Zeitensprünge

Interdisciplinary Bulletin
Off-Print

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The Self-Deception of the C14 Method and Dendrochronology

from ZEITENSPRÜNGE 8 (1996) 3 361-389

January 1997
DM 10,-

Highlight
"Absolute Dating"

MANTIS Verlag

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Author's note: This article reflects the discussion up to November 1996. A more detailed position is worked out since then. Nevertheless we publish this version and refer to the actual discussion in ZEITENSPRÜNGE and to the book "C14-Crash" to be published in spring this year.

The use of C14 instead of ^{14}C: It has been our experience that the whole world talks about "Cee-fourteen" (= C14), but not about "radiocarbon" or "fourteen-cee" (= ^{14}C). Furthermore, everybody means the same thing with that, namely either the radioactive isotope of carbon ^{14}C or the method of determining absolute age based upon the measurement of its radioactivity. Therefore, we will follow this custom to make it easier to read and speak about it.

Source of text: This is a translation of "Der Selbstbetrug von C14-Methode und Dendrochronologie" originally appearing in ZEITENSPRÜNGE 8 (1996) 3 361-389 and also as an off-print (MANTIS edition) together with "Die 'magic dates' und 'secret procedures' der Dendrochronologie" by H.-U. Niemitz and "Dendrochronologische Zirkelschlüsse" by H. Illig. The main body is professionally translated while small parts of it are edited or supplemented by the authors themselves. Part 9 is based completely on the authors' English.

The Self-Deception of the C14 Method and Dendrochronology

How Dendrochronology Has Been Lulled into a False Sense of Security by an Urgently needed Auxiliary Science

Christian Blöss • Hans-Ulrich Niemitz

Figure 1: Graphic in Libbys 1st data list 1951 in SCIENCE

What was announced as a "C14-article" at the annual chronology-meeting in Hamburg has developed into a second "dendro-article". The previous one also appeared in this magazine last year [Niemitz 1995, see also Illig 1991]. The insider knows that the C14 method would have been long lost had it not been for the intervention of dendrochronology: a C14 measurement has to be calibrated, and it is only dendrochronology which supplies the required source of comprehensive calibration (see figure 4). Without this assistance, the C14 method would have lost its reputation as the most reliable method of determining the absolute age of historical artefacts for the most recent 50.000 years.

In contrast, it is hardly known that without C14 dendrochronology would never have been able to bring about a complete tree-ring sequence for the Post Glacial Age.

1. How Dendrochronologists Have Humbled Themselves to the Traditional Chronology of Europe

The dendrochronologists' predicament, that there are floating tree-ring sequences which could not be predated, was and is great. The floating chronologies were supposed to have been worked into a tree-ring sequence covering the entire Post Glacial Age which was to be complete in the end. If an unknown sample of wood were investigated for its

usefulness in extending the "master" (the standard sequence) and there was no indication of which area of chronology it belonged to, the "a-priori probability of finding the correct date [that is, the correct synchronous layers] was so small that there was little chance of actually finding it" [Hollstein 1970, 147]. For Hollstein, it was not reliable to work out a tree-ring sequence without it being predated by historical sciences since without outside support an approach such as this held the danger of improper dating if the wrong "synchronisation" were accepted from among the many thousands of possibilities.

Most of Hollstein's colleagues relied upon the C14 method when predating, with whose help dendrochronologists, lacking any wood artefacts which could be dated absolutely, planned to push ahead into the early Post Glacial Period. H. Schwabedissen [1983, 284] remarked that investigations by C14 physicists and dendrochronologists alone can not lead us to our goal. Rather "consistently competent archaeologists" would have to be called in.

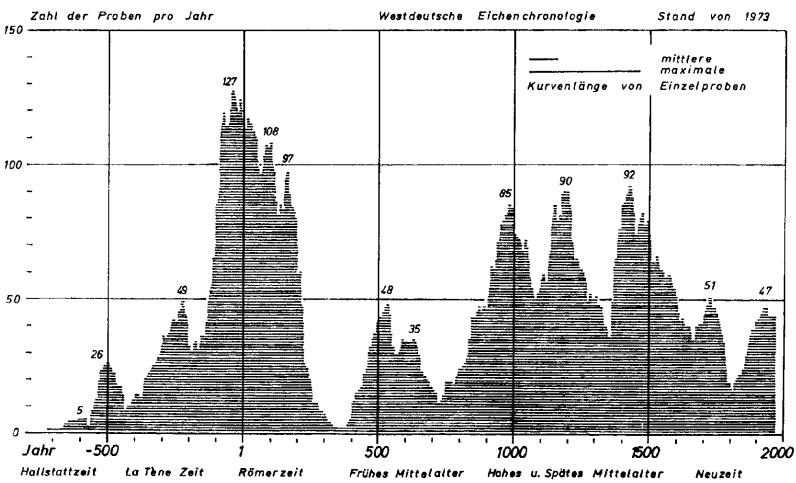


Figure 2: Record of chronological cover for the Middle-European oak chronology. Note that at 380 and 720 AD the true cover is 2 and 4 specimens respectively [for details see Niemitz 1995, 305 and Illig 1991, 128].

We agree with Hollstein's opinion about the fundamental difficulties of accepting the synchronous layers for a tree-ring sequence without seeking premises: it is simply not possible to be successful at synchronising without predating. On the other hand, we reject the traditional chronologies being accepted unreserved by dendrochronologists such as Hollstein who thoughtlessly submit their synchronous layers to the regime of a Christian calendar which came about based on dubious criteria. If one wishes to rely upon the help of other methods, one has to be sure that they are suitable.

Dendrochronology believes itself to be on absolutely safe ground when it relies upon historical data which are integrated into the context of European history. Comparisons are made until an adequate synchronous layer is found. This is what dendrochronologists like to call "successful" synchronisation [see for instance Becker/Schmidt 1982, 104]. However, even "convincing synchronisation" [Schwabedissen 1983, 282 on the Master from "Kirnsulzbach"] proves to be false dating in the final analysis [a summary, for example, with Leuschner/Delorme 1984, 234]. H.-U. Niemitz has described the alarming frequency of oddities in the period of the so-called "mass migration gap" [1995; also Illig 1991] which are also unacceptable according to the internal criteria of dendrochronology and for which it will probably only be possible to be solved after other synchronous layers can be freed from the principle of "traditional chronology" (see figure 2) .

When dendrochronologists are asked about their dependency upon predating, they usually state it is not important that an auxiliary science which is consulted for predating tree-ring sequences has to be valid in the end since the methodologically highly reliable standard of dendrochronology will be applied exclusively in the end. In contrast to this are not only the errors and contradictions in the individual tree-ring sequences which have been negotiated openly and corrected afterwards, but also those that can be only recognised indirectly in the tree-ring chronologies. It is just as proper to ask the question of what this auxiliary science is used for if it doesn't have any effect in the end. The assertion that C14, by presetting an event, does not produce any predisposition towards a decision on the later synchronous layer, is simply wrong. It is urgently advised to look at the suitability of the C14 method as an auxiliary science for dendrochronology.

2. Under What Conditions C14 Functions ...

The idea for developing the C14 method arose as W.F. Libby recognised in 1939 that 1) the steady and uniform production of radioactive C14 in the atmosphere (as a result of normal atmospheric nitrogen N14 being bombarded by slow neutrons from cosmic rays) as well as 2) its unusually slow radioactive decay would have to produce there a proportion of C14 and normal carbon C12 which is globally and locally almost stable and uniform.

R.D. Long remarked correctly that we would only be entitled to make this assumption if nature were organised in a fundamentally uniform fashion [Long 1973, 125]. This would mean that in all living organisms the same ratio of C14/C12 would be present, exactly as this ratio has to appear constant over time in atmospheric CO_2.

If an organism ceases exchanging materials with the outside world ultimately when it dies, it particularly stops to exchange any carbon atoms with the surrounding. Although the C14/C12 ratio should remain dynamically stable in the outer world, it now decreased exponentially within the organism. The longer ago it was that an organism ceased exchanging materials, the lower the share of C14 atoms would be in relation to the C12 atoms present in it. It was possible to calculate the time that has past since its death from the measure of that lower ratio between C14 and C12. This means that it should in principle be possible to determine the point in time when a sample ceased exchanging materials from the measurement of the remaining radioactivity.

The evidence of the C14 method was originally made dependent on the following 5 prerequisites:

1. *Measurability:* The C14 radiation to be measured must differentiate itself distinctly from the background radiation in order to measure exactly and to receive a definite determination of age. (Problems of C14 laboratories with their results on replicated measurements)
2. *Cutting Off:* During its storage period between the time when it died and the corresponding investigation today, the sample under investigation may not have had any exchange of carbon (Problem of contamination)

3. *Spatial Invariance by Instantaneous Distribution:* There must have been the same C14/C12 ratio in all organisms which lived simultaneously at different places (Problems arising from comparing different hemispheres, reservoir effects).
4. *Organic Invariance:* There must have been the same C14/C12 ratio in all different organisms which lived simultaneously at the same place (Problem of "isotopic fractionation").
5. *The Fundamental Assumption:* The C14/C12 ratio must have always been the same in the past. From this follows the statement: "The appropriate age can be directly calculated from a C14 value." (Problem of the "Suess Effect", C14 fluctuations around the theoretical value in long tree-ring sequences).

There are some other assumptions which are, of course, less decisive, which we will not deal with here. We refer to our book on this topic which will appear shortly. As long as these prerequisites could be looked upon as fulfilled, the formula of "one measurement is one date" applied. The immense fascination which drew laypersons and scientists working on questions of dating rested upon this efficient nature of the method expressed here in the form of a formula: without looking at the 'before' and 'after', the 'above' and 'below', without weighing the 'more' or 'less' in samples against one another, it was possible to discover the *absolute* age of a sample in a direct fashion by means of one single measurement!

Aitken's often rendered statement that "one date is no date" [1990, 95] makes it clear that people have dissociated themselves from the rigorous validity of the prerequisites named here. With the exception of the third prerequisite of "spatial invariance by instantaneous distribution", none of them is "officially" valid anymore. In addition to that this third prerequisite is the most important prerequisite for applying the C14 method because simultanous tree-ring sequences have grown always more or less *spatially separated from one another*. At least sequences growing thousands of kilometres apart were compared for their C14 dates under the prerequisite 3! If this possibility of meshing were no longer to exist, the alliance between C14 and dendrochronology would have to break apart.

3. .... and How C14 Fulfilled These Prerequisites and Fulfils Them Today

The history of the C14 method is simultaneously a history of the criticism of the practising laboratories. The question "error in measurement or not?" can apparently never be dealt with unemotionally. This becomes clear when we for example read J.G. Ogden III's remarks about how the results of measurements from his laboratory were accepted: "It may come as a shock to you, but fewer than 50 percent of the radiocarbon dates from geological and archaeological samples in northeastern North America have been adopted as 'acceptable' by investigators" [Ogden 1977, 173]. We also quote R.M. Clark as another example of the standard of the measuring laboratories' errors: "Thus there can be no doubt that on average the variability between replicate observations *is far in excess* of the variability expected in view of the quoted standard errors" [Clark 1975, 252; same statement Clark 1979, 52; emphasis added].

Clark's estimations come from a time when the completion of the European oak chronologies using the C14 method had come into our immediate grasp. Ten years later, after it had seemingly already been completed, the decision was finally made to carry out a more precise investigation of systematic deviations between the measuring laboratories. Some of the measuring laboratories evidenced deviations in their measured values that were so alarming that they had to acknowledge that their image had been damaged. "It may be yet a few years before the C14 community can repolish its somewhat tarnished image. The important thing is that we have begun a process of self-healing" [Long 1990, iii]. At this point we naturally ask about the self-healing process for the tree-ring sequences drawn up using C14.

Prerequisite No. 2 (Cutting Off) touches upon the large area of "contamination". Although here the most drastic errors have actually become obvious, we are of the opinion that this is only a minor important scenary to distract people from the actual problem. Nevertheless in our context it becomes controversial when we ask the question if the annual rings can absorb C14 from earlier rings or, as the case may be, if the annual rings can give their C14 to earlier rings. In a systematic investigation using a sample of the Californian bristlecone pine which is so very important for

dendrochronology, it was shown that carbon was diffused in over 100 annual rings from the sapwood to heartwood areas [Long et al. 1979, 536].

We will skip over prerequisite No. 3 (spatial invariance, etc.) and dedicate ourselves to No. 4 for a moment, which originally also made a demand on invariance: Regardless of the type of metabolising organism, the relation between C14/C12 occurring in the atmosphere or in water was to appear again in the same manner in all living things - leaving spatial differences completely aside. But even Libby had to differentiate between simultaneously living shells and wood because the wood shows - luckily in a systematic fashion - less radioactivity than the shells. Of course, the recently felled wood seemed to have had a corresponding difference in age of 600 years by which they were too old from a radiometric point of view. The phenomenon that various organisms have a different preference for each of the various carbon isotopes is designated "isotopic fractionation". In practice it should be corrected before each statement on age is made.

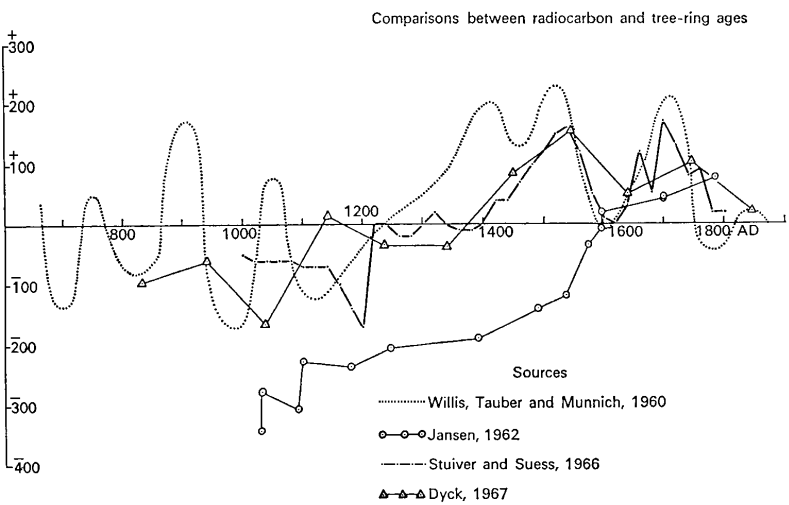


Figure 3: Four curves comparing recent radiocarbon and tree ring ages plotted on a common scale contradicting altogether the central Simultaneity Principle. The lines are primarily a visual guide [from Shawcross 1969, 190].

The "Fundamental Assumption" which is listed as the prerequisite No. 5 had to be watered down only a few years after the method was introduced in its general version. It was recognised that both the increased burning of natural fossil resources since the start of the Industrial Revolution as well as the latest atomic bomb tests have led to a sometimes dramatic shift in the relationship of C14/C12. Originally this relationship was looked upon as a temporal constant. In the course of the sixties, it was recognised that it was necessary to water down the Fundamental Assumption still further as fluctuations were seen even for the time before the Industrial Revolution.

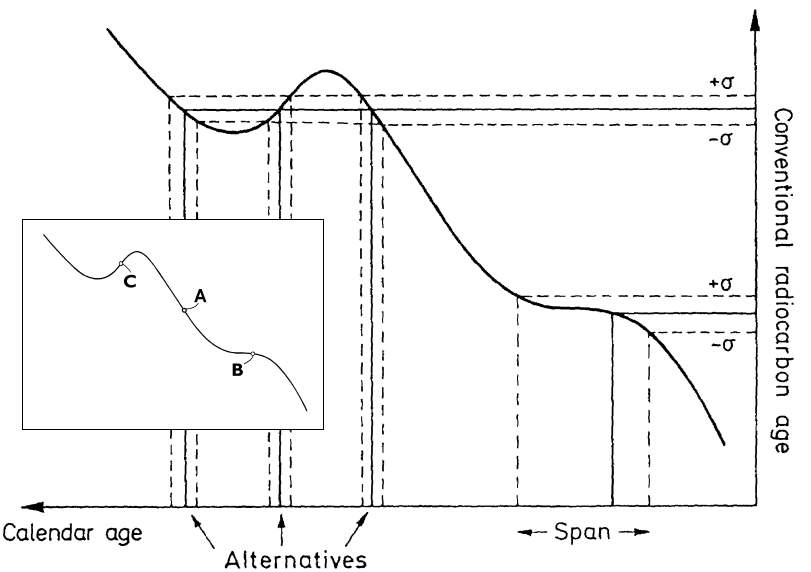
Finally, C14 measurements on trees recently felled and especially old which therefore had ring sequences reaching far into the past and which of course could be measured with regard to C14 made it necessary to make the transition from the Fundamental Assumption to the so-called "Simultaneity Principle" which was weaker [as an example Willis et al. 1960]. But see figure 3 which demonstrates the fundamental problems with that principle.

The Simultaneity Principle, which succeeded the Fundamental Assumption, only made the statement "that radiocarbon dates are the same at any given epoch over the entire earth so a calibration at any one locality is equivalent to a world wide calibration" [Libby 1970, 9]. We shall interpret this Simultaneity Principle directly with regard to dendrochronology: while the validity of the Fundamental Assumption means that two different tree rings of random origin which have the same C14/C12 ratio must be *of the same age*, the Simultaneity Principle only allows the following statement: Two distinct tree rings of different origin and of the same age have the same C14/C12 ratio. The reverse conclusion - having the same values for the C14/C12 ratio automatically means the same age - is now no longer permissible (for the procedure of calibration see figure 4).

This meant that not only the method's elegance but also its independence was gone. In 1960 there was no one calibration scale which reached into pre-Christian times. It would take just under 10 years until the first tree-ring chronology was drawn up for this purpose in the USA. In Europe it took longer. A comprehensive independent tree-ring

Figure 4: Calibration of the C14 Statement of Age

The representation of a calibration curve for C14 data using a tree-ring chronology shows the dendro-years as *calendar age* on the (horizontal) abscissa and the C14 years calculated from the measured *radioactivity* of the corresponding annual rings on the (vertical) ordinate. Initially, it was thought that checking the C14 data of an annual ring sequence would prove that the same C14/C12 ratio had existed at all times. In that case, calibration would have been unnecessary and the calibration curve would have been identical with the bisector of the cartesian cross in question. On the other hand, if the C14/C12 ratio fluctuated over time, then the calibration curve could deviate from the bisector of the angle in a certain way. Indices exist that the calibration curve should be steeper and corresponding artefacts therefore younger than conventionally given (see figure 10 and discussion in chapter 7).



A C14-age calculated from the activity reading is calibrated by a horizontal line being drawn towards the left starting from its location on the ordinate. All of the points intersecting with the calibration curve theoretically represent a potential absolute age. This can be read in each case from the abscissa. The corresponding information also applies to the error interval that must be given for the reading. The fact that a measured C14 age is ambiguous comes from the curve shape C (falling C14 age with increasing actual age).

See chapter 7 for a discussion of the 3 marked patterns A, B and C of the calibration curve in relationship to the production rate of radiocarbon.

chronology was available only after about 25 years. The C14 scientists had a decisive share in its construction. Why were dendrochronologist so urgently dependent upon the assistance of C14?

4. Why Dendrochronology Needs C14 ...

Trees which form yearly tree rings grow rings of varying thicknesses year for year depending upon the specific climate. This produces tree-ring sequences which are typical not only for each type of tree, but also for each region and epoch (microclimates). We will be primarily looking at the conditions in Europe. Therefore, tree-ring sequences which have grown at the same time and in neighbouring areas can be combined to what is called "local masters" (see figure 5). Although some Irish oaks may be correlated up to a distance of 70 kilometres from the place where they were found [Smith 1972, A92], the distance for comparing the master locations of the Danube and the Upper Main has increased by more than two times [Becker/Frenzel 1977, 16]. "Local masters" which have been verified well and for a long time can be synchronised over a distance of up to 300 kilometres [Hollstein 1977, 16]. However, non-local comparisons, such as those between the southern and northern German regions, have shown that various oakwood chronologies are not applicable [Eckstein 1984, 40].

The individual characteristics of single tree-ring sequences have been filtered out of local masters such as these and are therefore typical tree-ring sequences which generally only include a limited period of time (typically some hundreds of years). They are the building blocks of the only absolute tree-ring chronologies which are to be drawn up. Nonetheless, they must remain local. It is not without reason that they are designated "southern German", "western German", "northern German", etc.. As long as the local masters were not synchronised in relation to one another, they remained as "floating chronologies" without an absolute date. On the local level, when a local master is drawn up, experience shows that one moves ahead quickly and reliably. The ring sequences are generally long and synchronisms can be recognised in a statistically significant fashion. Experience has shown that it is fundamentally more difficult to combine these local masters to a regional chronology among one another. Although we can derive the temporal combination from

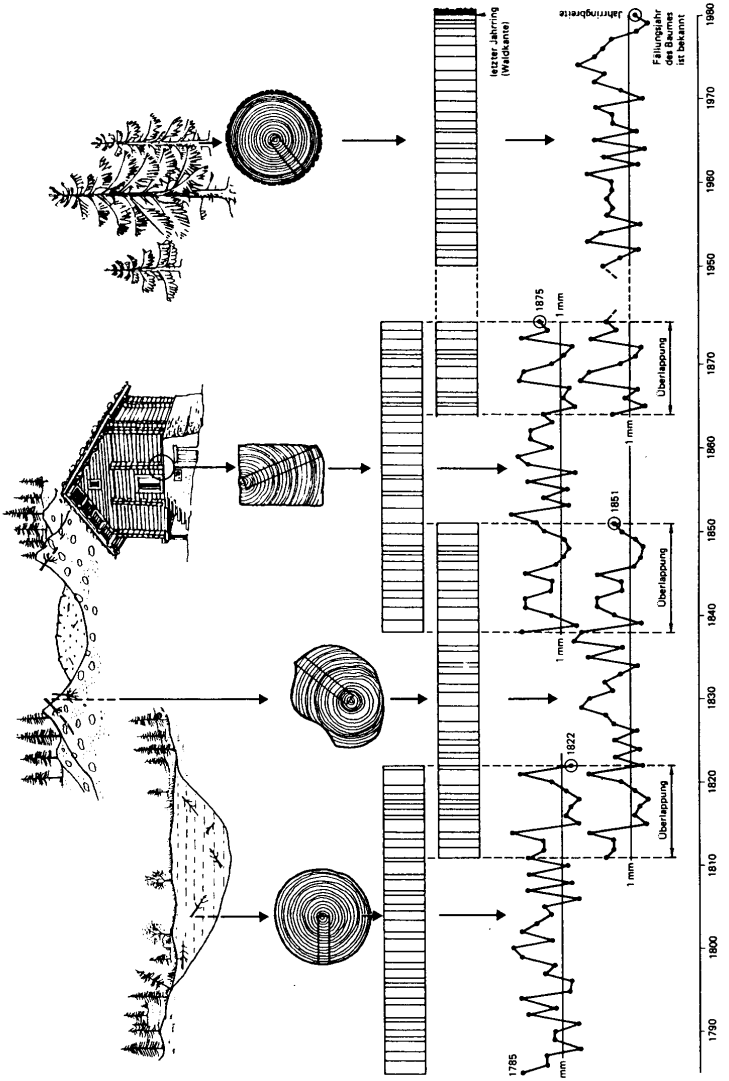


Figure 5: Method of spanning time by comparing yearly tree-ring sequences [Schweingruber 1983, 85].

stratigraphic evidence locally, there are no such aids on the regional level. What can you do if these aids are missing? This is dendrochronology's crunch question: should all of the layers be checked for synchrony or should we rely upon the aid of predating?

If that means that a particular time is poor in discoveries and that it is therefore difficult to bridge the gap between the bordering masters which are already present, then this question has already been decided. Predating was done (this is how the "mass migration gap" [Hollstein 1970] came about, for example) and avoided doing all that expensive and time-intensive 'detailed work' of going through all of the sychronisms one could imagine. The Irish dendrochronologists, for example, had a curious problem when they used C14 predating for their local floating tree-ring chronologies: the longer they worked and the more wood samples they gathered, the more difficult it was to classify the new wood samples. If they worked properly, there should have been the opposite effect, namely: the more wood samples they gathered, the easier it should have been to classify the new wood samples. However, the Irish alarm bells haven't rung yet.

We would like to present another example to the reader to make dendrochronology's dependency upon C14 clearer: the position of the floating sequence "C" of the southern German oak chronology - it included some 2.350 years at that time [Becker 1980, 219] - was temporally anchored on the basis of the Fundamental Assumption (principally with only one C14 value) with the approximate date of "900 before Christ" for the most recent ring. After synchronizing a number of connected C14 values with corresponding values of a ring sequence on the other side of the Atlantic (what is only legitimate if the Simultaneity Principle is entirely correct!), this date but not that of the neighboured sequence B shifted by just under 1.000 years into the past (see figure 6). After later dendrochronological interlinking, the accuracy of this immense shift was verified by means of an inconspicuous correction of less than 10 years [Linick et al. 1985, 21]. If dendrochronological interlinks were above all suspicion, in retrospect the Simultaneity Principle would then have been brilliantly confirmed along the whole line. However, if this principle proved to be wrong, that would mean that dendrochronology would have to put up with some critical question, for instance if it really is of the

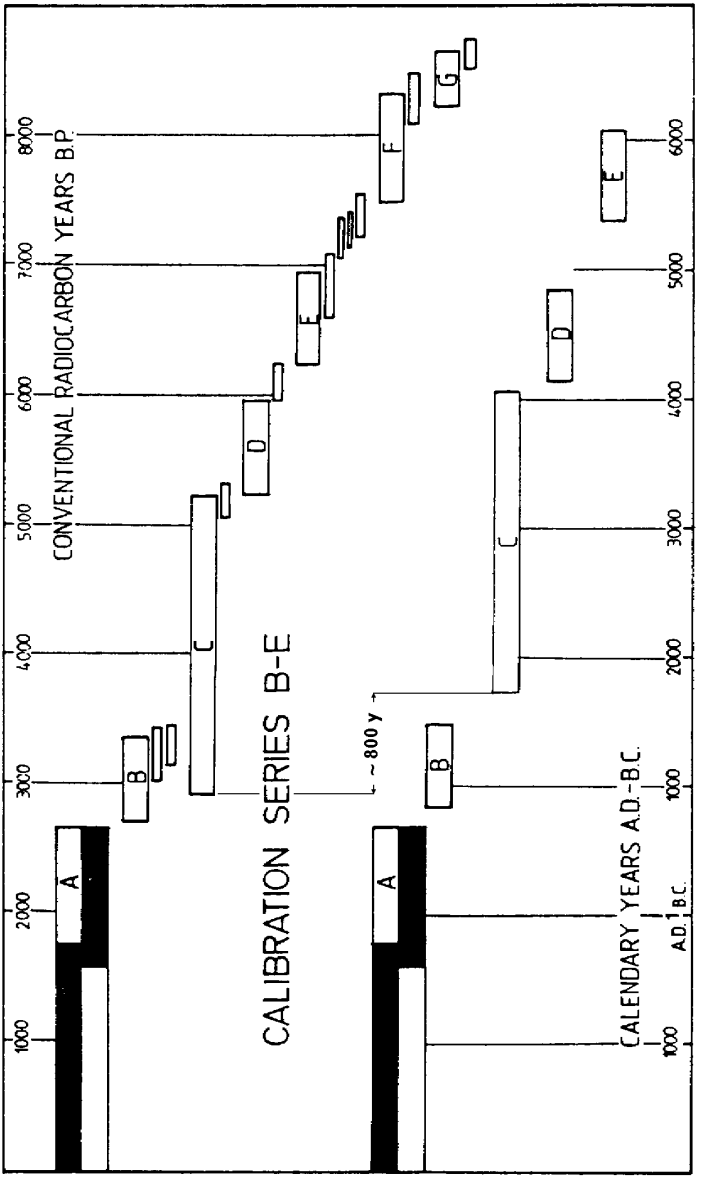


Figure 6: The stage of the Holocene oak tree-ring chronology in South Central Europe in 1980. Upper part: floating chronologies plotted according to their conventional radiocarbon ages BP. Lower part: the same series plotted according to their radio carbon age BC arrived by comparison of their C14 pattern to those of the American bristlecone cone pine [Becker 1980, 220].

opinion that a wrong - or perhaps better: corrupted - C14 result was so accurately fitted by pure coincidence?

5. ... and How Dendrochronology has Made Itself Dependant Upon C14

In 1966, a team consisting of two dendrochronologists and a person versed in the practical use of C14 demonstrated in a key article on methods what the auxiliary science C14 will bring for dendrochronology in future [Ferguson et al. 1966]. An undated, therefore "floating" master chronology from Thayngen in Switzerland as well as Burgäschi-South was historically predated as Neolithic and was synchronised using a set of C14 data by means of an American annual ring chronology (see figure 7). At the time, this chronology was looked upon as finished. This means that this master chronology received an absolute date which was confirmed to the year almost exactly by means of the later approaching European chronology [Becker 1992, 38]. This floating tree-ring chronology formed a first building block in a central European oak chronology which was almost completely prestructured via America. Only a little bit less

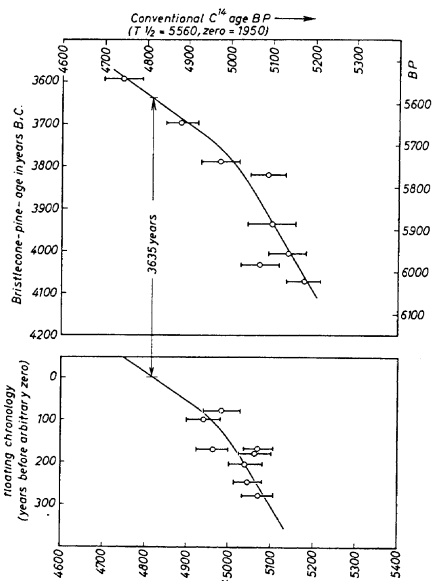


Figure 7: Absolute dating by using a C-14 pattern comparison method called "wiggle-matching"

The upper figure shows the C14 data from the bristlecone pine chronology [Ferguson 1969] and the lower figures shows the C14 data from samples from trees from Thayngen (and Burgäschi) [Ferguson et al. 1966].

We do not understand the bend in the balancing curve for the data mentioned last and we have also not been able to find anyone as of yet who has been able to explain the validity of procedure here - leaving aside all of the problems in method of the C14 comparison in samples.

than 20 years later, as filler sequences were found in time for all of the gaps, it was finally secured for dendrochronology.

In 1966, German dendrochronologists discovered that the *Tree-Ring Laboratory* in Arizona had worked out the longest continual tree-ring chronology up to that time for Pinus aristata (bristlecone pine). H.E. Suess, a C14 scientist - co-author of the key article on methods from 1966 - carried out C14 calibration using this tree-ring chronology. In doing so, he arrived at the conclusion that the assumption of a constant C14/C12 ratio in the past was only acceptable in very limited cases. This meant that dendrochronology moved into first place in the "fraternal competition between the two methods of dating", as B. Huber, the German forest botanist and dendrochronologist, indirectly hinted [Huber 1966, 1].

Long before 1966 H.E. Suess had pleaded for dropping the Fundamental Assumption. Beyond that, he was the first person who consistently demanded that calibration curves be worked out (see figure 11). Since at least 1963 Suess has regularly carried out measurements for the *Tree Ring Laboratory* in Arizona. In 1965 he published the first and the most recent calibration curve which extends over 2.000 years. This curve made one thing clear: The author would accept certain fluctuations, however the concept of a basic imbalance between the production and disintegration of C14 appeared unthinkable to him. His calibration curve meandered "faithfully and truly" along the bisector of the angle which represents perfect balance between disintegration and production which remains static.

In 1966, Suess was among the most progressive chronologists. He was one of the first persons to be aware of the fact that it is only possible to predate a sample of wood using several C14 surveyed tree rings. The C14 values produced a pattern of fluctuation - the so-called "wiggles" - which could be used for dating if it agreed with the fluctuation pattern of another wood samples. The dendrochronologists were used to a similar procedure with the pattern of thickness of their tree ring samples. In a forced march which left the stagnating Europeans behind, C.W. Ferguson from the *Tree Ring Laboratory* in Arizona was incidentally also a co-author of the key article 1966 and set up his tree-ring chronology which at

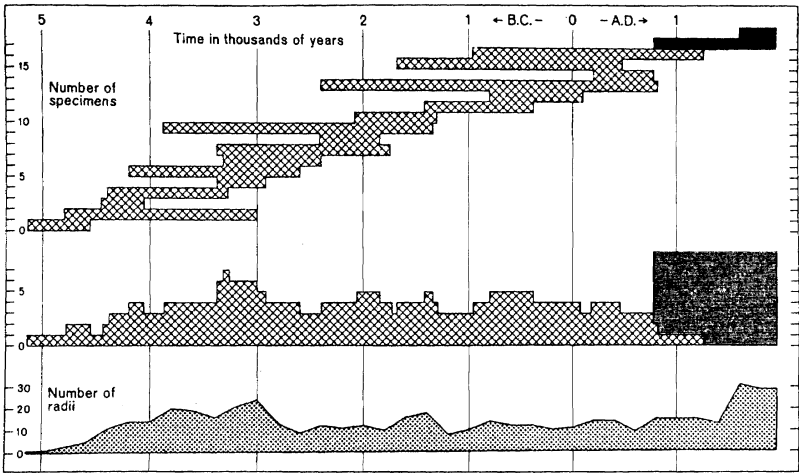


Figure 8: Components of the master chronology known as the "bristlecone pine": (upper) the time interval contained in each tree specimen; (middle) specimen depth throughout the whole chronology, expressed as a histogram-compaction of the upper bar-intervals; and

the end extended over more than 7.000 years. He required only about three years for that, a measure the European Dendrochronologists could only dream about.

How could he have been so successful so quickly? We have to assume that the Pinus aristata tree-ring chronology itself was built up through C14 sample comparisons, which was at that time the most modern procedure and the one which promised the best chances of success; apparently, the Pinus aristata tree-ring chronology was only meagerly verified on the basis of dendrochronological criteria. The publications in this area are rare and only rudimentary. Even in the main publication from 1969 the exact dendrochronological information for this tree-ring chronology is missing [Ferguson 1969; 1965 as well].

The quick success in constructing the Pinus aristata tree-ring chronology is surprising since we know that the bristlecone pine (Pinus aristata) has much worse dendrochronological characteristics than European oaks

(ring thicknesses in the μm instead of the mm range, up to 5% missing rings, only a fraction of all of the rings can be evaluated, changing the drilling head within a tree, significantly lower density of samples, etc., see figure 8 for the inadequate small number of used tree-ring sequences) That should have increased the suspicion that the designer of the Pinus aristata tree-ring chronology required C14 as an aid even more than those for the European oak chronology. Without any possibility to "wiggle-match" he based the raw construction on the actualistic dogma, that a radiocarbon age is always nearly the true age.

In the attempt to understand the genesis of the European oak chronology, we recognised rather quickly that all of the relevant oak chronologies - partly after a lengthy rejection - had gone through a phase of "tentative absolute dating" of the corresponding floating sequences by comparing C14 samples with the tree-ring chronology of the American bristlecone pine. That confirmed our former estimation that there will be no success without some help of predating methods. We could only wonder that the Europeans were so naive in trusting the Americans. To be fair, we also have to point out that in the beginning the Europeans delayed and fundamentally rejected this type of dating to a certain extent. This resistance only subsided in the 70's. B. Becker used the Pinus aristata tree-ring chronology after 1973 [Becker/Suess 1977], the Irish no later than the beginning of the 80's [Baillie 1983] only after a hefty dispute which was carried out in the periodical NATURE [Pearson et al. 1977].

In all publications which referred to Ferguson's bristlecone pine chronology in any form, we encountered an unshakeable belief in its correctness. However, where the question of methodological reliability had to be asked, the authors constantly referred to LaMarche and Harlan [1973] confirming Ferguson's chronology. (LaMarche and Harlan managed to prepare their own chronology within a very small number of years from 118 ring sequences after apparently one single collecting period ...) Ferguson undermines the arguments of his opponents himself since he was only able to state that there "is no conflict" between LaMarche and Harlan's temperature-determined tree-ring sequence which came into being at the White Mountains' upper limits of tree growth and his own moisture-determined tree-ring sequence from the lower limits of tree growth [Ferguson 1979, 209]. These chronologies can not be compared with

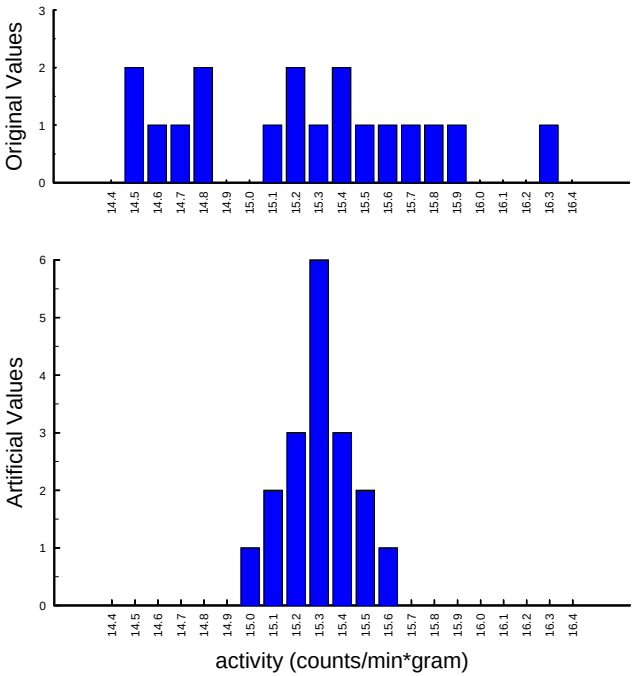


Figure 9: Histogram of the activity measurements carried out by Libby up to 1949 on 18 contemporary woods

The upper range shows the values actually measured, the lower range shows the values that should be expected in accordance with the stated mean value of 15.3 ± 0.1 counts/min*gram (= incidents of disintegration per minute and gram of carbon; in the form stated in the 1952 book "Radiocarbon Dating"). The variety of the actual measured values gives no credit to deal with a gaussian distribution underlying as Libby wanted to have. A statistical test would have shown little chance for the hypotheses of simultanousness to Libby. Instead he decided to interprete this as yet not high enough probability to throw his hypotheses away.

It seems to us that the whole scientific world *wanted* to be persuaded with that unquestionable brilliant idea of absolute dating.

one another according to dendrochronological criteria since the ring thicknesses are dependent upon different climactic factors.

6. Betting on the Wrong Horse or The Simultaneity Principle is Wrong

Everything would have been different if W.F. Libby had done his homework properly in 1949. That consisted primarily of verifying the Simultaneity Principle in the sense of the spatial invariance of the C14/C12 relationship in organisms which are metabolising simultaneously. Libby had arranged for this revision to be carried out using living organisms because it was the basic prerequisite for an intensification in the direction of the Fundamental Assumption. If it was not even possible to prove spatial invariance for today, then the hypothesis on spatial *and* temporal invariance, which had much more far-reaching consequences, would be totally meaningless. 1949, Libby had measured the C14/C12 values in samples from 18 modern wood species of global origin. In an article for SCIENCE [Libby et al. 1949] he was able to report that the test for spatial invariance was passed with flying colours: we could assume a mean variation which only amounted to ± 50 years.

An unprejudiced analysis of the results of his measurements arrives at a totally different result: The range between the smallest and the largest values corresponded to a difference in the C14 age of approximately 1.000 years while the measured values are almost evenly distributed over this range without significant compression. The reason for a discrepancy to a flatteringly small variance of ± 50 years is to be found in Libby's methodological procedure. He based his analysis on the assumption of a normal distribution of the readings. That has the same meaning as the assumption that the improperly measured values have a mean variation around *one single* "true" value. (That was what Libby actually wanted to prove.) If that were the case, the measured values would have to have been distributed in an approximate bell shape (figure 9). In contrast, it is only the flat distribution, which is much worse, which depicts his *actual* measured values. It is simply not possible to view this as a normal distribution.

The result: Libby fudged. He applied the methods in such a fashion that things came out that he wanted to see: spatial invariance of the C14/C12 relationship in organisms living at the same time. He therefore created the initial situation in which the intensification of the - supposedly verified - Simultaneity Principle to the Fundamental Assumption was accepted as self-evident. This silly custom of underhandedly obtaining a date which the consumer considers reliable from a larger amount of readings which are in and of themselves disparate by means of an improper hypothesis is still cultivated to the present day. (We showed this using the example of the treatment of C14 data of what is known as the "Cadbury Massacre" [Campbell et al. 1979] when we presented our paper in Hamburg. We also refer to our forthcoming book.)

At the C14 conference in Uppsala in 1970 for any observer who had his eyes open the decisive Fall came. Here the question was also that of the Simultaneity Principle. Although this was confirmed by J.C. Lerman et al. in a paper on "C14 in Tree Rings from Different Areas" [J.C. Lerman et al. 1970, 295], H.S. Jansen, together with T.A. Rafter, presented measurements (see figure 10) on annual rings on a Kauri tree from New Zealand

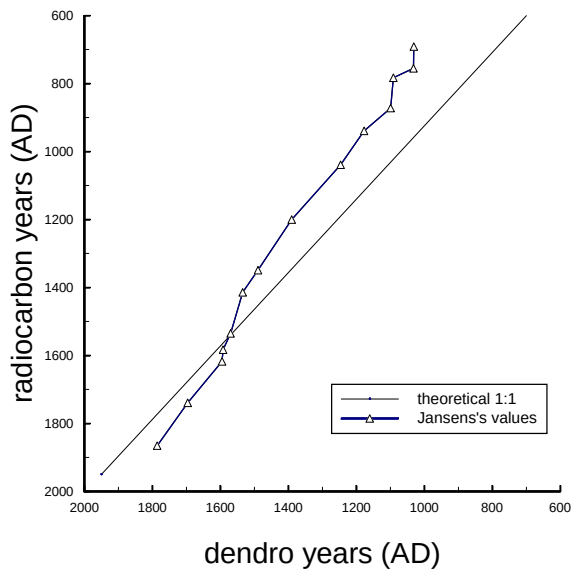


Figure 10: A calibration curve from a New Zealand Kauri-tree ("radiocarbon years" are given as conventional).

The curve is steeper than the bisector of the cartesian cross and indicates hereby an 45%-overproduction of C14. It contradicts the self-evident fit of the bisector as shown in figure 4.

which evidenced a completely different tendency than those of the American bristlecone pine (see figure 11) and which therefore disproved the Simultaneity Principle [Jansen 1970]. The calibration curve drawn up on the basis of these values deviated systematically from the bisector of the angle as an equivalent to the balance between production and disintegration of C14. It therefore evidenced a permanent increased rate of production in relation to the rate of disintegration by 45%. Jansen had already published the results of his measurements in the NEW ZEALAND JOURNAL OF SCIENCE [1962, 74ff.] without there being any reaction. In spite of the obvious controversial nature of the data presented, the debate recorded exclusively questions on the chemical preparation of the wood measured. There was only one place where Rafter admitted his uneasiness by expressing his "suspicion" that the southern hemisphere may run somewhat differently. P.E. Damon supplied the key word of "missing rings". This caused Rafter to say that adding approximately 50% of the existing rings as missing would set everything to rights.

Of course, the problem would not allow itself to be solved by adding missing rings. W. Shawcross, in an article for WORLD ARCHAEOLOGY, held this situation to the C14 scientists. At the same time, he expressed the concern that "one might shudder at what prospect would be unfolded by another, older, *kauri* log" [Shawcross 1969, 191]. Shawcross, who is a historian by profession, would have been glad to take recourse to the auxiliary science of C14 for New Zealand since there were difficulties in drawing up an absolute chronology for the time before the settlement by the Europeans. He was only doing the homework for C14 scientists by compiling information on growth characteristics of the Kauri tree in order to receive a statement on the reliability of the calibration curve. Although the only way that the C14 scientists saw out of this disaster was to assume the presence of missing rings, he presented the experience made by a forestry expert that Kauri trees tend to form double rings [ibid, 192]. Of course, this aggravated the situation even more.

In 1970, in the hot and decisive phase of laying the foundation for the C14 method, something scandalous happened at the conference in Uppsala: the scientific community throughout the world had waited ten years long to see how the C14 community would deal with the fact that the Fundamental Assumption had broken down, which had become

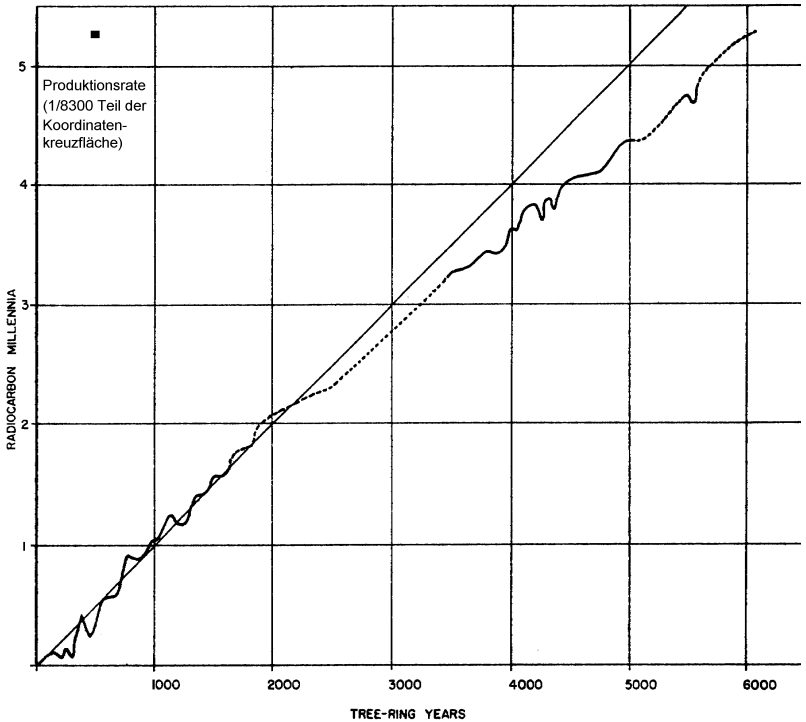


Figure 11: The calibration curve (above) from 1970 [Berger 1970, 96] shows very distinctly the belief in the stationarity of dynamic C14 concentration. For the first 2000 years the bisector of the angle was covered perfectly in spite of all fluctuations. Because it is parallel to the bisector of the angle, even the neighboured dotted area in the first pre-Christian millennium suggests 'regular' conditions. Realize that the curve starts explicitly in the origin of the cartesian cross expressing the idea that actual conditions are constant through the relevant history of earts' environment.

It should be realized that those "wiggles" (C14 patterns as overlay to the linear curve) indicate dramatic rise and fall of the C14 production rate. Who would trust that the overall trend shows nevertheless during some thousand years always the same rating?

The black box inside the graph (at the top, left) versus the area given by the graph itself demonstrates the ratio of "stationary" production (7.5 kg/y = 62.000 kg/8.300 y) to the actual C14-reservoir (ca. 62.000 kg).

obvious in 1960. There were two possibilities: admit that the hypotheses for securing the method which formed the basis of the theory were wrong, or sketch out an alternative path upon which the vehicle of "absolute dating" could be brought to its goal in spite of everything. The title of this symposium clearly expressed the conflict in that situation: "Radiocarbon Variations and Absolute Chronology". A decision was made in favour of the second alternative without any reason - in spite of this flagrant contradiction which Jansen discussed and proved - with the hypothesis of spatial invariance of the C14/C12 ratio which was presented as if it were irrefutable. If they had seriously entered upon a discussion on the subject of "Kauri tree versus bristlecone pine", that would have meant the end of the C14 method.

7. Can There be Any Patterns of C14 Fluctuations Such as This?

We have shown how at the cradle of the C14 method, people closed their eyes to the all-decisive problem. Libby measured a range of 1.000 (written: one thousand!) C14 years in his contemporary wood samples and assured his adherents that there was only an error of ± 50 years. We have already pointed out the fact that synchronising woods which are of different regional origin by means of C14 - especially if the Atlantic is between them - has to rely upon the validity of the Simultaneity Principle. We would like to show now that the C14 pattern itself, which is the important one when annual ring sequences are being synchronised in the generally recognised fashion, contain the decisive references for the fact that the Simultaneity Principle is invalid. These patterns which are considered to be "state of the art" [Baille 1995] of dendrochronology point out the insoluble contradictions in the C14 method and therefore also in dendrochronology.

The calibration curves show that the C14/C12 ratio had already fluctuated in the past. This fluctuation could be a result of a temporary increase in C14 production or - the equivalent to this - C12 atoms disappearing. That would mean that we have a section of the calibration curve before us which would run more steeply than the bisector of the angle (see curve shape A in figure 4). Or, on the other hand, the fluctuation is a result of a temporary decrease in the production of C14 or - the equivalent to this - an increase in C12 atoms. That would mean that we have a

section of the calibration curve before us which would run more flatly than the bisector of the angle (see curve shape B in figure 4). It should be noted that this curve shape is less important for the characteristic C14 pattern. There is, of course, a third way how the calibration curve runs: Here the sign of the curve's rise changes compared to the general pattern (see curve shape C in figure 4). The reason for this can only be found in a significant decrease of C14 atoms - going beyond the range given by the radioactive decay - since even a total stop in C14 production can only force the calibration curve to become horizontal. However, the stop of production can not change the sign of the rise of the calibration curve.

It seems to be just as questionable if additional "fossil" carbon compounds (i.e. without C14) appeared in the atmosphere as if the proportional amounts of C14 disappeared. The only things which at first glance seem to be at all explicable by "normal" means are the curve shapes A and B - that is, with a change in C14's rate of production.

However, although the changes in activities remain in the range of a couple of percentage points within a period of time of some decades generally, the equivalent production rate increases becoming a multiple of the "normal". We would have to observe an increase of six-fold [from data in Vogel 1969, 1144] or even fourteen-fold [from data in Mook 1972, F27] of the "standard production rate" in order to be able to explain the C14 fluctuations. It is, of course, not possible for a reversal corresponding to curve shape C of this tendency, which is almost jointly correlated to follow out of a correspondingly high, but negative production rate since C14 disappearing alone is out of the question.

In fact, the C14 fluctuation patterns require production rates for C14 (in the final analysis for C12 as well to explain curve shape C) which are multiples of the value which is looked upon as normal. Are publishing scholars aware of this circumstance? Can they explain how these "fluctuations" in the production rate come about, considering that the general tendency of published calibration curves lead us to the assumption that there was a constant production rate which was perhaps a little less 10.000 years ago than it is today? Can they explain the ranges in their calibration curves which correspond to curve shape C and which in the final analysis could only have been caused by gigantic injections of

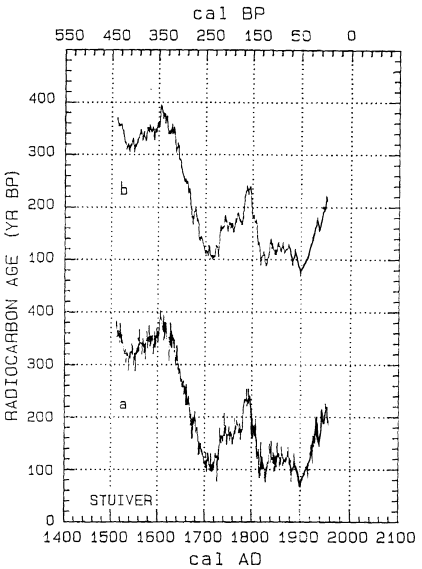


Figure 12: C14 age vs. cal age for single-year samples (a) and 3-yr moving averages (b) [Stuiver 1993, 69]

With a standard deviation of 12.8 C14 years the figure is a trustworthy example of both normal and retrograde pattern within a calibration curve.

fossil carbon into the atmosphere? All of these questions have to be answered with "no".

The fact that there has been a consistent lack of analysis of C14 patterns with regard to C14 production - in spite of its fundamental significance for dendrochronology as well - is characteristic. The only thing we see are the general suppositions on the causes in the direction of changes in the earth's magnetic field, sun spot activities and cosmic radiation. However, the only thing which happens is that the change in activity is observed and stated in quantitative terms. This is in the range of some percentages and is therefore rather tame. As a result, the inversion of the production rate for C14 (which is in fact impossible) remains undiscovered. However, the calculation of the production rate leads - in the framework of an uniformitarian's point of view - to incomprehensibly large values. How could we explain the C14 pattern?

a) *Impermissible curve features - errors in measurement or a violation of the Simultaneity Principle?:* if C14 activity is regionally scattered

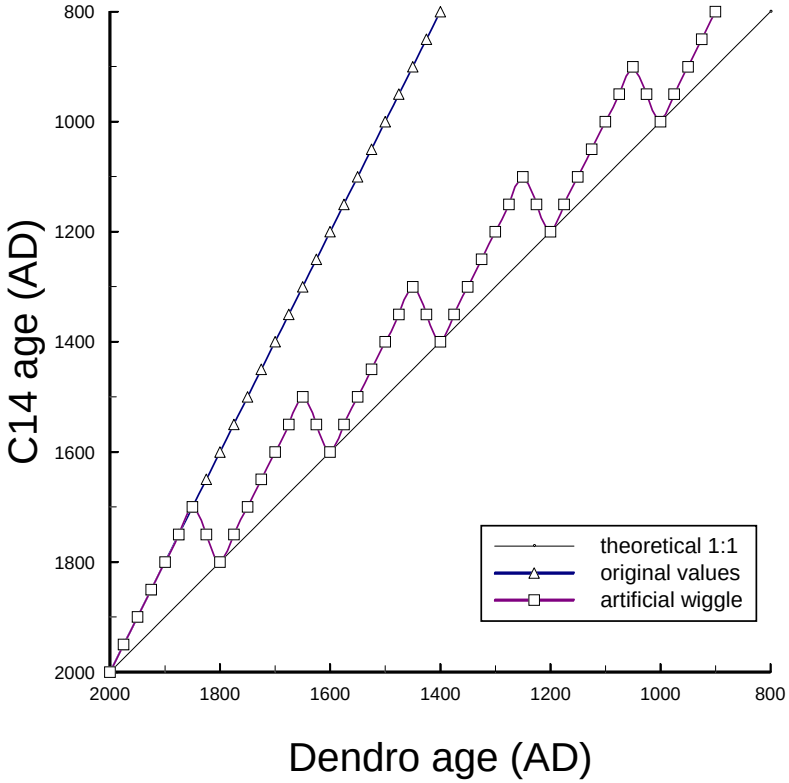


Figure 13: How C14 Patterns Come About by Artificial Rejuvenation

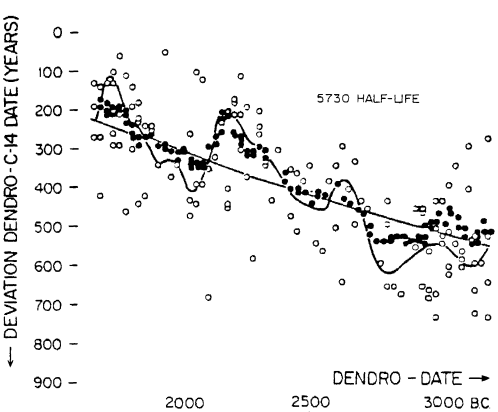
If the production rate for C14 is systematically higher than the present disintegration rate of 6 to 9 kg per year (unsaturated state), the actual calibration curve would be above the bisector of the angle. In the picture we can see schematically illustrated how the C14 patterns come about when the sequence is torn apart and forced into the partial sequences on the bisector of the angle (saturated state). The partial pieces corresponding to curve shape **C** were produced artificially to come back to a closed curve.

in such a fashion that an ensemble of annual rings from various trees would produce a wide range of C14 instead of a linear C14 pattern, then a calibration curve with the familiar design would be an illegitimate approximation curve within this range. As far as the result is concerned, that is exactly what the official critics of the use of C14 patterns for purposes of synchronisation have always asserted [Damon 1978, Clark, 1975-1980]. They traced scattering in replicated measurement to errors in measurements (see figure 14). They would doubtless have received more attention if they had shown the reasons for curve shape C, which are in and of themselves impossible without injecting large amounts of fossile carbon (free of C14) into the atmosphere (see figure 12 for a trustworthy pattern of the retrograde run in question). We do not wish to decide the question of "errors in measurement *or* spatial variance" (not dealing with quantitative injection of fossile carbon) at this point. As far as the effect is concerned, both meant in the final analysis that C14 patterns are artefacts of a mathematical way of treating the body of readings and are in fact not real effects. In this case, dendrochronologists are back to where they were after the Fundamental Assumption broke down: even if there is a multiplicity of C14 values for the sequence in question, it is not possible to get sufficiently exact predating from C14.

b) *Distorting the calibration curve which in fact runs much more steeply in relation to the bisector of the angle by breaking it up and by additionally adding "invented" ranges in accordance with curve shape C:* In the analysis of C14 patterns, we already noticed that these patterns consist primarily of curve shapes A and C. However, there is only a tenable explanation for curve shape A: the amount of C14 produced exceeds the amount that disintegrates. Curve shape C requires the "destruction" of C14 or - its equivalent as well as the only thing conceivable - that the atmosphere is "vaccinated" with pure C12 (what is called fossil carbon). We believe that a quasi-periodical change in increased C14 production (A) and the vaccination with fossil carbon (C) is absurd as long as this leads to a copy of the bisector of the cartesian cross (see chapter 9 which was added for this off-print). On the other hand, it seems completely possible to us in the final analysis that these abnormal ranges (C) can be - among other things - artificially created by placing the corresponding ring

sequences in order to be able to serve natural history's lasting paradigm of uniformitarianism (see figure 13). Since the measured values are generally scattered over a very large range - see explanation attempt a) - it is relatively easy to fudge these ranges in.

Explanation attempt a) comprises the non-applicability of the Simultaneity Principle. Either the measurements of various laboratories (or even in the same laboratory on different days) can not be correlated, or there are *local* C14 fluctuations. If curve shapes "C" were produced generally by upwelling ocean layers containing fossil CO_2 these must come out to be genuine local for the inhomogenous character of the oceanic streams ("conveyor belts") and would disprove the Simultaneity Principle automatically. In any case, this would threaten the credibility of all tree-ring chronologies. Explanation attempt b) also fundamentally questions the calibration curves which are in use. This approach is primarily directed against the central but never consciously stated prerequisite of the C14 method that the same conditions are supposed to have applied for the past 50.000 years as for the last 50 years.



Path of 6th order polynomial (smooth curve), 9-cell averages (●), raw data with outliers rejected (○), and sinusoidal local fits [Ralph/Klein

Figure 14: Problems with interpreting near-random behaviour of the activity measurements. This is what Damon et al. [1978, 488] forced to the statement, that the "present state of the art barely allows us to measure the most recent and obvious ^{14}C fluctuations induced by solar activities". Nevertheless European Dendrochronolgy submitted to the allegedly decade-precise method of C14 pattern comparison (= "wiggle matching") for predating.

It assumes on the other hand that either the global C14 clock was set back by a corresponding supplement in the C12 reservoir in the recent past or that there has been a lasting increase in the production rate for C14. In any event, the correct calibration curve (or the calibration range if there is a corresponding lack of reliability) would be steeper than the bisector of the angle. This would mean that practically all of the previously measured samples - assuming that the measurements were trustworthy - are significantly more recent than previously assumed.

C14 science should formulate its protest against these attempts at explanations carefully. In the final analysis, it not only has an undigested burden from the past to swallow (Libby's fudging extravaganza). It also must admit to itself that there has never even been an attempt to explain the causes for the C14 pattern, nor the "deviators" such as the Kauri tree mentioned above. The protest from the dendrochronologists' camp should be directed exactly at their "brother-in-arms" C14 which has repeatedly avoided clarifying methodological problems. The C14 community has remained silent on these problems because they would have denied themselves the only salvation still open to them: support from dendrochronologists.

We are aware of the fact that these are processes which are hardly the result of conscious deception or consciously misleading anyone. It becomes that much more obvious how strong the power of self-evident truths is which are founded in all levels of consciousness. One of them has been mentioned here: the unquestioned belief that there were not any different conditions which could have influenced processes of development than those which are predominant today (= "uniformitarianism"). We can see how effective this belief is when we observe how Libby swept away explicit evidence for disparate C14 findings. He did so in order to allow this belief to become effective in the first place by opening the way to transferring - allegedly regular - conditions to the past by his fudging.

We recognise this opinion about how natural processes work even in the first calibration curves. The bisector of the angle was a symbol for the possibility of continuing today's conditions - in spite of any fluctuations. We also interpret the omissions in the analysis of the C14 patterns as a

result of attitudes which have remained unconscious on the limited possibility of processes in nature. H.E. Suess, the person who defended the use of C14 patterns most eagerly and with the most endurance, was never really interested in how nature actually produces these patterns. The pencil with which he initially drew the curves through the patterns of measured values should have fallen from his hand when he created the first of his impossible curve shapes (C). After all, this section meant that the atmosphere was quickly and lastingly vaccinated with fossil carbon, i.e. C12. Of course, Suess expressly rejected this as a cause originally because it would have required a stronger change in temperature (to release fossil carbon dioxide from the deep water of the oceans) as would have been necessary to end or to start respectively the Ice Age [Suess 1965, 5949]. Instead of this, he still defended Lucrecius' claim in 1990 that nature doesn't make any jumps and insisted on uniformitarianism [Suess/Linick 1990, 406]. In the final analysis this was based on his feeling and not on any effective arguments.

8. Summary and the View to Warwen Chronology

It was our goal to show that dendrochronology, in drawing up European oak chronologies, has relied upon a method which is untenable. (The same statement applies to American dendrochronology.) It would be a nice coincidence if European dendrochronologies were correct. However, we do not believe that one can arrive at the right goal when inadequate methods are used on a permanent basis. Two tasks can be seen at the end of this article. The findings and conclusions which have been compiled and presented here shall be presented in a more detailed and differentiated fashion in the form of a book. They shall therefore be revised. We would like to wait for the reactions to our speech in Hamburg on the occasion of the annual meeting as well as this article and, if necessary, fit them in.

At the same time, it is obvious that our criticism of dendrochronology as well as of the C14 method reaches through to the absolute chronology of the Post Glacial. The minimum duration for the tree-ring chronologies to be constructed was estimated at approximately 10.000 years because of the date (which is part of common property in science, although it is amazing how little substantiated it is). One of the persons who substantiated this number was G.J. de Geer, who drew up a Warwen chronology

at the beginning of this century going back 7.000 years which was worked out with similar methods and is supposed to have come about exclusively Postglacial. We consider the comment, which we have encountered on a number of occasions, namely that this chronology conforms at least roughly with modern calibrated C14 data (although we haven't yet come across the corresponding publication), as one touch-stone for our criticism. It has only been presented here in its rudiments.

9. Remark About the Possibility of Wiggles

It is our experience that difficulties occur when analyzing the interdependence of the production rate of C14 and the actual C14/C12 relation as starting activity for an organism which stopped metabolism. It seems to be not easy to accept that the production rate for C14 becomes *negative* on principle when the *calibration curve* changes the sign (described as curve-form C). *Lowering* of the rate is not enough. Of course there can be no negative production of C14, so flow-in of C12 instead must occur. The only effect we learned of so far and which can produce the wiggled form of the curve is the upwelling of ocean-layers with fossile carbondioxid (which diffuses into the atmosphere to lower the actual concentration of C14 faster than by radioactive decay alone). Discussion of this effect continues. If this comes out as the main cause for the "wiggles" the Simultaneity Principle must break down for the globally inhomogenous character of the so-called oceanic "conveyor belts".

Once again we want to emphasize that the real production rate must be an order of numbers larger than the always cited imaginary and never measured value of ca. 7.5 kg per year and that the ocean-layers do *not* upwell just in a way that it might compensate this production rate in bringing the calibration line into the neighbourhood of the bisector of the cartesian coordinates referring to calendar and conventional radiocarbon age respectively. The well-known calibration curve is in the end the product of fitting the dendro-sequences to that bisector instead of fitting them exclusively mutually in a methodologically satisfying procedure.

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