

**NOTE CONCERNING “INTERCOMPARISON OF HIGH-PRECISION <sup>14</sup>C MEASUREMENTS AT THE UNIVERSITY OF ARIZONA AND THE QUEEN’S UNIVERSITY OF BELFAST RADIOCARBON LABORATORIES” BY KALIN *ET AL.* (1995) AND THE REGIONAL EFFECT**

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**INTERCOMPARISON OF DATA FROM HIGH-PRECISION <sup>14</sup>C LABORATORIES**

Although a coauthor of the paper of Kalin *et al.* (1995), I was not available in time to review the final copy. Consequently, I take this opportunity to make further comments and, hopefully, clarify some of the content and conclusions.

From Table 2 of the paper by Kalin *et al.* (1995), the Arizona average ( $-15.35‰$ ) is in close agreement with the average of Stuiver and Becker (1993) ( $-15.30‰$ ), where the difference between the two laboratories, Tucson (T) and Seattle (S), is  $T(1995) - S(1993) = 0.05 \pm 0.15 (\bar{\sigma})‰$ . On the other hand,  $S(1986) - S(1993) = 2.50 \pm 0.24 (\bar{\sigma})‰$ . This implies that comparing Stuiver and Becker (1986) with Stuiver and Becker (1993), the data in 1993 have been increased by  $21 \pm 2 (\bar{\sigma})$  yr. From their Table 1, the data from Pearson *et al.* (1986), Belfast, B (95) and Tucson T (95) are all greater than the equivalent values from Pearson and Qua (1993). The average difference is  $2.3 \pm 0.5 (\bar{\sigma})‰$  or  $19 \pm 4 (\bar{\sigma})$  yr. The difference between the two averages is insignificant, implying that, to intercalibrate for the Calibration 1993 issue of *RADIOCARBON*, both sets of data have been increased by *ca.*  $20 \pm 4 (\bar{\sigma})$  yr (Stuiver 1993).

In Table 1, I compare data from the 1986 and 1993 calibrations (Stuiver and Kra 1986; Stuiver, Long and Kra 1993). The table compares Seattle (Stuiver and Becker 1993) with Belfast (Pearson and Qua 1993; Pearson, Becker and Qua 1993), Groningen (de Jong, Becker and Mook 1986), Pretoria (Vogel *et al.* 1993; Kromer *et al.* 1986) and Tucson (Linick *et al.* 1986). The data in Table 1 are referred to as S (93), B (93), G (86), H (86), T (86) and P (93).

Table 1 shows that agreement is good between S (93) compared with G (86), P (93) and T (86) in the BC period in intervals where comparisons are available, from 1930 BC to 6360 BC. Note again that in the AD period S (93) seems to be  $21 \pm 2$  yr older relative to S (86) (Table 2, Kalin *et al.* 1995). This is not presented in the above comparison. Also, all of the tree-ring samples for G (86) and P (93) and most of the samples from S (93) are from Southern Germany oak trees. The wood used by T (86) is bristlecone pine from the White Mountains of California, implying no significant regional effect between California and South Germany during measured intervals from 1930–6360 BC.

A significant difference ( $\sim -41 \pm 8 (2\bar{\sigma})$  yr) appears between Seattle (93) and Heidelberg (86) as well as Belfast (93) in the BC interval from 4075 to 6000 BC. According to Stuiver and Pearson (1993: 3), “the reasons for the larger offsets are, as yet, not well understood”. In comparing S (93) with B (86), the difference would increase by another *ca.* 20 yr resulting from the 1993 corrections.

Pearson, Becker and Qua (1993) report six replicate measurements of samples in the age range 3130 to 3230 BC previously reported in the 1986 Calibration Issue and remeasured for the 1993 Calibration Issue. The difference is  $14 \pm 8 (\bar{\sigma})$  yr, with the remeasured data being older, as before, at 92% confidence.

TABLE 1. Comparison of High-Precision Measurements\*

Comparison	Age range (cal AD/BC)	Offset (years $\pm \bar{\sigma}$ )	No. of comparisons
S (93) – G (86)	3210–3910 BC	$-4 \pm 2$	36
S (93) – P (93)	1930–3550 BC	$+4 \pm 2$	72
S (93) – T (86)	5680–5810 BC 6475–6360 BC	$-3 \pm 7$	17
S (93) – T (95)	AD 1085 to AD 1115	$0 \pm 1$	4
S (93) – B (93)	AD 1840 to 5180 BC	$+2 \pm 1$	344
	5180–5500 BC	$-54 \pm 5$	24
	5500–6000 BC	$-15 \pm 4$	16
S (93) – H (86)	4075–5265 BC	$-41 \pm 4$	65
	5805 to –5995 BC		
B (95) – B (86)	3450–3470 BC	$-18 \pm 3$	2
	2490 BC	$-5 \pm 19$	1
	390 BC and 370 BC	$-16 \pm 1$	2
B (93) – B (86)	3450 BC and 3470 BC	$-28 \pm 8$	2
	2490 BC	$-15 \pm 22$	1
	340 BC and 370 BC	$+5 \pm 20$	2
	3450 BC and 3470 BC	$-42 \pm 8$	2
B (93) – T (95)	2490 BC	$-1 \pm 18$	1
	340 BC and 370 BC	$-28 \pm 11$	2

\*Data are from Stuiver and Pearson (1993) and Kalin *et al.* (1995)

#### CAUSES OF OFFSETS BETWEEN HIGH-PRECISION $^{14}\text{C}$ LABORATORIES

Stuiver and Pearson (1993: 1) summarized various causes of such offsets of measurements:

The precision and accuracy of the  $^{14}\text{C}$  measuring process is limited, and dendrochronological errors (if any) may result in  $^{14}\text{C}$  age differences when materials of different chronologies (and “identical” AD or BC ages) are used. And although relatively fast transport in the troposphere causes atmospheric  $^{14}\text{CO}_2$  to be fairly uniformly mixed near the earth surface, small regional differences remain. General circulation and carbon reservoir model calculations [Braziunas 1990] ... predict regional “age” differences of maximally 20  $^{14}\text{C}$  years within the northern hemisphere.

Stuiver and Pearson (1993) treat the question of the precision and accuracy of  $^{14}\text{C}$  measurements in great detail. Discrepancies in dendrochronological dating are not unprecedented. For example, Becker and Kromer (1986: 961) explained the following case history:

In 1982 we noticed an offset of 70 years between the tree-ring time scale and the matching of its  $^{14}\text{C}$  variations to those of the Bristlecone Pine (Becker, 1983). In cooperation with the Belfast and Cologne Tree-Ring Laboratories, oak chronologies of northern Ireland, England, and northern and southern Germany have been cross-matched over the first and second millennium BC. After a correction of the Hohenheim series at 500 BC by 71 years, the Hohenheim and the Belfast oak masters evidently cross-date continuously over their critical bridgings of the first millennium BC (Pilcher *et al.* [ ], 1984).

#### REGIONAL EFFECTS

An interest in regional effects began in the latter part of the fourth decade of radiocarbon dating. Previously, it was considered that the rapid mixing of the atmosphere would minimize this effect. This

was true before the advent of high-precision measurements in the third decade of  $^{14}\text{C}$  dating (Damon 1987). With the advent of high-precision dating and single-year dating, researchers have become more aware of real regional differences that are by no means trivial nor constant in time. For example, by means of high-precision  $^{14}\text{C}$  analyses in this laboratory, we first noticed a significant difference between coeval individual tree rings from the Santa Catalina Mountains near Tucson and the Olympic Peninsula (Damon, Cheng and Linick 1989; Stuiver and Quay 1981). Another comparison of single-year data had shown no significant difference between dates for bristlecone pine from the White Mountains of California and Douglas-fir from the Olympic Peninsula (Linick *et al.* 1986). However, upon plotting the previous single-year  $\Delta^{14}\text{C}$  data [T (89) – S (81)], we observed that the data fell on two lines with high correlation. The offset on one line was 1.5‰ (12 yr) and the offset on the other was 4.5‰ (37 yr) with the average offset at 2.5‰ (21 yr). The Olympic Peninsula tree rings were relatively depleted in  $^{14}\text{C}$ . We concluded that "This difference of 2.5‰ would not be difficult to rationalize as the result, eg, of a mixture of only 2.5% of 10%  $^{14}\text{C}$ -depleted  $\text{CO}_2$  derived from the marine upwelling with undepleted  $\text{CO}_2$  in the prevailing air masses of the Olympic Peninsula" (Damon, Cheng and Linick 1989: 712). A depletion of 4.5‰ would require mixing of 4.5% of 10%  $^{14}\text{C}$ -depleted upwelling  $\text{CO}_2$  mixed with undepleted  $\text{CO}_2$ . Naturally, it occurred to us that the increased upwelling of  $^{14}\text{C}$ -depleted  $\text{CO}_2$  might be related to El Niño events. Jirikowic and Kalin (1995), who were then Ph.D. candidates, formulated a statistical correlation but I declined to coauthor the paper because I could not find a visual correlation between specific El Niño events and evidence for increased upwelling in the data.

Fan *et al.* (1983, 1986) concluded from their  $^{14}\text{C}$  analyses of tree rings at 60°N, 130°W comprising 1–3 annual rings that the data "exhibit a 10‰ fluctuation with an 11-year periodicity anti-correlated with the solar activity cycle" (Fan *et al.* 1986: 300). However, from measurements on annual rings of Douglas-fir from the Olympic Peninsula, Stuiver and Quay (1981: 353) concluded that the 11-yr  $^{14}\text{C}$  cycle was "not much beyond the uncertainty of the measurements (ca. 1.5‰)". We decided to check the results of Fan *et al.* (1986) on annual tree rings from the same location just above the Arctic Circle (Damon *et al.* 1992). Our data agreed with that of Stuiver and Quay (1981) concerning the magnitude of the 11-yr  $^{14}\text{C}$  cycle but with a depression of  $2.6 \pm 0.9\%$  relative to the Olympic Peninsula. We ascribed this to release of  $^{14}\text{C}$ -depleted  $\text{CO}_2$  during the late spring-summer thaw.

If we use the Olympic Peninsula as our frame of reference, tree rings at high elevation (2740 m) from the inland Santa Catalina Mountains near Tucson are elevated in  $^{14}\text{C}$  by as much as 4.5‰ relative to tree rings from the Olympic Peninsula. On the other hand, tree rings from near the Arctic Circle in Mackenzie Valley are depressed by 2.6‰ relative to the Olympic Peninsula. This implies that  $\text{CO}_2$  in tree rings from near the Arctic Circle are depressed in  $^{14}\text{C}$  by as much as 7‰ relative to the inland Santa Catalina Mountains at an elevation of 2740 m. This would result in a maximum difference of 58 yr and is almost three times the maximum regional effect in the northern hemisphere predicted by Braziunas (1990). This difference would not be constant in time but would vary within at least the range of 4‰ to 7‰ (33 to 58 yr).<sup>1</sup>

## CONCLUSION

1. In arriving at their 1993 calibration, it appears that Stuiver and Pearson (1993) made changes that increased the average age of their samples by *ca.* 20 yr. This brought the two laboratories into excellent agreement with each other in the overall range of AD 1840 to 5180 BC ( $+2 \pm 1\%$ ). However, it resulted in a 20-yr offset between B (93) and B (86) as well as for B (95) and T (95).

<sup>1</sup>The reader interested in the regional effect should also refer to McCormac *et al.* (1995).

2. The high-precision laboratories at Belfast, Groningen, Pretoria, Seattle and Tucson are able to attain an accuracy of  $\pm 1\%$  or better and a precision of  $\pm 3\%$  or better including an error multiplier of 1.5 to account for non-Poisson errors.

3. The H (86) offset from S (93) is large,  $-41 \pm 4$  yr (4075–5265 BC and 5805–5995 BC). However, the tree-ring chronology was then still floating and linked only by  $^{14}\text{C}$  wiggle-matching. Consequently, in Table 1 of Kromer *et al.* (1986: 955), the authors list dendrochronological year as “Age (BC) (approx)”. The large offset of B (93) data,  $-54 \pm 5$  yr, occurs during 5180 to 5500 BC. This offset occurs within the 2680-yr extension of the oak chronology since the B (86) data. The measurements were also done in the new Belfast laboratory with LKB Wallac (Quantulus) counters. There is as yet no explanation for this large offset.

4. At the present state of the high-precision  $^{14}\text{C}$  measurement art, data should be mixed only in formulating calibration tables and graphs when the laboratories concerned have demonstrated their ability to obtain precision of  $\pm 3\%$  (24 yr) or better and where offsets between data sets are  $< 10$  yr. Of course, data of lesser precision and accuracy, if they are the only means of extending the calibration curve, should not be excluded. However, until the data are verified by two or more laboratories, such extensions (*e.g.*, Becker 1993) should be temporarily accepted, and with appropriate skepticism. This philosophical bias of ours led the late Professor Suess to state that “it was pointed out by Damon *et al.* (1978) that ‘no single deVries-type fluctuation prior to the Medieval Warm Epoch has been confirmed by two or more laboratories’ and also that ‘wiggles (deVries type fluctuations; *omitted in quoting*) reported by Suess have not been confirmed.’ This, and probably other factors, then led to the U.S. National Science Foundation to deny repeated requests for further financial support” (Suess and Linick 1990: 411). I am pleased to say that most, but not all, of Professor Suess’s wiggles have been confirmed subsequently. However, I stand by our philosophical principle. Undoubtedly, deVries-effect-type fluctuations (wiggles) during and after the Medieval Solar Maximum had been proven to exist. Those wiggles had been amply confirmed (see Table 4 of Damon 1987). Results from one laboratory are interesting but an essential factor in science is confirmation!

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