AFTER FIVE DAYS RETURN TO
WESTERN STRUCTURAL COMPANY
3201 FOURTH AVENUE
MOLINE • ILLINOIS

[Roys, 11/21/1961]

Enclosed with letter to L.S.

Dr. E. K. Ralph

Dept. of Physics
University of Penn.

Philadelphia 4

Penn.

LAWRENCE ROYS
825 TWENTY-SEVENTH STREET
MOLINE, ILLINOIS

Nov. 21st, 1961

Dear Dr. Ralph:

Thanks for your memoranda and explanations, and particularly for your putting your letter on a conversational basis; I fully understand that the mistakes did not affect the main conclusions. The C-14 age formula was especially welcome, and I have run through about a third of the transfers from Table 2 to Table 1 with the same results that you obtained. I have started to plot them graphically and found it interesting to see that over a century of time, the plot appears as a straight line, although the logic is logarithmic of course.

After pecking away at a couple of statistical textbooks for a considerable time, I felt that I had absorbed enough of the peculiar vocabulary to tackle one of the experts in my community for some discussion. It was gratifying to find him in agreement with me that for Temple IV and Temple I, we are fortunate in having a series of observations where arithmetical means alone tell a story that is forcibly clear.

We talked about the formulae on page 177 and I take you up on your offer to discuss matters.

A) I, as a rank amateur, cannot get the logic of $\sigma_s = \sqrt{(\tilde{n}_t + b)N} \div N$ which adds the 10,000 background count to n_t, the gross count which in itself includes the background.

B) My friend brings up another point which I will try to express. Variance, or sigma^2, is customarily equated with the square of some value or summations of squares which are 2d powers. Here sigma, a 1st power, is equated to the square root of a first power, which seems unconventional, and apparently illogical.

C) I may have found an answer for this question. Libby (1952, p. 68 and 1955, p. 71) says: "The statistical error is taken as the square root of the total number of counts divided by the number of minutes during which they occurred." If Libby means by "statistical error" something equivalent or parallel to the standard deviation, it would seem to indicate a difference in logic from our question. This equates a 1st power with a square root.

It occurs to me that in his opening a new field, practically a new discipline, Libby may have felt justified in breaking away from the pure mathematical approach, and has given a simplified rule of thumb.

Also in the sentence following, Libby mentions "probable error". I am wondering what he means.

D) Maybe I had better mention your other equation $\sigma_t = \sqrt{n}_t$ which is in line with the other points raised.

Any thing that you care to say about any of theses point will be much appreciated.

Very sincerely

Lawrence Roys

November 28, 1961

Dr. Lawrence Roys
825 27th St.
Moline, Illinois

COPY

Dear Dr. Roys:

Thank you for your letter of Nov. 21st. I admire your energy in checking the calculations, and it is good to learn that most of them are correct.

In regard to your paragraph A), the additional background uncertainty, b, is added because the background count is determined independently with a sample of anthracite coal, that is, it is not counted at the same time. Therefore, its uncertainty, as well as the uncertainty in background during the counting of the unknown sample, is included.

Paragraphs B) & C): Libby's "statistical error" is the so-called "standard deviation" rather than the probable error or some other one that he might have chosen. The names are simply definitions, as shown below.

Values of the Probability Constant, K

K	Probability that
0	1.000
0.6745	0.5000 (probable error)
1.000	0.3173 (probable error)
1.6449	0.1000 ("reliable" error)
etc.	etc.

Libby happened to select the standard error.

Libby's simplified system is all right for single counts of samples, but for multiple counts and counts of series of contemporaneous samples, then a more elaborate treatment such as the one we chose is needed.

I think that the matter of sigma^2 is explained in the copies of pages (enclosed) from Segre's book, Experimental Nuclear Physics (John Wiley & Sons, 1959). If not, please let me know.

Thank you again for your interest.

Sincerely yours,

LAWRENCE ROYS
825 TWENTY-SEVENTH STREET
MOLINE, ILLINOIS

Jan. 28, 1962

Dear Dr. Ralph:

Please pardon my failure to acknowledge your letter sooner. I plead Christmas activities etc.

I am especially grateful for the enclosure of the documentation of the formula. It would have been quite a nuiscance to send away for it.

I am soon leaving for a three week vacation in Guatemala, and am writing Dr. Satterthwaite of it. I do not know whether he is there or in Philadelphia, but if he should be in Guatemala, would you mind phoning over to the Museum that the letter should be forwarded.

Again thanking you for your letter,

Very sincerely

Lawrence Roys

Nothing privats in my letter. It simply gives my schedule of dates

Dr. Satterth

LAWRENCE ROYS
825 TWENTY-SEVENTH STREET
MOLINE, ILLINOIS

8/5/65

Dear Beth:

I have been home from Europe over two months and am just catching up on letters that should have been written earlier. Apologies for delay in showing some of my enthusiasm over your report in the April issue of American Antiquity. It is a needed presentation and will be widely appreciated.

The last paragraph referring to Structure 10 is a constructive move. Morley certainly used association plus imagination to find any coefficient at all in front of the Ahau in his "3 Ahau, 3 Mol". Regardless of whether 9.15.10.0.0 is right or wrong, any positive interpretation can be misjudged as indicating careless thought in the article as a whole. I suspect Smilie would put forward such an argument if he had thought of it.

The same idea applies to your conservatism in showing the extreme date variation that is possible in interpreting the tree rings, your Table 4. While such extremes are very improbable, you have largely discounted iconoclastic use of this situation. In short, I think that you have strengthened the original presentation of 1960 by mentioning difficulties.

I have a pleasant compliment for you from England. Dr. M.J. Aitken was not able to see me, but gave me a cordial telephone interview from Cambridge. I wanted more knowledge regarding his page 110 of "Physics and Archeology", Apparently little has been published since Vries. 1958, and Willis et al, 1960; and I pressed him to name some one I could reach in the United States. He said that you were

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the person that I ought to be in touch with.

I still must write to Dr. Satterthwaite and others, but as soon as I get my affairs in order, I will shoot a couple of specific questions at you, hoping that they will be answerable.

Best regards,

Lawrence Roys

✓ Roys

825 27th St. Moline, Illinois.

June 15th, 1964

Dear Miss Ralph:

I have delayed writing you because each question that I originally had in mind has brought up others, Hydra-like. First I will say that it was a pleasure to meet you, and helpful to me to talk with you. Now for the questions, and please do not consider them demands for anything formal.

A: Would you give me the name of the text that you referred to as your general hand-book? I think that there is a copy in Moline, as I have already found Lindgren and McElrath, and Croxton and Crowden. No two texts seem to come close to each other on presentation, or even vocabularies.

B: Referring to the exponential decay curve, (1960, Fig. 2), did you use the curve, (a much larger original I suppose); or is it worth while to compute from the formula? If the latter, which form seems most convenient to use? I rather suspect that a curve would be more practicable, and if so, is it asking too much if you could trace off for me the limited part of the curve from 1100 B.P. to 1300 B.P.? Nothing fancy as I could carefully do the drawing if you pricked in four or five points. It cannot be very far from linear.

C: Transferring from Table 2 to dates B.P. seems obvious via exponential curve readings; but the failure of transformation from Table 3 data into the "plus-or-minus" figures **in years** in Table 1 puzzles me. I should not say "failure" here because the underlined sigmas of Table 3 are <u>approximately</u> consistent with the "plus or minus's" that govern the dates B.P. for the wood samples as given in the middle column of Table 1. However, I am quite curious as to why they seem only approximate. To arrive at Table 1 figures, I have to vary my multiplier from the figure 0.286 up to 0.310; and the range of 8% makes me suspect that I may be ignoring some intermediate step.

Speaking further of these "plus-or-minus", does each different laboratory use its own individual formula, or are they working toward some sort of a standard? For instance Libby (both 1955 edition, p. 77 ; which is word for word <u>like the</u> original edition) gives the simple formula $\sigma = \sqrt{\frac{\text{gross count}}{\text{number of minutes}}}$ Maybe I have missed some reference, but I doubt if this is more than a rough and ready rule of thumb.

At just this moment, I think that I am beginning to see light. Even as late as 1955, Libby was still worrying about sewing up the exponential curve tighter with stuff like Egyptian dates. However I have seen no evidence of standardization of the refinements that you are utilizing.

Along these lines, I get the impression that the "plus-or-minus" shown in years (in Radio-Carbon Supplement of Science) means a one sigma amount. I have not been able to find any statement to such an effect. I agree that the one sigma probability is both reasonable and workable, but it is not mentioned in the ten commandments. No use digging for the answer, but if you recall a pronouncement on the subject, you might mention it.

More important, I am puzzled by Libby's mention of "probable error" on the same page as one sigma. Would you care to say something about that? A definition?

I again say that it would be an imposition on you to answer all of the questions that I have raised. However I should be grateful for any comment you could make without having to dig deeply. Also, I certainly would not quote you without asking again of you what I could use.

I have just gotten hold of a copy of Libby's contribution to the Urey memorial volume, and his Apr. 1963 article in Science.

With cordial regards

Sincerely Royce

Will you kindly hand the copy to Dr. Satterthwaite. I am trying to write to him tomorrow.

June 26, 1964

Mr. Lawrence Roys
825 27th Street
Moline, Illinois

Dear Mr. Roys:

Thank you for your letter of June 15th. In regard to your questions, I am not sure that I understand them all correctly, but here are some answers.

A. For C-14 dates, the general "hand-books" are the volumes of Radiocarbon, a supplement of The Amer. J. Sci., published at Yale Univ. For a good description of the method, see M. J. Aitken, Physics and Archaeology, Interscience Publishers, 1961 or for a more detailed but out-of-date account, see W. F. Libby, Radiocarbon Dating, Univ. of Chicago Press, 1955.

B. We calculate dates by the formula, not the graph of the decay curve. The basic form is $I = I_o e^{-\lambda t}$ which is reduced to $t = 18.5 \times 10^3 \log \frac{I_o}{I}$ for the Libby 5568 half-life. $I_o = 32.146 \pm .040$ for our counters at that time and I = counting rate of unknown sample. If you plot the decay curve on semi-log paper, it is a straight line.

C. Table 3 provides the plus or minus tolerance for the average net counting rates (n_n) in Table 2. The way these were derived from the data in Table 2 is explained in the Segre reference. To transform these into dates, use the age calculation equation first with $I = n_n -$ and then with $I = n_n +$. Subtract one from the other and half of the difference is the plus and minus.

The sigma that Libby used mostly is the simple form for a single count. We are dealing with multiple counts.

In most date lists, the plus or minus represents one sigma, that is, one standard deviation. The probabilities are the same whether it represents many counts or a single one, but for the latter, it is usually larger.

With best regards,

EKR:pc

E. K. Ralph

LAWRENCE ROYS
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MOLINE, ILLINOIS

July 6th, 1964

Dear Beth:

Just a line of thanks for your letter. It filled in gaps in my technical knowledge quite precisely. It is bad to have to wonder whether one is not wrongly interpreting a phrase, say by Libby.

I am really grateful for your typescript, "Review of Radio-carbon Dates". Of course I will use nothing without writing you first, but it advances my thinking by months. The detailed presentation of closeness of tree-rings in the tropics is a real contribution.

With regards and thanks,

Lawrence Roys

Your shortened signature seemed such a pleasant cordiality that I have used it above.

LAWRENCE ROYS
825 TWENTY-SEVENTH STREET
MOLINE, ILLINOIS

Aug. 14, 1964

Dear Beth:

Delayed somewhat by business, I am now making progress on the paper trying to outline the radiocarbon dating process in semi-technical language; i.e., to some one who remembers a moderate amount of high-school algebra.

I told you that I would be careful about quoting you; hence this letter. I would like to use the following as an a actual example of how the counting of Beta particles may be converted into years B.P.

"Miss Ralph has kindly given me the convenient reduction which she used in her 1960 report. It is given in common logs; viz: $t = 18.5 \times 10^3 \times \log \frac{I_0}{I}$ "

I understand that the constant 18.5 is peculiar to your laboratory.

Is it all right to quote you as above. If you prefer not, do not hesitate to say so.

I enclose a post card for your convenience.

Best regards to yourself and the others.

Lawrence Roys