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Thoughts on radiocarbon dating

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This discussion began with some thoughts on radiocarbon dating sent to us by Euan W. MacKie of the University of Glasgow and arising out of the symposium on the Impact of the Natural Sciences on Archaeology held jointly by the Royal Society and the British Academy in December 1969. At the same time we had received comments from John Collis, Lecturer in Archaeology in the University of Exeter, and Professor D. W. Ewer of the Department of Zoology in the University of Ghana. We print these with comments by Dr A. G. Smith, of Queen's University, Belfast, Dr Hans Suess, and Dr A. C. Renfrew.

Mr MacKie writes:

There are several major aspects to the use of radiocarbon dates by archaeologists and these include (1) the actual causes of what one supposes was a large increase in the amount of C14 in the atmosphere which started some time before 5000 BC in real years and had largely disappeared by about 800 BC: (2) the effect on prehistory of the greater real ages of C14 dates between about 4200 BC (and probably for some time earlier) and about 800 BC: (3) the problem of what should be done about correcting such C14 dates: and (4) the accurate relating of individual dates to their archaeological context. I shall comment briefly only on the third point; the first one, and several others, have been treated in some detail by Neustupný (1970) and the second several times by Renfrew (1968, 1970) and also by Suess and Strahm (1970). The fourth is a technical problem which it is inappropriate to discuss here.

THE CONVERSION OF C14 DATES

Perhaps the most urgent question which now arises over C14 dates is what to do with them now that we have comprehensive information from tree-ring analysis about the various discrepancies between them and real years from about 5000 BC onwards. Should one actually convert all the relevant existing and forthcoming C14 dates into real years with the aid of the published graphs, or just keep adding to them laborious phrases like 'which is equivalent to about 4000 BC in real years', or just use them as they are? I distinguish here between *conversion*—that is providing only a 'corrected' date in real years—and *translation*, which to me is giving the equivalent real year date as well as the radiocarbon age. In my view it would be very unwise to use only converted dates in publications. Apart from the possibility of the author having made a mistake in the conversion, or having used out-of-date information for it, it is an important principle that the reader should not be divorced from the primary dating evidence—which is what a radiocarbon date, with all its faults, is—and so forced to rely totally on the author's estimate of what it means. The original C14 date, *with its laboratory number*, must always be quoted somewhere with any translated date. There are already enough difficulties in relating the C14 age of a sample—that is of its biological death—to its prehistoric use and ultimate archaeological context (part of the fourth problem mentioned earlier) without dragging in a new one.

Most people will probably agree that the C14 date as the primary evidence must always be included in a paper or report but many might argue that all our elaborate chronological schemes for the later Mesolithic, the Neolithic

and the Early and Middle Bronze periods should be translated into real years with the help of the tree-ring calibration curve. This is a more difficult question, but my view at present is that it should not be done. Not the least of the reasons for this is that archaeologists have, after twenty years, become accustomed to the radiocarbon time scale for prehistoric Europe which was drastically different from the previous one and required some getting used to. To wrench the cultural and environmental record yet again off a familiar chronological framework and nail it on to a new and unfamiliar one invites confusion and should not be done unless it is shown to be clearly necessary. It seems to me that it is not necessary at the moment though it may perhaps become so. The main reason for thinking this is that the tree-ring calibration programme is incomplete, as are many of the interesting relevant research projects that have sprung from it to clear up specific problems. Until we have a final and unequivocal explanation of exactly what was going on in the atmosphere and the biosphere between say 6000 and 1000 BC, a complete and comprehensive alteration of the whole prehistoric chronological framework seems premature. Again the C14 dates can at present be corrected only from about 4200 BC onwards. While those back to 6000 or even 7000 BC may one day be corrected as the tree-ring chronology is extended backwards, and the varve chronology brought in (Stuiver, 1970), radiocarbon dates go back to more than 50,000 years ago. Thus for the first four-fifths of the period it covers the C14 chronology will remain the only one for the foreseeable future, whatever its accuracy.

In any case does it in fact matter for most prehistoric archaeological purposes exactly when certain sites were occupied or exactly how old a given object is? Is it important for example that the British Neolithic period began at about 4200 BC in real years and not 3500 BC in radiocarbon years? Obviously it is in the context of a few problems—such as how many generations might have used collective tombs or when non-literate, radiocarbon-dated cultures are being compared with those having independent historical chronologies (Renfrew, 1970). But for most research the greatest need is for a thoroughly reliable *relative* chronology, accurate to within a few centuries in terms of real years of course but particularly precise in *synchronizing* a great diversity of natural and human events so that as large a part as possible of the total environment of each period can be inferred and the relative ages of physically unconnected sites of all kinds worked out. This is exactly what radiocarbon does and this, not its value for absolute dating, is to my mind its main enormous virtue.

It may be necessary to state again that I am not arguing against *using* C14 dates translated into real years, only against a wholesale conversion at present of the chronological framework of Neolithic and Bronze Age Europe from radiocarbon into real years. Only in those few short periods when, because of rapid short-term fluctuations in the C14 reservoir, one radiocarbon date can equal several real year dates, need the tree-ring calibration constantly be borne in mind but again this is because it shows that the C14 method is intrinsically inaccurate as a relative dating method for these short periods and is therefore less reliable for synchronisms of disparate phenomena.

DISTINGUISHING TYPES OF DATES

Whether or not this view is generally accepted there remains a clear need for a simple way of distinguishing different kinds of dates. Comparisons between C14 dates and their equivalents in real years will always need to be made whichever system the main dating framework follows. What then is to be done if one is not tediously to explain what one is doing in the way of translation in every sentence? The answer is surely to invent a simple notation which will distinguish at a glance between different kinds of dates. This would allow one to indicate equivalences between one chronological scheme and another without ambiguity while simultaneously preserving and *identifying* the sources from which the dates come. This last is a particularly important point as the dating systems multiply. If every date quoted is henceforth distinguished by a prefixed letter

C.2050 BC. *Carbon 14 years:* this would be a C14 date as published in *Radiocarbon,* that is in terms of the Libby half-life of 5570 years.

C^2.2170 BC. *Carbon 14 years:* the same radiocarbon date when the total age is multiplied by 1·03 and is thereby expressed in terms of the best half-life of 5730 years.

T.2600 BC. about the same date in *Terrestrial years,* that is revolutions of the Earth round the Sun as recorded by contemporary markers of seasons like tree-rings, varves or human chronologies with a known base-line.

A.2600 BC. *Astronomical years:* ought to be the same date as T.2600 BC, but has been calculated from astronomical data by retrojecting the modern motions of the solar system into the past.

E.2600 BC. *Egyptian years:* about the same date in the unique 'floating' Egyptian historical chronology which is pinned down by astronomical computation (see below).

K.80,000 BP. *Potassium/Argon years:* ages calculated on the basis of the rate of decay of potassium to argon.

according to something similiar to the system above most of the difficulties should disappear.

The benefits of this system are that it would distinguish between different types of dates with the minimum of verbiage and maximum of clarity. It would also oblige all users of dates to work out for themselves the nature and origin of each one, which would be an advance. The only slight difficulty perhaps is that it is a little cumbersome to describe a century with these letter-codes. Perhaps the best way would be to say 'the nineteenth carbon century BC' and write it as 'the C.19th century BC'. The same century in real years could be written 'the T.19th century BC' and described simply as 'the nineteenth century BC', it being understood that the absence of a qualification means real (T) years. The practice of putting a C in front of the number to mean 'century' would have to cease, at least for the BC era.

A special problem is presented by Egyptian historical chronology from about 1000 BC backwards and with the traditional historical dating systems which depend on it—those of the Minoan, Helladic and Hittite cultures and so on as well as the more remote but somehow linked European Bronze Age societies. The relative lengths of the various Egyptian dynasties are known by and large from native records but the absolute age of the whole 'floating chronology' is tied down by about three astronomical computations, which interpret the modern meaning of ancient observations of the star Sirius. Thus Egyptian chronology for the two millennia prior to about 1000 BC is technically in A. years but is given its internal precision and detail by historical records. This system is unique and of great importance because of its antiquity and because of the many other cultures which have depended on it for their own dating, and it therefore deserves a notation of its own. Rather than use the awkward AH prefix which I first thought of (for Astronomical/Historical years) it would be better to express all such dates in E (for Egyptian) years, even those as far afield as the *traditional* dates for the Wessex Bronze Age of Britain (as opposed to those suggested by Renfrew (1970)). One would need to be told by the experts concerned how much for example of the early Assyrian and Babylonian chronologies is self supporting and how much depends on E.year dates.

In what epochs should this system of letter-dates be used? It will always be important to distinguish radiocarbon dates of whatever period, and especially so to show clearly with the C. and C^2. notation which half life is being used. As a general rule T.years will probably not need to be distinguished as such after about 500 BC when the divergence between them and C.years is negligible. A post-500 BC date without a prefix can be considered to be in Terrestrial years. Before this time however the distinction becomes progressively more important. Not only do the reliable human chronologies with a known base-line soon stop but the divergence between C. and T.years quickly becomes significant. Hence the letters

will always be useful before 500 BC. An Astronomical year date should always be distinguished as such.

NEUSTUPNÝ, E. 1970. A new epoch in radiocarbon dating, *Antiquity*, XLIV, 1970, 38–45.

RENFREW, C., 1968. Wessex without Mycenae, *Annual of the British School of Archaeology at Athens*, LXIII, 277–85.

1970. The tree-ring calibration of radiocarbon: an archaeological evaluation, *Proc. Prehist. Soc.*, XXXVI, 1970, 280–311.

STUIVER, M. 1970. Tree rings, varve and carbon-14 chronologies, *Nature*, CCXXVIII (Oct. 31st), 454–5.

SUESS, H. and C. STRAHM. 1970. The Neolithic of Auvernier, Switzerland, *Antiquity*, XLIV, 91–9.

Mr Collis writes:

It is now some 20 years since the advent of C14 dating, and yet there is still a state of anarchy surrounding publication and use of dates. The dates themselves are quoted on this or that half-life, without a statement of which is being used and now in more generalized uses of the dates, we have the added confusion of recalibration. Partly the problem is due to the failure of prehistorians to understand the basic principles and statistical nature of C14 dating, and partly due to their blatantly ignoring the advice given by the physicists on standard publication procedure. May one make the plea yet again that every article and book using C14 dating be prefaced with a note stating which half-life has been used and whether recalibration has been employed. In the first case the half-life has not yet been definitely fixed, and in the second there are certain problems surrounding recalibration which have yet to be resolved.

In European prehistory the advent of C14 dating was initially felt most in the re-dating of the Neolithic cultures and the reconsideration of their relationship to the historically dated sequences in the Near East. The new pattern that has emerged differs so radically from the old chronology and is so internally regular, that adjustments in the half-life and recalibration alter our view but little, especially as the earlier phases of the Near Eastern chronology are themselves based on C14. The impact of the new half-life, and especially recalibration, has, however, fallen most heavily on the question of where the early development of copper and bronze metallurgy took place.

In a series of stimulating articles, Dr Renfrew (1969) has suggested that, contrary to received belief, the origin of metallurgy lay not in the civilizations of the Near East but in the tell cultures of the Balkans, while smelting and bronze first appear in the Baden horizon. The classic Early Bronze Age culture, the Unetice, and the other related groups such as the Wessex burials would now antedate the culture of the Mycenean shaft graves, which had previously been assumed to be the parent culture.

The re-interpretation is based on three assumptions:

1. That we are already in a position to compare C14 and historical datings.
2. That C14 chronologies in different areas can be directly compared.
3. That recalibration using the curves for the fluctuation of solar radiation obtained from the sequoia and bristlecone pines in the South-western United States, can be directly applied to European material.

Recalibration is the outcome of questioning one of the basic tenets of C14 dating, that during the past 50,000 years the amount of solar radiation, and with it the ratio of C14 to C12 in the atmosphere, has remained constant, at least up to the industrial revolution. Now that fluctuations have been observed, it is assumed that they are world-wide, following another basic tenet, that dispersal of newly formed C14 in the atmosphere is so rapid that geographical variations do not exist, and thus that all C14 dates are directly comparable, and on the same scale.

The two major series of readings from tree rings showing the fluctuations are derived from the sequoia, and especially the bristlecone pine. Both are from California, and so if geographical factors do in fact exist, they would not be expected to appear in these curves. For the rest of the world there are only two series, that by

Janssen for the Kauri Pine in New Zealand, and two floating series from the Neolithic site of Auvernier in Switzerland. The Kauri Pine results have been compared with those of sequoia and bristlecone pine in graph form in an article by Shawcross (1969) on New Zealand chronology. This curve covers only the last few hundred years, and contrasts strongly with the other two. But New Zealand dates recalibrated against the kauri pine curves seem to make more archaeological sense than calibration with the bristlecone pine. Thus a local factor seems to be at play.

The Auvernier sequences have recently been published by Suess (1970, 91–9), and the curves were correlated with the bristlecone pine by means of a 'nearest fit' comparison. If we assume that the same factors cause the fluctuations in both curves, and that direct correlation is valid, then we find that almost all of the twelve readings are on the low side, six being outside 1σ range of the curve and one outside 2σ. Had these been individual dates, and not linked in a sequence, then they would have been considered several hundred years older. Thus again there seems to be a local factor, either that the bristlecone pine curve is not relevant, or that something causes unusually high readings in California and low in Switzerland.

What this factor may be is not yet clear. The peculiarity of the New Zealand sequence was explained as due to volcanic activity in the area (a factor in the Aegean as well) while in his discussion on the Auvernier sequence Suess suggested differences in laboratory preparation, or perhaps direct solar radiation on the bristlecone pine, thus producing high C14 counts, and even that the 'dead' tree rings were absorbing C14 after death. In this last case, perhaps the calculation of the half-life of C14 can be affected.

What we most desperately need now is a series of long sequences which can be compared with the Californian sequences, but it is clear that, at our present state of knowledge, none of the three basic assumptions for comparison of C14 dates especially with calendar years is yet acceptable, and we need to know more about regional effects before definite conclusions can be drawn.

RENFREW, C. 1969. The autonomy of the South east European Copper Age, *Proc. Prehist. Soc.*, XXXV, 12–47 (with further references).

SHAWCROSS, W. 1969. Archaeology with a short, isolated time-scale: New Zealand, *World Archaeology*, I, no. 2, 184–99.

SUESS, H. and C. STRAHM. 1970. The Neolithic of Auvernier, Switzerland, *Antiquity*, XLIV, 91–9.

Professor Ewer writes:

In reading recently Dr Evžen Neustupný's article on radiocarbon dating (*Antiquity*, 1970, XLIV, 38–45) I have wondered whether the present habit of citing the results of C14 determination in years alone may not be laying up trouble and irritation for future generations. It seems likely that tinkering with radiocarbon dating will continue for many years to come. Better values for half-life, better corrections for secular changes, possibly corrections for localities will continue to appear and be adopted in a somewhat haphazard fashion by different laboratories.

If we try to foresee the research worker of 2060 reading a paper or report published in 1975 and meeting a date of say 7650±250 BP (followed by some laboratory citation) he will be faced by the tedium of having to find what half-life was used and what corrections were applied, before he can reduce the date to a form in which he can apply the half-life value and corrections believed, in 2060, to be valid.

The dates are based upon physical measurements, which are themselves independent both of half lives and corrections. It might therefore be of value, for the comfort of future generations to devise some unit (possibly a Libby) which directly reflected the original measurements made. At the present time results from radiocarbon laboratories could give both as dates, for immediate consumption, and as Libbys for future convenience. As technical refinements are made, tables could from time to time be published for the conversion of Libbys to dates which would be as valid as contemporary knowledge at that time allowed.

Radiocarbon dating has been current only for 20 years and the problems of changing corrections do not now seem formidable. But, as in other cases, the use of units with little thought of the future can lead to difficulties. In the present case, this seems to be a matter which could be avoided.

Dr A. C. Smith writes in March 1971:

I have read the comments on radiocarbon problems with much interest. My own feeling is that it is as yet too early to begin calibrating radiocarbon dates (not 'recalibrating', surely, *v.* Collis), except as an exercise, and in the most general terms.

First, I believe it is felt by several people that the high altitude at which the bristlecone pines were growing may have laid them open to *in situ* formation of ^{14}C atoms by cosmic ray neutrons. Suess admits of this possibility in *Antiquity*, 174, 95. It would make the C14 dates of bristlecone pine material too young and, before *c.* 2000 BP, emphasize the De Vries effect. Whether this really has taken place is not known. The Egyptian dates described by Edwards at the London conference (*Antiquity*, XLIV, 136 or *Phil. Trans. Roy. Soc. A*, vol. 269, 11–18) were, however, closer to the radiocarbon dates than the Californian tree-ring dates. It may thus be necessary to look at more results from low-altitude trees before the real magnitude of the effect can be assessed. This will take a long time, if, indeed, it is possible. To expect authors to wait until this kind of check has been made is unrealistic.

Secondly, the recently published proceedings of the Uppsala conference on 'Radiocarbon Variations and Absolute Chronology' (Nobel Symposium No. 12) show that there is as yet no general agreement between the radiocarbon laboratories concerned as to any standard form in which the data can be used. Undoubtedly, however, the curve published there by Suess will be used for calibration, in the same way that its predecessor has been (e.g. Renfrew, *Proc. Prehist. Soc.*, XXXVI, 1970, 280). The minor fluctuations of this curve are open to question, however. As Suess says (Nobel Symposium, 12, 310), the curve was drawn by 'cosmic schwung' and there is nothing final about the 'wriggles'.

Since the great body of the calibration data points in the same direction it appears theoretically possible to bring it together and produce an internationally acceptable calibration chart or table. That this was not achieved with the present data at Uppsala is, to say the least, unfortunate. Undoubtedly this will again be a lively topic of discussion at the Radiocarbon Conference to be held in New Zealand in 1972. Should we not forbear from calibration until another attempt has been made, or the full La Jolla data has been made available?

In general I think that the points made by MacKie, Collis and Ewer are valid. I am in strong agreement with the invariable publication of the original dates as listed in *Radiocarbon*, whatever else is done with them. After all, these are, or should be in a standard form using the original 5570 year half-life. If you like they are quoted in 'Libby years'. We already have in essence the standard that Ewer wants. It was mainly to avoid confusion that it was decided to continue using the original half-life at the Pullman Radiocarbon Conference. But it has opened the door for multiple repetitive conversions. Is it too much to hope that authors will check the dates they use against the original publication? This is now quite simple using the comprehensive indices in *Radiocarbon*.

A scheme such as that suggested by MacKie has a good deal of merit. But could it be universally enforced? MacKie's difficulty in the confusion between his 'C' category and 'Century' could very easily be resolved by substituting 'R' for 'C'. Clearly, however, his category 'T' will be worked out on different bases by different authors until some international agreement is reached on calibration. It might only disguise such differences to use a system of categorization at present.

Finally, perhaps I might say that editors, and referees, have an important responsibility in seeing that radiocarbon dates are published in an unambiguous way.

Professor Hans Suess comments briefly on MacKie and Collis:

You wanted some comments on the notes by Collis and MacKie. There is not very much that I can say. Dr MacKie fully understands the situation, but much of what he is saying in his note has been discussed before. His suggestion of a lettering system to distinguish the different kinds of dates is an interesting one that should be discussed at an international meeting. Such a system would certainly be of great value if adopted by a representative international group of scientists.

Dr Collis appears to me to be too much concerned with an exaggerated accuracy of the radiocarbon method. A radiocarbon date may be uncertain by several hundred years and still be very valuable for archaeological research. I would simply say that for the present time the possibilities of such uncertainties should always be kept in mind, and that radiocarbon experts should be consulted whenever maximum accuracy is desired.

Dr Renfrew writes:

We have as yet no detailed and generally agreed chart for the calibration of radiocarbon dates. The pattern of carbon dates from dendrochronologically dated samples, as reported by Suess to the 12th Nobel Symposium (Olsson, 1970) is not identical with the chart compiled from the results of other laboratories analysing material of similarly known date. This gives no cause for alarm: there seems a very wide acceptance among physicists of the general magnitude of the deviations. Only the smaller variations are at issue: the squiggles on the calibration curves. As several authors have observed, continued progress is to be expected, and new factors will continue to come to light, like the remarkable 11-year cycle recently reported by Baxter and Walton (1971).

It is important, then, to separate the general trend—a deviation of up to seven centuries from the uncalibrated chronology, widely accepted as valid—from the more problematical fine structure. Errors and uncertainties of at least a century or two are inevitable in the present state of knowledge, and we should not be too optimistic about the accuracy of our calibrated dates. But John Collis underestimates the great body of scientific work, from the pioneering study of Libby's colleague E. C. Anderson to the findings reported at the Uppsala Symposium, indicating that the variations are indeed worldwide within a couple of per cent. The simultaneity principle holds because the atmospheric mixing is so effective. Unfortunately no work on the altitude effect, mentioned by Dr Smith, has yet been published, but no one suggests that this would account for the entire deviation observed before 800 BC—nor is it easy to see why it should cease to operate after that time.

The detailed problems of the calibration will be with us for a long time yet, but this need not prevent our assessing its broader implications. What we can do at the moment, after quoting our dates systematically in radiocarbon years, is to investigate the considerable impact which the general outline of the calibration has had upon our thinking in prehistoric Europe. I agree with Euan MacKie and Professor Ewer that the calibration is not always relevant, and that we can often operate with the old relative chronology in radiocarbon years. But the difference is crucial in two cases: when we are considering the duration of periods, and above all in analysing relationships and contacts between one region and another, if a historical chronology is directly or indirectly involved. Since the latter has been one of the chief fields of investigation in the past—often to an exaggerated extent—the result is to invalidate or set in question much that has been written about prehistoric Europe. It is now an urgent task to assimilate the broad effects of the several-centuries shift suggested by the calibration, and to reconcile our archaeological data with it. This proves surprisingly easy if one or two assumptions, which once seemed of obvious validity, are called into question. So far, I feel, one of the main benefits brought by the calibration has been to show us that, in the past, much

of European prehistory has been built up on a questionable premise.

For such purposes as this it is necessary to quote dates, converted as accurately and reliably as is possible using the physical evidence currently available, into approximate calendar years. In other instances we can make do with the plain radiocarbon dates, which are, anyway, of necessity our starting point. In any case the conventional radiocarbon dates should always be quoted first, with their laboratory number, on the 5568 half-life. Everyone agrees about that.

BAXTER, M. S. and A. WALTON. 1971. Fluctuations of atmospheric carbon-14 concentrations during the past century, *Proc. Roy. Soc. Lond. A*, 321, 105–27.

OLSSON, I. U. (ed.), 1970. *Radiocarbon Variations and Absolute Chronology: Proceedings of the 12th Nobel Symposium* (London and New York).

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