	76.49 + 17.45
89.80 + 1.00	82.55 + 1.65	89.06 + 5.50
86.43 + 1.05	88.72 + 1.44	88.78 + 5.98
82.75	90.97 + 1.99	
85.50	72.20 + 2.44	
85.46	91.13 + 1.37	
84.85 + 0.90	85.29 + 1.36	
78.23 + 2.96	87.34 + 1.66	
81.08 + 1.88	87.25 + 1.62	
	87.20 + 1.80	
Ave: 83.94	Ave: 87.16	Ave. (including ash): 94.32

Column 1 refers to samples which had been prepared originally with the hydrochloric acid extraction and then had been treated with nitric acid. Column 2 refers to samples which had had one hydrochloric acid treatment, three water washes, one nitric acid treatment, three water washes, and was then dried. Column 3 refers to the normal hydrochloric acid treatment. The numbers given in the table are the carbon and ash contents. These results indicate that carbon analyses are necessary for the nitric acid extraction, and that the net carbon content is somewhat lower than with good hydrochloric acid extraction. It may be that our procedure is not the same as that used in your laboratories, and that your samples have higher carbon contents. I would suggest, however, that check analyses be made. Considering column 3 for the samples prepared by the hydrochloric acid extraction, one notices that the carbon plus ash content does not add to 100%, but rather to 94%. It is also noticeable that the total is quite reproducible. Presumably, therefore, if one makes inter-comparisons between the two types of samples one must correct to 94% carbon rather than 100%. The averages of the data in the three columns indicate that this intercomparison would predict a net count of about 6.0 cpm for modern wood with the nitric acid method, as contrasted with 6.7 cpm by the hydrochloric acid method after the ash correction is made in the old way (assuming carbon plus ash to be equal to 100%). This apparently explains an anomaly that has appeared in both Dr. Kulp's and Dr. Crane's results, namely, their values for modern wood are closer to 6.0 than the 6.7 value we claim after correcting for the rather high ash obtained in the hydrochloric acid method.

I am writing this note to call your attention to this possible source of error. I think one knows the source of the trouble. The nitric acid extraction produces a much more finely divided carbon black than does the hydrochloric acid, though the hydrochloric acid itself produces a sample possessing a very considerable surface area. The absorption of water vapor and other substances on this extensive surface probably constitutes part of the volatile noncarbonaceous constituent. Direct analysis for water on a few samples has indicated that it is not all water. We have noticed also that inadequately pumped nitric acid samples have a tendency to exude a noxious vapor which reduces the counting efficiency after a few hours of counting.

I hope that you will write me of any thoughts this note leads to, and perhaps we can have a sort of round robin correspondence on the whole subject. It appears that if one makes carbon analyses either method gives satisfactory results. We carefully sample the carbon just as the material is demounted at the end of the run so that an analysis is obtained for the material as actually counted. I may say, however, that repeated mountings have given rather reproducible analyses for a given sample, so I am not at all certain this precaution is necessary.

A handwritten signature or set of initials, possibly 'W.F.P.', written in dark ink at the bottom right of the page.

July 10, 1952

Dr. W. F. Libby
University of Chicago
Institute for Nuclear Studies
Chicago 37, Illinois

Dear Dr. Libby:

Thank you for your communication about carbon analyses and acid extractions. I have had five samples of modern wood analyzed for carbon content, and the results are in fair agreement with yours. They are as follows:

HNO ₃ Method			HCL Method		
Sample	%C	%ash	Sample	%C	% ash
P-2 (Tulip)	88.4	0.2	P-3 (Pine)	87.3	3.0
P-2a' "	87.7	0.2			
P-3a(Pine)	83.5	2.8			
P-4' (Oak)	86.6	5.7			

Aug. = (C + ash) = 88.8

Total = 90.3

My net counts for modern wood, however, are still low, as you will see from the data below.

Sample	Net c/m x ash corr.	Net c/m corr. to 100%C	Net c/m corr. to 94%C
P-2	5.61 ± .12	6.35 ± .14	5.96 ± .13
P-2a'	5.13 ± .07	5.85 ± .08	5.50 ± .08
P-3	5.76 ± .10	6.29 ± .11	6.02 ± .11
P-3a	5.69 ± .15	6.62 ± .18	6.22 ± .17
P-4'	5.57 ± .11	6.07 ± .12	5.70 ± .11

The data were consistent when only the ash correction was applied except for sample P-2a', but there is not good agreement when corrected for carbon content.

The figure for the first sample (P-2) is the least reliable since voltages were drifting and so forth at that time. It is possible that

Dr. W. F. Libby
July 10, 1952
page 2

samples P-3 and P-3a were slightly contaminated because they were run while the Nevada bomb tests were in process. I am now monitoring the natural background radioactivity whenever a sample is exposed so that I will know if it gets contaminated. At the moment I am acid treating these two samples and will run them again. I am also running a sample of sugar to see what happens with a sample of negligible ash content.

If my low figure of approximately 6 c/m is found to be consistent, I still don't know how to account for the difference from your 6.7 value. Variations of counter cylinder sizes, number of screen-wall wires, and circuit time constant changes have made no difference in my net counting rate.

Sincerely yours,

Elizabeth K. Ralph

THE UNIVERSITY OF CHICAGO

CHICAGO 37 · ILLINOIS

INSTITUTE FOR NUCLEAR STUDIES

July 17, 1952

Miss Elizabeth K. Ralph
Randal Morgan Laboratory of Physics
University of Pennsylvania
Philadelphia 4, Pennsylvania

Dear Miss Ralph:

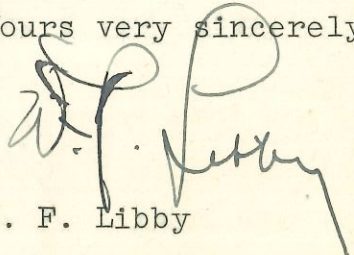
Thank you for the detailed data contained in your recent letter. I would suggest that you examine the absolute sensitivity of your counter. I find that counters frequently are inefficient, the deficiency amounting to as much as 10% in some cases. Apparently this is due to the exudation of noxious vapors from the finely divided carbon sample. This is particularly noticeable in the case of the nitric acid carbon sample. The test is as follows:

Establish by repeated use of fresh fillings and various counters what the background rate is when your anticoincidence counters are turned off but your screen wall is in your iron shield. Then make this observation routinely at the beginning and once during and at the end of each run. For example, my counters have a rate of 108 cpm when in the shield and with the AC's turned off. Frequently I find that with the carbon sample in place the rate will drop to as low as 100 cpm. I then multiply by 1.08 to correct for this inefficiency. Essentially the technique uses the meson component of the cosmic radiation as a standard source.

This is the only thought I have off hand which might possibly be a proper explanation of the lower count rate you observe for modern carbon.

I remain

Yours very sincerely,


W. F. Libby

c/o W. F. Libby
Inst. for Nuclear Studies
Univ. of Chicago
Chicago 37, Illinois
17 December 1952

Miss Elizabeth Ralph
Department of Physics
University of Pennsylvania

Dear Miss Ralph:

I am Mr. Libby's assistant in the radiocarbon dating lab. On December 27th I shall head for the East with the intention of attending the 30 December session of the AAA Meeting. If my hitch-hiking turns out well, I may be in Philly on the 29th. I should like to visit your radiocarbon lab, if there will be someone there and if such a visit will be convenient for you.

Very truly yours,

Jonathan Gell
Jonathan Gell

December 22, 1952

Mr. Jonathan Gell
c/o W. F. Libby
Institute for Nuclear Studies
University of Chicago
Chicago 37, Illinois

Dear Mr. Gell:

I shall be very glad to have you visit my radiocarbon laboratory and to talk to you on December 29th. The laboratory is located in the basement of the University Museum at 34th and Spruce Streets. The museum is not open to the public on Mondays, so please come to the service entrance and ring the bell loudly.

Very truly yours,

Elizabeth Ralph

January 30, 1953

Dr. W. F. Libby
Institute for Nuclear Studies
Chicago 37, Illinois

Dear Dr. Libby:

I am returning your paper entitled "Sensitive Radiation Detection Techniques", which you loaned to me many months ago, or should I say, years.

I believe that I have finally straightened things out here and am starting to date the Hotu cave sequence. When I use seasoned modern wood, my net counting rate is in agreement with yours, but with samples of green wood (taken from the center portions of very young trees), it is, at least, 10% lower. This has been consistent with 6 runs of 3 different samples. My latest problem - things of known age were too young - was apparently due to some C 14 contamination which I suspect, came from the nearby medical department.

I have received another communication from Mr. Daniel E. Lynch, Coordinator of Special Projects Health and Safety Division, Atomic Energy Commission, P. O. Box, 30, Ansonia Station, New York 23, N.Y. He suggested that if you would like information about the bomb tests, which is similar to that given the photographic industries, that you write to him directly, indicating what kind of work you are doing, and why you need it.

Probably, you already have this information, but I am passing it on as he requested.

Sincerely yours,

Elizabeth Ralph

March 8, 1961

Dr. W. F. Libby
Isotope Laboratory
Institute of Geophysics
University of California
Los Angeles 24, California

Dear Dr. Libby:

For many months I have intended to send you my congratulations for the honor conferred upon you. Now, in addition, I should like to commend you for the excellent article ("Radiocarbon Dating") in this week's Science. Thank you, too, for including the results of some our work.

I have enclosed a reprint of our Maya calendar dating project, and apologize for the excessive data. We should have condensed it if only to prevent it from being scrambled by the editor.

Our most recent pitfall is tritium contamination from nearby He³ users. We are in the process of sealing our laboratory, isolating the air supply, and so forth. Needless to say, C-14 dating has been halted for several months.

With best regards,

Beth Ralph

Beth Ralph

THE UNIVERSITY OF CHICAGO
CHICAGO 37 · ILLINOIS
INSTITUTE FOR NUCLEAR STUDIES

March 12, 1953

LIBBY

Dr. Ernst Antevs
The Corral
Globe, Arizona

Dear Dr. Antevs:

The first results on our series of tests for alteration in modern wood samples are now available. The results for the samples which we have measured are given below. As you see, we selected the four samples of highest priority according to your letter of October 4.

	dpm/g of carbon
<u>Lakeside 2</u> : The decomposed interior of the trunk of a Gambel oak tree cut down about 45 years ago at Lakeside, Arizona.	15.4 ± .4
<u>Lakeside 8</u> : This sample consists of the decayed center of a Ponderosa pine which you collected at Lakeside in July of 1952.	15.6 ± .3
<u>Globe 1</u> : This was the dead branch from a live Mesquite tree collected at Globe, Arizona, in August 1952.	15.3 ± .4
<u>Globe 2</u> : This was the same dead branch as Globe 1 except that it had been burned to charcoal.	14.5 ± .5

It is to be remembered that the errors quoted guarantee that for 2 out of 3 chances the true value lies within the range of the error. However, it is possible once in 20 for the truth to lie within a range twice as large. For these reasons we do not believe that Globe 2 is significantly low. I am very sorry that we have not had the time to run these more accurately, but it does seem that the data obtained so far indicate no serious deviation from the standard world-wide average of 15.3.

I wonder whether you think we should proceed to measure some of the other materials. I am asking Professor Urey's

Dr. Ernst Antevs

- 2 -

March 12, 1953

colleague, Dr. Harmon Craig, to measure the carbon-13 abundance of these four samples which we have analyzed. I guess you know that it is highly probable that any C^{14} abnormality should be paralleled by a C^{13} abnormality, and since the abundance of C^{13} can be measured with significantly higher precision than the natural radiocarbon, I personally believe that it is a better criterion.

I remain

Yours very sincerely,

W. F. Libby

cc: H.R.Crane
E.S.Deevey
R.F.Flint
J.L.Kulp
H.Levi
E.Ralph/
H.E.Suess

405 Hilgard Ave.

DEPARTMENT OF CHEMISTRY
LOS ANGELES 24, CALIFORNIA

UNIVERSITY OF CALIFORNIA

MARCH 27, 1961

MISS BETH RALPH
DEPARTMENT OF PHYSICS
UNIVERSITY OF PENNSYLVANIA
PHILADELPHIA 4, PENNSYLVANIA

DEAR MISS RALPH:

THANK YOU FOR YOUR VERY NICE ARTICLE ON THE MAYAN
CALENDAR DATING PROJECT.

I AM SORRY TO HEAR ABOUT YOUR TRITIUM CONTAMINATION
PROBLEMS. WE HAVE BEEN VERY LUCKY IN OUR LABORATORIES.
I MUST SAY I CAN ONLY FEEL SORRY FOR YOU AND HOPE OUR
GOOD LUCK CONTINUES.

I REMAIN

SINCERELY YOURS,



W. F. LIBBY

WFL/AIS

UNIVERSITY OF CALIFORNIA

INSTITUTE OF GEOPHYSICS AND
PLANETARY PHYSICS
LOS ANGELES LABORATORIES
LOS ANGELES 24, CALIFORNIA

November 1, 1961

Miss Elizabeth K. Ralph
Radiocarbon Laboratory
University of Pennsylvania
Philadelphia 4, Pennsylvania

Dear Miss Ralph:

As you can see from Professor Godwin's letter, a copy of which is enclosed, the subject of a Radiocarbon Dating Conference to be held at Cambridge in July 1962 is under consideration.

I am rather in favor of the idea of another Radiocarbon Dating Conference. Fergusson and I have discussed it and we are wondering whether Cambridge is the most suitable geographic location for all the people who might attend.

I am holding Professor Godwin's letter until I receive your comments.

I remain,

Sincerely yours,


W.F. Libby

WFL:mh

Encl:

UNIVERSITY OF CAMBRIDGE : DEPARTMENT OF BOTANY

H. GODWIN, F.R.S.
PROFESSOR OF BOTANY

BOTANY SCHOOL
DOWNING STREET
CAMBRIDGE
TELEPHONE 58304

24 October 1961

Professor W.F. Libby,
Department of Chemistry,
University of California,
Los Angeles 24,
California,
U.S.A.

My dear Libby,

By the summer of 1962 three years will have elapsed since the last Radiocarbon Symposium was held in Groningen, Holland. Many new aspects of the study have already emerged since this occasion, and some of us have felt that by next year the time will be ripe for another such symposium on the lines of its eminently successful predecessors.

There have been several suggestions that Cambridge might prove a suitable place for the meeting, as it did in 1956. These suggestions were prompted in part by the facilities that Cambridge offers in the way of accommodation and lecture rooms, and in part by its convenience geographically. If the climate of opinion proves favourable to having a further Cambridge meeting, then we should begin at once to make plans so as to allow people to make early applications to grant aiding bodies. It is for this reason that I am making an initial approach to you and a few other leading laboratories. Your views on the matter would be greatly appreciated. The time we had in mind was the last week in July (which suits the needs for College accommodation), but this might be somewhat modified if it were the general wish.

I shall look forward to hearing from you at an early date.

Kindest regards.

Yours sincerely,

Henry Godwin

*It would really be my
pettifog if you could visit Cambridge for the 2nd
as you did for the 7th 'C meeting here!*

UCT 27 1961

November 16, 1961

Professor W. F. Libby
Institute of Geophysics and Planetary Physics
Los Angeles Laboratories
Los Angeles 24, California

Dear Professor Libby:

Many thanks for your letter, which, I am sorry to say, went to the University of Pennsylvania Hospital and rested awhile before it was directed here. I think that it is fine that there are prospects of a Radiocarbon Dating Conference this summer.

When I received Professor Godwin's letter, I went ahead with other plans for the field testing of instruments for underground archaeological exploration which will take place in Italy before July. I'm not usually so forehanded, but these new projects that Dr. Rainey has initiated happened to tie in nicely for me with a conference in England in July.

Dr. Carleton Coon has included some of the key C^{14} dates in his new book about Races. He asked yesterday about the dates from Florisbad, South Africa (copies enclosed of yours and of Kulp's), and, especially, about the reason for the big discrepancy between the two. We are both inclined to think that your samples were more reliable because the so-called rootlet intrusion doesn't amount to much, and also because of the problems at the site. Dr. Coon relates that methane and other vapors evolve at the site and may have contaminated the more recent sample collections. Dr. Coon would appreciate it very much if you could elucidate this matter. The one thing that worries me is that C-850 is listed as older than 41,000, which must be a record for solid carbon. Is this a misprint, or was it the result of an especially careful counting?

These old C^{14} problems are a headache, especially, because in most cases the collectors don't know just what

2--Letter to Professor W. F. Libby dated November 16, 1961.

they did collect. Dr. Coon would be glad to know, however, that your samples and dates are the more reliable ones because they are also more consistent with his chronologies based on other evidence.

I was terribly sorry to hear that your home was destroyed in the recent fire, and extend my sympathy to you and Mrs. Libby.

Sincerely yours,

Elizabeth K. Ralph

EKR/bg
Enc.

UNIVERSITY OF CALIFORNIA

INSTITUTE OF GEOPHYSICS AND
PLANETARY PHYSICS
LOS ANGELES LABORATORIES
LOS ANGELES 24, CALIFORNIA

December 1, 1961

Miss Elizabeth K. Ralph
Radiocarbon Laboratory
University of Pennsylvania
Philadelphia 4, Pennsylvania

Dear Beth:

On Dr. Libby's behalf I would like to thank you for your reply to his letter of 1 November.

The attached copy of a letter to Professor Godwin of 29 November indicates the majority opinion of the people whom Dr. Libby contacted.

Sincerely yours,



G. J. Fergusson

GJF:mh
Encl:

P.S. Guess you wonder what's happened to us all out here for I owe you a reply to your letter re Jansen's kauni work. I am not sure where the trouble lies but strongly suspect the ring counting - will write in more detail. Re your letter to Dr. Libby - we have been trying to find C-850 to check on this >41,000 !! by solid carbon!!

Gordon.

UNIVERSITY OF CALIFORNIA

INSTITUTE OF GEOPHYSICS AND
PLANETARY PHYSICS
LOS ANGELES LABORATORIES
LOS ANGELES 24, CALIFORNIA

November 29, 1961

AIR MAIL

Professor Harry Godwin
Department of Botany
University of Cambridge
Downing Street
Cambridge, England

Dear Dr. Godwin:

Dr. Libby received your letter of 22 November late Monday afternoon and as he was traveling to the East Coast on Tuesday morning he has asked me to reply on his behalf.

As you are probably aware Dr. Libby has always been definitely in favor of a C-14 conference this coming summer, and as the last conference was held in Europe, he wondered if this time it should be held on this side of the Atlantic. Therefore, immediately on receipt of your letter of 24 October he contacted some of the American workers in the C-14 field for their reaction and comments.

It is now quite clear that the majority of the people feel that the conference would be more successful if held at Cambridge as compared to a location in the U.S.A. and so Dr. Libby would like to lend his full support to a meeting at Cambridge in late July. This date seems also to be a very convenient one for most people-- and may I add that people obviously still remember the stimulating atmosphere and hospitality encountered at the 1955 meeting.

I would like to add my personal support and offer of assistance in any way that may be possible from this side of the Atlantic-- for I am confident that it will be an interesting and valuable meeting.

Sincerely yours,



G. J. Fergusson
Associate Professor

GJF:mh

September 20, 1962

Professor W. F. Libby
Institute of Geophysics & Planetary Physics
Los Angeles Laboratories
University of California
Los Angeles 24, California

Dear Professor Libby:

Your thoughts about the Tikal samples seem logical to me, but these Mayanists are stubborn people.

After talking again to Dr. Satterthwaite, I think that the real problem is the fact that they don't accept our explanations for the difference between our dates and yours. Naturally, your reputation is much greater than mine and even though we made 33 counts for Temple IV, they still don't quite believe us.

If you ran the seven samples from structure 10 and they happened to agree with our results for Temple IV, I think that the whole matter would be settled. If they disagree, however, we'd be right back where we started. We sent the two from Temple IV to you for this latter reason - that is, as a cross-check of identical samples.

I imagine this "doubting Thomas" attitude is as much of a nuisance to you as it is here, so please make your decision as you would like.

If you decide to plunge into this and want more samples, I think that they can be obtained.

With best regards,

Beth

EKR/deh

September 13, 1962

Dr. W. F. Libby
Institute of Geophysics and Planetary Physics
Los Angeles Laboratories
University of California
Los Angeles 24, California

Dear Dr. Libby,

Thank you for your letter and the copy of your paper entitled "The Veracity of Radiocarbon Dates." Needless to say, I do not object to your using our figure of Tree-Ring Dated Samples. I should point out, however, that some of our Sequoia results may be erroneously old, notably the extremely low one (-4.5%) at 2400 B. P. in the figure. Apparently, the boys in the lab were having trouble with the purification of these samples while I was away. This problem continues to bother us with Sequoias especially, and occasionally with Oaks. If you have any suggestions of why this is so and what we should do about it, some help would be very much appreciated. I have a notion that it may be due to some phenolic compound from tannins that is oxidized in the preliminary steps and then not absorbed in later traps, but I haven't found a sure way to eliminate it. As usual, with these elusive things, it is present in very small amounts but does cause about a 50 v shift in our calibration curve (cross-over point between pulses 5-30 volts and those greater than 30 volts). I imagine that it does not bother methane nor acetylene counting.

In your paper, there is one minor correction which I noted on page 4. Aristata should be Pinus aristata or Bristlecone Pine. In general, I think that your paper is fine and a good reply to the doubters who read fragments about our methodological problems and then question the whole method.

Dr. Satterthwaite, our Curator of the American Section, has asked me several times about the samples from Tikal, mostly from structure 10, which he sent to you. I forgot to ask you about them when I had the pleasure of talking to you in Cambridge. If your laboratory has dated them or plans to in the near future, he would be very pleased.

Sincerely yours,

Beth Ralph

BR:dml

UNIVERSITY OF CALIFORNIA

INSTITUTE OF GEOPHYSICS AND
PLANETARY PHYSICS
LOS ANGELES LABORATORIES
LOS ANGELES 24, CALIFORNIA

September 17, 1962

Miss Elizabeth K. Ralph
Physics Department
University of Pennsylvania
Philadelphia 4, Pennsylvania

Dear Beth,

The thought occurs to both Gordon and me that we can't do much for Dr. Satterthwaite and the Mayan calendar problem by running just one or two or even five or six samples. If we can we'll be pleased to but it looks to us as though you cleaned up the problem pretty well. Please drop us a note on this right away. Our present thought is that we should just return the samples with our most humble apologies to Dr. Satterthwaite. Please let me know whether this is right.

Thanks for your comments on my paper. I'm putting in all your changes.

Yours very sincerely

Bill
W. F. Libby

WFL/ef

UNIVERSITY OF CALIFORNIA

INSTITUTE OF GEOPHYSICS AND
PLANETARY PHYSICS
LOS ANGELES LABORATORIES
LOS ANGELES 24, CALIFORNIA

August 30, 1962

Miss Elizabeth Ralph
Department of Physics
University of Pennsylvania
Philadelphia 4, Pennsylvania

Dear Miss Ralph:

With your permission I would appreciate the use of your figure Tree-Ring Dated Sample given in your paper at Cambridge Radiocarbon Dating Conference in this little paper of mine.

Please give me your comments on it in general.

Thanks in advance.

I remain,

Sincerely yours,



W.F. Libby

WFL: mh
Encl: 1

T. Libby

JOHN SIMON GUGGENHEIM MEMORIAL FOUNDATION

551 FIFTH AVENUE • NEW YORK 17 • N. Y.

October 2, 1962

MEMORANDUM:

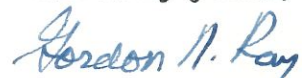
Dr. W. F. Libby

has suggested to us that you may wish to make application for a Guggenheim Fellowship and, accordingly, we send you, herewith, a statement concerning our Fellowships and a set of application forms.

In making this suggestion that you may be interested to apply, we do not give you any assurances concerning the result of an application here. That the staff is unable to do; for all decisions are made by the Foundation's Committee of Selection. But we do assure you that any application you may present will be carefully considered.

If you have questions, we shall be pleased to try to answer them.

Sincerely yours,



Gordon N. Ray
Secretary General

Miss Elizabeth Ralph
University Museum
University of Pennsylvania
Philadelphia, Pennsylvania

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INSTITUTE OF GEOPHYSICS AND PLANETARY PHYSICS
LOS ANGELES, CALIFORNIA 90024

December 11, 1964

Dr. Elizabeth Ralph
Department of Physics
University of Pennsylvania
Philadelphia, Pennsylvania

Dear Beth:

I am terribly sorry to learn of your illness. I do hope everything is going better for you.

We are just finishing our current Radiocarbon Date List and will send you a copy soon. It is reported that the Tree Ring Lab will shortly be able to supply bristlecone pine rings of known age back to 6000 years. This seems rather difficult to believe. I am not certain this is actually their statement, but Rainer thought he was told this.

I remain

Sincerely yours,

A handwritten signature in blue ink that reads "Bill".

W. F. Libby
Director

wfl/jl

December 18, 1964

Dr. W. F. Libby
Institute of Geophysics
and Planetary Physics
Los Angeles, California 90024

Dear Dr. Libby:

Many thanks for your kind letter. I am now feeling much better except for being "grounded" in Philadelphia for the winter.

I have enclosed preprints of our Date Lists VII and VIII which have been submitted to Radiocarbon for publication.

We are a bit disturbed by a draft of an article entitled "A Review of Radiocarbon Measurements from Peru and Bolivia" by John Howland Rowe (Department of Anthropology, University of California, Berkeley), which has come to our attention. Prof. Rowe tries to state that the C-14 dates for Peru and Bolivia form two separate time scales that differ by 20%. Quite naturally, in order to support this hypothesis which Bob Stuckenrath and I consider ridiculous, he has had to perform some strange imitations of C-14 dates. Among them is the addition of 200 years to all (or most) of your Chicago dates. If you would like to voice your objection to this, I'm sure it would help to discourage Rowe's article.

We, too, are excited about the bristlecone pines and I do hope that the tree-ring dating will go ahead with as much speed as possible.

With best regards,

Beth Ralph

EKR:ek

Encls

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INSTITUTE OF GEOPHYSICS AND PLANETARY PHYSICS
LOS ANGELES, CALIFORNIA 90024

March 10, 1965


Dr. Elizabeth K. Ralph
Associate Director
Applied Science Center for Archaeology
The University Museum
University of Pennsylvania
33rd & Spruce Streets
Philadelphia, Pennsylvania 19104

Dear Beth:

Congratulations on your becoming Associate Director of the University Museum Applied Science Center for Archaeology. I look forward to working with you over the years as we have in the past. I hope you are feeling better now.

I remain

Sincerely yours,


W.F. Libby
Director

wfl/jl

Techniques

April 15, 1965

Dear Bill:

Just a note to say I have seen your note to Alvarez about his scheme to X-ray the pyramids and I want you to know that we expect to be working at the Bent Pyramid next winter and I have offered Alvarez all kinds of cooperation in his experiment. Also, in case you have not seen the first issue of our ASCA bulletin, I am enclosing a copy. The latest news is that our man, Mark Han, really seems to be working out the thermoluminescence method of dating pottery.

It is good to know that you are still working on archaeological projects and I am sure we should keep in touch,

Very best wishes,

Froelich Rainey
Director

Mr. William F. Libby, Director
Institute of Geophysics and Planetary Physics
University of California
Los Angeles, California 90024

FGR/vg



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LOS ANGELES, CALIFORNIA 90024

March 15, 1965

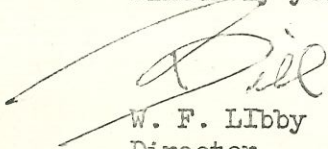
Dr. Luis W. Alvarez
Lawrence Radiation Laboratory
University of California
Berkeley, California 94720

Dear Luie:

Your proposal to X-ray the Egyptian Pyramids with cosmic rays is a fabulous thing, and I am 100% for it. If there is any way in which I could help I would be very glad to do so. I have checked through on your considerations in every way I can, and it seems to me the proposal is completely sound. This is a very remarkable idea.

I remain

Sincerely yours,


W. F. Libby
Director

wfl/jl

UNIVERSITY OF CALIFORNIA

Tednique

LAWRENCE RADIATION LABORATORY
BERKELEY, CALIFORNIA 94720

April 7, 1965

file

Dr. Froelich Rainey
The University Museum
University of Pennsylvania
Philadelphia, Pennsylvania

Dear Dr. Rainey:

It was certainly a pleasure to hear from you and to have a chance to read your most fascinating newsletter. John Wilson and Bill Libby certainly knew what they were doing when they steered me in your direction.

I am pleased to hear that Dr. Fakhry has played such an important role in smoothing out the difficulties that have stood in the way of archeological research in Egypt for the past few years. I look forward to meeting Dr. Fakhry, since I enjoyed his book so very much. John Wilson is planning to brief Dr. Fakhry on our proposal, and I hope you will have the chance to discuss it with him either by letters or in person.

In view of your plans to excavate at Dashur next winter, it might be appropriate to try out the equipment in the lower chamber of the Bent pyramid before moving into the second pyramid, the target proposed in my memorandum. One advantage of the Bent pyramid is that there is enough structure known to make it a worthwhile exercise to see if that structure could be picked up on the "x-ray film".

One of my students is making some simulated films on the computer, using a "Monte Carlo routine". So far he has produced a number of photographs of a solid pyramid, but he hopes to insert some chambers in the programmed pyramid in a few days. He will then ask me to see if I can find the chambers after exposures of gradually increasing length, so that the statistical fluctuations are gradually reduced. This will be a very exciting exercise, and I will send copies of the photographs to you as soon as we have something to show.

Thank you again for your cordial response to my letter.

Very sincerely yours,

Luis W. Alvarez

Luis W. Alvarez

LWA:am

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INSTITUTE OF GEOPHYSICS AND
PLANETARY PHYSICS
LOS ANGELES LABORATORIES
LOS ANGELES, CALIFORNIA 90024

January 22, 1965

C
Dr. Sidney Goldstein
Chairman, Department of Sociology
and Anthropology
Brown University
Bristol, Rhode Island

Dear Dr. Goldstein:

O
Thank you for your letter about the plans in honor of the late Dr. James Louis Giddings. I would like to make a small contribution by offering to do some radiocarbon dating for the project. I know it is not likely that this will be necessary, but in case Miss Ralph should be rushed we would be please to do some dates in honor of Dr. Giddings.

I remain

P
Sincerely yours,

✓
W. F. Libby
Director

wfl/jl

Y
cc: ✓ Dr. Elizabeth Ralph

May - June



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✓
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October 20, 1967

Dr. Elizabeth Ralph
University of Pennsylvania
Philadelphia, Pennsylvania

Dear Beth,

Bill Libby will be coming through New Jersey on the evening of Thursday, November 2, and we are contemplating having an informal get-together to discuss the constancy of the atmospheric radiocarbon activity over the past 10,000 years. We are hoping to get together as many as possible of the radiocarbon fraternity in the East and to make it an enjoyable time.

We would hope to have dinner together first, followed by the meeting at Isotopes. In this way we can have plenty of time to chat with old friends and to see where the state of the art is leading us.

I really hope that you can find it possible to join us on this occasion. I will have more complete details nearer the date.

Looking forward to hearing from you,

Yours sincerely,

Eric H. Willis
Vice President
Director of Research

EHW:mm





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A TELEDYNE COMPANY

November 3, 1967

Dr. Elizabeth K. Ralph
Museum Applied Science Center for Archaeology
The University Museum
University of Pennsylvania
33rd & Spruce Streets
Philadelphia, Pennsylvania 19104

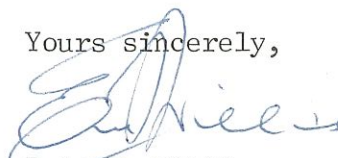
Dear Beth,

I was sorry to hear about your accident. It must have been extremely painful and I trust the period of convalescence is being successful.

We missed you last night at the meeting which was extremely successful. Bill Libby was in good form and we had about twenty people. The investigation of the constancy of the radiocarbon in the atmosphere has a long way to go before anything definitive emerges. I am sure that your bristlecone pine samples will be extremely important in this. In fact it would seem that they are now crucial and I shall look forward to hearing how you get on both in your work and your recovery.

With warmest regards,

Yours sincerely,



Eric H. Willis

EHW:mm



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SANTA BARBARA · SANTA CRUZ

DEPARTMENT OF CHEMISTRY
LOS ANGELES, CALIFORNIA 90024

August 2, 1968

Dr. Froelich Rainey
University Museum
University of Pennsylvania
Philadelphia, Pa. 19104

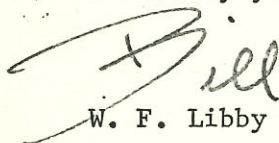
Dear Froe:

Thanks for everything. It was a most enjoyable visit we had;
and I do hope something will come of the whole matter.

You have a marvelous museum. I can see that a great part of
its excellence is due to you personally.

I forgot to ask you whether you and Beth will be at the
Uppsala Radiocarbon Dating Conference next August, (a year from
now!!). Dr. Berger from my shop, and Dr. Sues from La Jolla
are proposing an international Radiocarbon Dating conference
also. I hope things do not get confused. We should have spoken
about this a little, but it slipped my mind.

Sincerely yours,


W. F. Libby

WFL:rdj

Starting AUG 4, 1968
new Centrex telephone
number at UCLA is
(213) 825-3019 825-1968

August 13, 1968

Dr. W. F. Libby
Department of Chemistry
University of California, Los Angeles
Los Angeles, California 90024

Dear Dr. Libby:

Fro Rainey is on vacation, so I am writing to answer your question tentatively until he returns.

If there is an international Radiocarbon Dating conference next year and, hopefully, just one, I think that our laboratory should be represented, and I guess that I would be the one to go. Since I missed the last one at Pullman, I have felt a bit out-of-touch with other laboratories. Fro thinks that conferences are a waste of time and also he prefers to be in Vermont in August. It would be more convenient to have it in the U.S.A. rather than in Uppsala, but I should prefer to attend only one.

We all enjoyed your visit, and hope that we will be seeing you regularly.

With best regards,

E.K.Ralph

EKR/mhr

Dr. Rainer Berger
Institute of Geophysics
University of California
Los Angeles, Calif. 90024

August 15, 1968

Dear Dr. Berger:

While Dr. Libby was visiting here, he mentioned a few improvements that we might make in our C-14 lab, and suggested that I write to you.

1. Combustion in a combination of oxygen and argon instead of just oxygen.

Could you tell us what proportions and flow rates you use?

2. Reliable supplier for anti-coincidence geiger counters.

We have tried at least five different suppliers over the years and geigers have not lasted more than a year. The most recent suppliers that we tried didn't even succeed in making the geigers. Dr. Libby said that yours have been in use for 10 years. May I trouble you for the name and address of your supplier. It might be helpful also to know the dimensions of your geigers. Ours are especially large, 39 in. long and 2 in. O.D. and they seem to be more difficult to manufacture.

Sorry to bother you with these questions.

Sincerely yours,

Elizabeth K. Ralph

EKR:zb



DEPARTMENT OF CHEMISTRY
LOS ANGELES, CALIFORNIA 90024

April 11, 1973

Miss Elizabeth K. Ralph
Department of Physics
University of Pennsylvania
Philadelphia, Pennsylvania 19104

Dear Beth:

At the recent international meeting in New Zealand, it became clear that radiocarbon can be useful in establishing the history of the climate world-wide over the last 40,000 or 50,000 years and a number of people suggested that it might be worthwhile to make a coordinated attack and to put together a proposal for the funding of the work.

I am planning to serve as initial planner, inquirer, and tester of the idea and so I write to you for your ideas as to what work you would like to do and how much the funding would probably amount to for a five year attack on this problem.

If you have no need of funding support, but would still like to be part of the effort, please indicate this.

Sincerely yours,

W. F. Libby
W. F. Libby

WFL:sw

- 1) Warrology?
- 2) Paleopalynology?
- 3) I could do Russian (and Czech) sources?
- 4) Radiocarbon dates of floating chronologies, such as those derivable from Ferguson's Colorado R. woods, and bristlecone remnants from caves and mouse furrows (seeds, cones).
- 5) Could excavate alluvial fans.

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DEPARTMENT OF CHEMISTRY
LOS ANGELES, CALIFORNIA 90024

April 11, 1973

Miss Elizabeth K. Ralph
Department of Physics
University of Pennsylvania
Philadelphia, Pennsylvania 19104

Dear Beth:

At the recent international meeting in New Zealand, it became clear that radiocarbon can be useful in establishing the history of the climate world-wide over the last 40,000 or 50,000 years and a number of people suggested that it might be worthwhile to make a coordinated attack and to put together a proposal for the funding of the work.

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If you have no need of funding support, but would still like to be part of the effort, please indicate this.

Sincerely yours,

A handwritten signature in blue ink that reads "W. F. Libby".

W. F. Libby

WFL:sw

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SANTA BARBARA • SANTA CRUZ

DEPARTMENT OF CHEMISTRY
LOS ANGELES, CALIFORNIA 90024

August 24, 1973

Ms. Elizabeth K. Ralph
Applied Science Center for Archaeology
University of Pennsylvania
Philadelphia, Pennsylvania

Dear Ms. Ralph:

For Bill Libby's 65th birthday in December, we would like to invite you to write a few typewritten pages about some scientific work of yours which:

- a) you are proud of and
- b) was stimulated by Bill in some fashion: either by his own research or by his suggestions and comments.

Please send both your comments (with references and footnotes in the style of Science) and two reprints of your work by September 30, 1973.

In case you would find it more appropriate we would welcome also a one page birthday greeting in a form you like best.

Sincerely yours,

A handwritten signature in blue ink that reads "Leona".

Leona Marshall Libby

A handwritten signature in blue ink that reads "Rainer".

Rainer Berger

LML/RB:nc



May 6, 1977

College of Science
Office of the Dean
(215) 895-2620

Dr. Elizabeth Ralph
Director, University Museum
University of Pennsylvania
Philadelphia, Pa.

Dear Dr. Ralph:

Dr. Willard Libby will be on Drexel's campus on Tuesday, May 10th, and Wednesday, May 11th. I hope that you will be able to attend Dr. Libby's lecture on "Science and Public Policy" at 3:30 P. M. on May 10th in Mandell Theatre.

Following the lecture there will be a reception in the Living Arts Lounge, next to the theatre. We would be very happy to have you come to the reception as our guest and would be happy to welcome any associates you would like to invite.

Sincerely yours,

A handwritten signature in blue ink that reads "Francis K. Davis".

Francis K. Davis
Dean of Science

hcw



UNIVERSITY of PENNSYLVANIA
PHILADELPHIA 19104

The Faculty of Arts and Sciences

DEPARTMENT OF PHYSICS

December 17, 1981

Dr. L.M. Libby
Dept. of Chemistry, UCLA
405 Hilgard Avenue
Los Angeles, CA 90024

Dear Dr. Libby:

Many thanks for the copy of Radiocarbon and Tritium. I had read the ^{14}C articles, but not many of the ones about ^3H , and I found them to be interesting.

Things here are much the same, but we do have new electronic equipment, which is functioning well.

I hope that your work is proceeding satisfactorily.

With best regards,

Beth Ralph

BR:bh

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405 HILGARD AVE
LOS ANGELES CA 90024



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DR ELIZABETH RALPH
RADIOCARBON LABORATORY
UNIVERSITY OF PENNSYLVANIA
DEPT OF PHYSICS
PHILADELPHIA PA 19104

WILLARD LIBBY DIED 8 SEPTEMBER HERE DUE TOPNEUMONIA. MEMORIAL
SYMPOSIUM WILL BE HELD LATER THIS YEAR. WILL KEEP YOU INFORMED.
RAINER BERGER

17:52 EST

MGMCOMP



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A Celebration of Willard Frank Libby

and

a Party for his Birthday

on which he would have been seventy-two

Thursday, December eighteenth . [1980?]

2250 Young Hall, UCLA

two to four in the afternoon

speakers

David Saxon

Sherwood Rowland

Edward Teller

Franklin Murphy

William McMillan

Serge Korff

Rainer Berger

four to six in the afternoon

orchestra, champagne, sandwiches on the terrace

Please reply to (213) 825-7675 or 825-1580