Dendrochronologia 20/1-2 (2002) 63–68
© Urban & Fischer Verlag
http://www.urbanfischer.de/journals/dendro

DENDROCHRONOLOGIA

Archaeological dendrochronology

Peter Ian Kuniholm

Malcolm and Carolyn Wiener Laboratory for Aegean and Near Eastern Dendrochronology, Cornell University, Ithaca, USA

Summary

Archaeological dendrochronology is generally alive and well, and a number of new and creative applications are to be seen, especially when the ecological and climatological implications of the archaeological and tree-ring records are considered, as demonstrated in recent work of the Tucson Laboratory, but we could all profit from more interchange of information among workers. Much of what we do is published in obscure journals with limited circulation outside of the geographical zones in which we work. If we learned anything at Davos, it is that networking is the key to success in the future. We need to exchange our publications, and we need to trade our data sets. We must also take serious steps to ensure the survival of the smaller labs.

Keywords: Dendrochronology, tree rings, archaeology, teleconnections, databases

Introduction

First, starting 90 years ago, there were the pioneers whom everyone cites and on whose shoulders we still stand: Andrew Ellicott Douglass in the New World followed by Bruno Huber in the Old World. But there were other less well-known pioneers who explored widening areas of the world and longer expanses of time. Since some of them are here today and would probably be embarrassed if I were to mention their names, I will refrain from specifics other than to say that each new venture into an unknown area is really a form of pioneering. Even the pioneers who failed in their efforts were just as important as the ones who were fortunate enough to succeed. I think of the master's student who worked for over a year in meso-America and had to conclude with reluctance that her work had been in vain. She is still part of the bigger picture in which we find ourselves today.

Secondly, there were the developers/solidifiers/reinforcers who built upon and exploited the work of the pioneers before them. Some of them are also here today, and I will not embarrass them either ex-

Address for correspondence:
Peter I. Kuniholm, M. and C. Wiener Laboratory for Aegean and Near Eastern Dendrochronology
B-48 Goldwin Smith Hall
Cornell University, Ithaca, N.Y. 14853-3201, USA
Tel. +1 60 72 55-97 32; Fax +1 60 72 55-83 36
peter@dendro.mail.cornell.edu

1125-7865/02/20/1-2-63 $ 15.00/0

cept for one: Fritz Hans Schweingruber, who has already been abused enough both yesterday and today so that one more mention should not bother him. To be sure, Fritz could be classified as a pioneer in many respects as well, but I like to think of his work as a mentor, encourager, facilitator, and exemplar as he has pushed a second and third generation of dendrochronologists into new accomplishments. As we meet in Davos to celebrate Fritz's career and to think about where we have been and where we are as of 2001 and where we might be going, we should be thankful that Fritz seized the opportunity to build upon the foundations so well laid by Douglass, Huber, and others before him. Fritz has also acted as the *pater familiae* of what he refers to as the dendrochronological "family." Not just "group," not simply "co-workers," not just "collaborators," but *family* with all the responsibilities and obligations that the word implies. He has set an example for all of us.

Some basic items

Since I recently published a so-called "state of play" article (Kuniholm, 2001), copies of which were handed out at Davos and duplicates of which are available on my web-site, I am not going to repeat the gory details here. Several points, however, are worth reiterating, not necessarily for this audience but for anybody who reads the proceedings of this meeting.

1. The dendrochronological method is still simple, at least in principle. Accurate ring-identification and matching can still be done by eye, and a one-year error is just as egregious now as it was in Douglass's time. The fact that Jeff Dean and his co-workers at Arizona still are able to use Douglass's techniques of skeleton-plotting and visual matching speaks for the elegance and simplicity of the original method.

2. Although computerization and other technological developments have speeded up the process dramatically, they have not necessarily made it any better. "Dendro-by-the-numbers" has always been a mistake, and would-be dendrochronologists who think that they merely have to plug numbers into a black box and sit back and wait for a correct date will inevitably be disappointed. Reality, as we so often forget, is the wood itself.

3. Dendrochronology is still the only archaeometric technique which is capable of annual or even sub-annual resolution. Radiocarbon is the next best game in town, and indeed it is the only solution when dendrochronologically datable wood is not preserved, but it still does not give us the deadly resolution we have all come to expect. Let me quote from Robert Hedges at Oxford University's Research Laboratory for Archaeology and the History of Art who wrote recently in his introduction (page 4) to the chronology section of Brothwell and Pollard: "... almost any technique that produces a date at one specific point may find itself playing a crucial role on another part of the planet. The potential application of Greenland ice core stratigraphy in the dating of the eruption of Thera in the sixteenth [sic] century BC illustrates well this kind of long distance linkage. Powerful examples of this approach result from the accumulated infilling of dendrochronological opportunities, in which archaeologically significant events can have a chance of being registered to within a frighteningly close precision. This depends entirely upon preservation conditions, but it is good to see that more than wetlands or deserts can be exploited; that charcoal can sometimes be dated; and that the whole enterprise becomes more powerful as it grows. The precision of dendrochronology brings quite new, and so far scarcely exploited, potential for archaeological inferences, but in any case the linkage between culturally defined stratigraphy and dendrochronological dating, at least in the later prehistoric periods, does engender a particularly close relationship between archaeology and the dating specialist. I for one am quite converted – from an initial position where the main value of dendrochronology seemed to be in providing a calibration for radiocarbon dating."

We should all sign thank-you postcards and send them to Dr. Hedges.

The ecumenical range of the archaeological posters presented in Davos is encouraging. Wiggle-matching has become routine as we see at Ljubljana, Venice, and Lavagnone. Evidence for timber trade and patterns of import/export continue to grow, and I

have a few Mediterranean and Aegean analogues to add to Dr. Wazny's impressive list. Esther Jansma and her colleagues are evangelizing the Netherlands. An oak chronology from ca. 3500 B.C. to A.D. 560 is a great achievement. Particularly interesting is the work of the Hemmenhofen group, especially the evidence for woodland management in prehistory. The scales are often astonishing: the Moudon group has analyzed over 17,000 samples, a figure that begins to approach the production-line output of Jeff Dean et al., in Arizona (and I mean that as a compliment).

Other work, not shown here in the Davos posters, is equally intriguing. Jeff Dean just sent me his and Gumerman's article on "Artificial Anasazi" published last year in *Scientific American* in which they compare simulated data with historical data from the 10th century through the 14th century. These are the kinds of imaginative exercises, using digital people and so-called "agent-based modeling," which would have been completely impossible without tens of thousands of dendrochronological dates derived from the time of Douglass onward. In other words Jeff and his colleagues can now ask questions that would have been quite unthinkable a couple of generations ago, much less answerable. Recent papers of this type by the Arizona group which I had overlooked for the 2001 survey, and which I suspect many other colleagues might have missed as well, are Abnabeh et al. 2000, Dean 1995, Dean, Funkhouser 1995, Dean et al. 1999, Dean et al. 1996, and Gumerman, Dean 2000.

4. What may seem perfectly obvious to us here today – for example, a single 8000-year-plus European oak chronology from the Baltic to the Pyrenees, inclusive of both the continent and the islands – was in no way obvious when the work began a couple of generations ago. The researchers who fought their way through the problems and past the resistance of those who believed the work was impossible and who identified mistakes, either their own or those of others made years before, deserve to be complimented on their persistence.

Part of the problem were faulty assumptions: John Fletcher, when I was just starting out, explained to me all about his Type II Chronology from a hypothetical oak forest which he said had grown on either side of the English Channel. The work of Tomas Wazny and others on Polish oak has now shown this assumption to be completely wrong. In 1976 I was assured by Axel Delorme that the four German oak chronologies: north east, north central, north west, and south would never be combined. We now know better. It was not Delorme's fault that some of the first German wood ever collected was also the noisiest.

In what was perhaps a case of overenthusiasm rather than faulty assumptions were the claims of teleconnections made first by Baron de Geer and then by D. Justin Schove. Fear of being equally condemned kept researchers from looking outside their little regional shells. Some teleconnections we now know are legitimate. Others are not. But as recently as 1982 at the Archaeometry Symposium in Bradford, Mike Baillie, not well-known for his caution, asked cautiously: "Is There a Single British Isles Oak Tree-Ring Signal?"

Maybe that is one of the problems with dendrochronology: much of what we know was neither obvious nor intuitive before our going out and trying it. John Fletcher said flat out that the chronologies of England, Scotland, and Ireland would never be merged. Who would have thought that Dieter Eckstein could have had the success he has had with teak in southeast Asia? If Dieter had been sensible, he would have stayed home and not bothered. As Malcolm Hughes likes to say, if Andrew Douglass had known more about biology and botany, he never would have gotten started in dendrochronology. If you had asked me in 1973 whether I would ever get crossdating across the Aegean, I would have told you not to be silly. I was so sure it was impossible that, when I found an excellent fit between the Gordion junipers on the Anatolian highlands and some juniper bedlegs from the Athenian Agora, I wasted a considerable amount of time trying to figure out how the Phrygians could possibly have exported wood to Athens, perhaps in the same way that Herodotus tells us King Midas sent his wooden throne to Delphi, a thing well-worth looking at. Then I found that Turkish and Greek forests crossdated, and I threw out all my old speculations. When the University of

East Anglia climatologists at Norwich were first approached about joining IPID, the now-defunct International Project in Dendroclimatology, they were quite skeptical, feeling – quite reasonably – that there would be far too much anthropogenic noise in the data to allow for any significant climatic reconstruction. Well, look at Keith Briffa's web-site or at Phil Jones's *c.v.* and count the number of dendroclimatic papers. So much for all *a priori* conclusions...

On the other hand, we have benefited from a number of serendipitous convergences: Bernd Becker came along just as large-scale dredging operations began on the River Main oaks, and he and his successors had the imagination and energy to exploit them. Mike Baillie, snoring away on the train from London to Durham, awoke from his nap just in time to spot a field full of sub-fossil oaks which yielded him a multi-century-long sequence just where he needed it. Five seconds later, and he would have missed it altogether. The wood was burned for firewood shortly thereafter. The Ypenburg work of Esther Jansma et al., could never have taken place if excavations for ten thousand houses had not taken place. Our work at Herculaneum on *Abies* from the first century A.D. back to the third century B.C. would never have been possible without a bizarre but happy combination of circumstances: 1) the excavations of the Italian Archaeological Service which undertook a massive excavation effort near the Villa dei Papiri; 2) the kindness of the British School of Archaeology in Rome which told me about it; 3) an invitation and permission from Dr. Pier Giovanni Guzzo, the Soprintendente of Pompeii; 4) the energies of my students who did the measurements; 5) the generosity of Bernd Becker who had sent me his unpublished oak chronology over twenty years ago; 6) the intuition of Maryanne Newton who spotted the resemblance between the German oaks and the Herculaneum firs and spruces when the latter end at A.D. 72; 7) the work of Bernd Kromer who wiggle-matched the sequence with a date different from Maryanne's by only two years; 8) the work of Kurt Nicolussi who investigated the firs and spruces from the Via Claudia Augusta in the Lermoos valley; and finally, 9) the kindness of Michael Friedrich to whom I sent our Herculaneum data, asking him to compare our fir with the unpublished Alpine fir (in this case mostly Kurt's data) at this particular placement, and who sent me a one-word e-mail: "Phantastisch!"

5. Underlying all of the above and one of the fundamental building blocks of the success of archaeological dendrochronology worldwide has been the willingness of the dendrochronological family in general to share data and exchange information. In contrast to many other archaeological and archaeometric enterprises, the dendrochronologists from the time of Douglass onward have also involved themselves with the problems and perceptions of the excavators responsible for the sites. Perhaps, as a Czech economist said to me, it is because "one does not have to have four years of calculus in order to understand your work" that we have the productive relationships that we do with the consumers of our information. At any rate, this positive producer/ consumer relationship has been at the core of dendrochronological thinking and practice. No matter how good your work may be, if your consumer does not understand or believe or trust in what you are doing, you will have problems.

Finally, replicatable chronologies are another key to the success of our method. The well-known story of the Northern Ireland/German collaboration and the sorting-out of an error that would otherwise never have been detected should serve as a model for all our endeavors. Remember: first they had to share data with each other.

6. All of the above is the good news. There are factors which govern our work over which we have no control whatever. We are at the mercy of the economies of multiple countries and the uncertain budgets of their archaeological services. If the excavators are not able to excavate, how do we collect? We are also approaching a time of diminishing returns. Can the European oak sequence be extended a hundred years? A thousand years? More? The bristlecone pine workers in North American thought for a while that they would reach 10,000 years in a relatively short time, but that has been an elusive goal for many years now. Can we work profitably further afield? Asia is wide open. I hope that Mike Baillie

will address this in his half of today's presentation and that some of you will have insights which I lack.

Above all, it seems to me, is the problem of our individual mortalities and lab survival. Not everybody is going to have the luck that Douglass did with the establishment of the Laboratory for Tree-Ring Research. (Of course it *does* help when the dendrochronologist becomes the president of the university.) It is a tribute to the foresight of the Swiss Forest Research Institute (WSL/EAFV) that they replaced Fritz, not only with one but with *two* researchers. Thank you, Drs. Broggi and Kienast! I have not tried to tally Dieter Eckstein's 1983 list of European dendro labs to see which ones are still in existence, much less to figure out which ones are seriously active, but I would be midly surprised if more than half were functional. Nor do I have a count of the private labs in western Europe. A tally of recent contributions to *Dendrochronologia* might be informative. Perhaps we could come up with an *ad hoc* count here at Davos. But it seems to me that a vital factor in what we all are doing is a serious consideration of succession. We will die. It is an actuarial certainty. Then what? For years the names Hollstein and Trier were practically synonymous. After his death I have heard very little from Trier, and I hope that it is just because I read the wrong journals. Has anybody succeeded Kolchin in Moscow? I have heard nothing from there. Who will succeed Jeff Dean at Arizona, or Burghart Schmidt in Köln, or Mike Baillie in Belfast, or Cathy Groves and Ian Tyers in Sheffield, or Leone Fasani in Verona? It is high time that we started thinking seriously about these problems and making appropriate arrangements.

Despite these morbid comments from me, for the time being archaeological dendrochronology is alive and well and operating in a variety of creative directions. It remains for Mike Baillie to tell us in what specific direction(s) we might want to go.

Endnote

Prior to the Davos Conference I sent out a circular requesting new bibliographical information from colleagues engaged in archeological dendrochronology. With very few exceptions, noted above, the response was lamentably poor. This underscores the comments made by Ute Sass and the members of the panel that we very much need to communicate with each other in the way that, say, the dendroclimatologists do.

Part of the problem may lie in our choice of publication venues. The dendroclimatologists write in a limited number of journals that everyone else also reads: *Nature*, *Science*, *Quaternary Research*, *Climate Change*, and the like. Those of us who serve the archaeological world, on the other hand, publish in highly-focused journals that are often of limited interest to those who work in other geographical regions. At the very least we should be sending our publication announcements to Henri Grissino Mayer for his monthly list.

A second problem manifested itself in Davos. I, for one, was quite surprised to learn that dendrochronological data in some countries is considered highly proprietary. One wonders what the governments of the various countries will do with the secret tree-ring data. The value, therefore, of the new European dendrochronological "database" is vitiated if there are no data attached and therefore available for use by others (always of course with proper citation and acknowledgment). We must keep in mind that unpublished information is next to worthless. Perhaps we should make a distinction in our terminology between "announcement" and "publication." For me to announce that I have a 1,000 year chronology from someplace or other does nobody any good whatever. Only when I provide some meat in the form of measurements is it at all useful.

I am not aware that any contributor to the International Tree-Ring Data Bank has ever suffered by having other colleagues use the data. The archaeological half of the dendro family needs to keep this in mind. After all, many of the master chronologies which we use daily (and without which we could not function at all) were provided *gratis* by unselfish co-workers. We would do well to follow their example.

References

Ababneh LN, Towner RH, Prasciunas MM, Porter KT, 2000. The Dendrochronology of Palluche Canyon Dinétah. Kiva, 66(2): 267–289.

Dean JS, 1995. Demography, Environment, and Subsistence Stress. In Tainter JA, Tainter BB (Eds), Evolving Complexity and Environmental Risk in the Prehistoric Southwest: Proceedings of the Workshop Resource Stress, Economic Uncertainty, and Human Response in the Prehistoric Southwest, Santa Fe Institute Studies in the Sciences of Complexity, Addison-Wesley: 25–56.

Dean JS, Funkhouser GS, 1995. Dendroclimatic Reconstructions for the Southern Colorado Plateau. In U.S. Department of Energy et al. (Eds), Climate Change in the Four Corners and Adjacent Regions: Implications for Environmental Restoration and Land-Use Planning: 85–104.

Dean JS, Gumerman GJ, Epstein JM, Axtell RL, Swedlund AC, Parker MT, McCarroll S, 1999. Understanding Anasazi Culture Change Through Agent-Based Modeling. In Kohler T, Gumerman G (Eds), Dynamics in Human and Primate Societies: Agent-Based Modeling of Social and Spatial Processes, Oxford University Press: 179–205.

Dean JS, Slaughter MC, Bowden DO III, 1996. Desert Dendrochronology: Tree-Ring Dating Prehistoric Sites in the Tucson Basin. Kiva, 62(1): 7–26.

Gumerman GG, Dean JS, 2000. Artificial Anasazi: Digital People Farm a Computerized Landscape in Prehistoric Arizona. Scientific American, 2(2): 44–51.

Kuniholm PI, 2001. Dendrochronology and Other Applications of Tree-Ring Studies in Archaeology. In Brothwell DR, Pollard AM (Eds), Handbook of Archaeological Sciences, John Wiley and Sons: 35–46

Available also at http://www.arts.cornell.edu/dendro (*s. v.* "bibliography").