Report
Submitted to the National Science Foundation
Climate Dynamics Program

on the

Dendroclimatology Workshop

held at the
Laboratory of Tree-Ring Research
University of Arizona
Tucson, Arizona 85721

June 1-5, 1977

by

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Grant No: ATM75-22378

Laboratory of Tree-Ring Research
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Tucson, Arizona 85721

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TABLE OF CONTENTS

LIST OF FIGURES

I. INTRODUCTION

On June 1-5, 1977, a workshop was held at the Laboratory of Tree-Ring Research under the auspices of National Science Foundation Grant No. ATM75-22378. The purpose of the workshop was for the participants to evaluate the current results of the research project and to advise the project staff as to future directions the work should take. Invited participants included: Roger Barry, Raymond Bradley, Henry Diaz, David Drury, Joan Jordan, Michael Kelly, John Kutzbach, Valmore LaMarche, Jr., J. Murray Mitchell, Jr., William Sellers, Charles Stockton, and Harry Van Loon. Albert Swain attended under the auspices of his own research grant. Additional participants included Harold Fritts, principal investigator, and members of his staff. For a complete list of the participants and their addresses see Appendix 1.

II. OBJECTIVES

The specific objectives of the workshop as outlined in the report and proposal submitted to the National Science Foundation (starting date: January 1, 1977) were: 1) to review the progress on the climatic reconstructions obtained in the project, 2) to acquaint participants with the results and software developed for testing relevant hypotheses, 3) to compare the results to those obtained from other proxy data, 4) to consider continuing the work as a collaborative effort involving some formal organization, and 5) to help plan future efforts.

III. FORMAT AND TOPICS OF DISCUSSION

The format of the workshop was informal with ample opportunity for questions, open discussion, and criticism. The participants were invited to bring with them relevant hypotheses to test. Development of new hypotheses to be tested during the workshop or at some later time was also encouraged.

The first sessions were devoted to a basic orientation and summary of the project work. This included an introduction, a description of the basic data set, a presentation of the calibration and verification procedures, and a summary of the results achieved thus far. Questions and suggestions by the participants were recorded and are compiled in detail for future reference by the project staff.

The second phase of the workshop consisted of 20-minute presentations by each participant summarizing his or her own work. These brief presentations introduced the participants to each other's interests, perspectives, and expertise. The topics are listed in the program outline included in Appendix 2.

The sessions which immediately followed the participants' presentations were devoted to discussion of possible directions which the project and the participants' work might take, including ways to analyze the present climatic reconstructions as well as ways to improve or expand the work.

The final phase of the workshop involved discussions regarding the possibilities for future dendroclimatological research including the definition of group goals, itemization of specific tasks, and the estimation of the time, resources, and personnel to begin a small collaborative effort.

A number of hypotheses were suggested by the participants, and several of them were tested during the workshop. The tests were discussed before the entire group so that everyone could see how the results and software could be used to assess various climatic phenomena. In some cases, the results suggested more work that needed to be done. As a result, a number of new projects will be undertaken by the participants at their own institutions. In other cases, the tests verified that the reconstructions were at least partially correct.

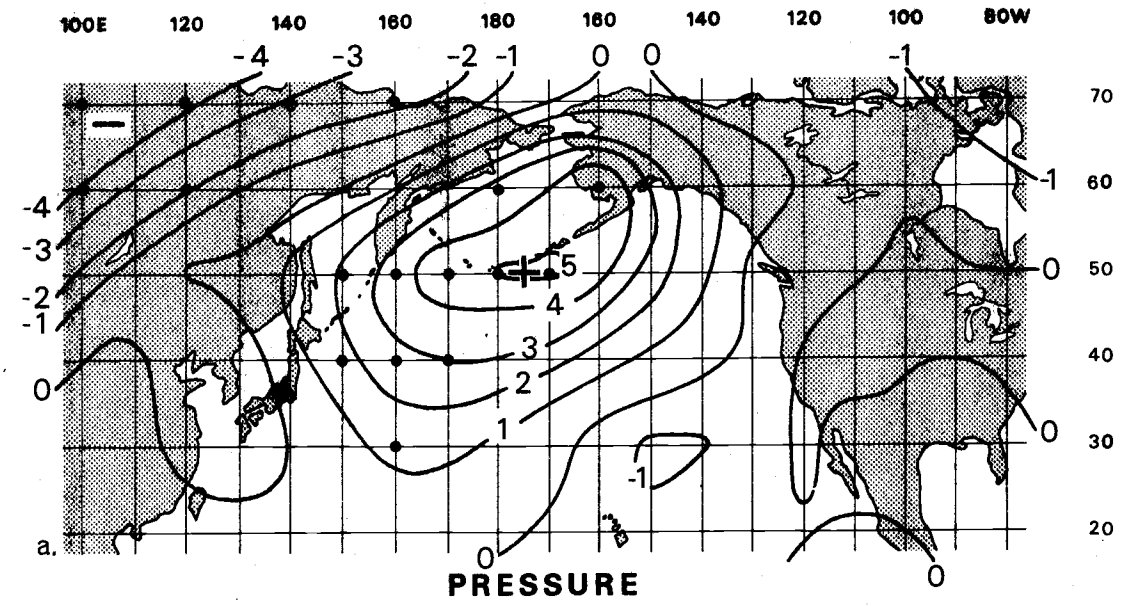
The most interesting example of a test was Michael Kelly's classification of the winter pressure reconstructions into the years of early and late freezing dates published for Lake Suwa, Japan, for both the period of calibration in the twentieth century and the period of reconstruction in the three earlier centuries. The actual winter pressure data and the reconstructions were used for the same years during the twentieth century. Only the reconstructions were used during the independent period prior to 1899. The differences were calculated as years of late freeze minus years of early freeze. The resulting pressure anomaly patterns over the Japanese islands associated with late freezing dates of the lake were similar in all three cases, although they were not as significant prior to 1899 (Fig. 1). This provided good evidence that the independent reconstructed winter pressures near the Japanese islands appear to be meaningful, although, as expected, they exhibit more error than the reconstructions during the calibration interval and, hence, were less significant.

IV. SELECTED SUGGESTIONS BY PARTICIPANTS

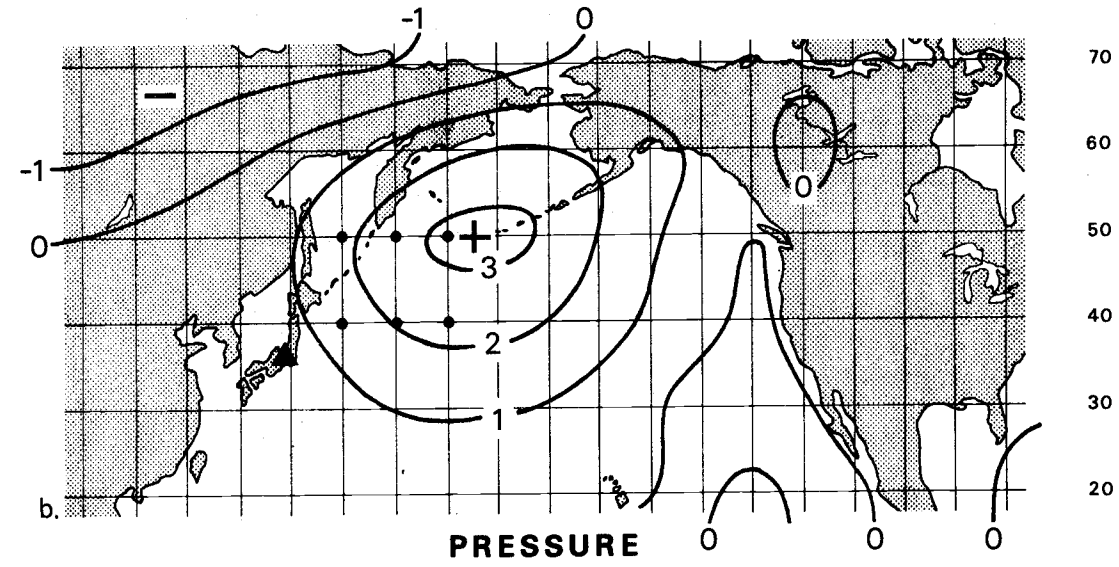
The following are some of the more important suggestions which were offered on the last day of the workshop:

1. The publication of the project results (maps, etc.) should be a separate task from a paper dealing with climatological interpretations.
2. Collaboration should be encouraged with other groups whose resources complement those of people already involved in the research project.
3. After a collaborative working group has been organized and has been given an opportunity to demonstrate the need for further effort, we should consider submitting a proposal to the National

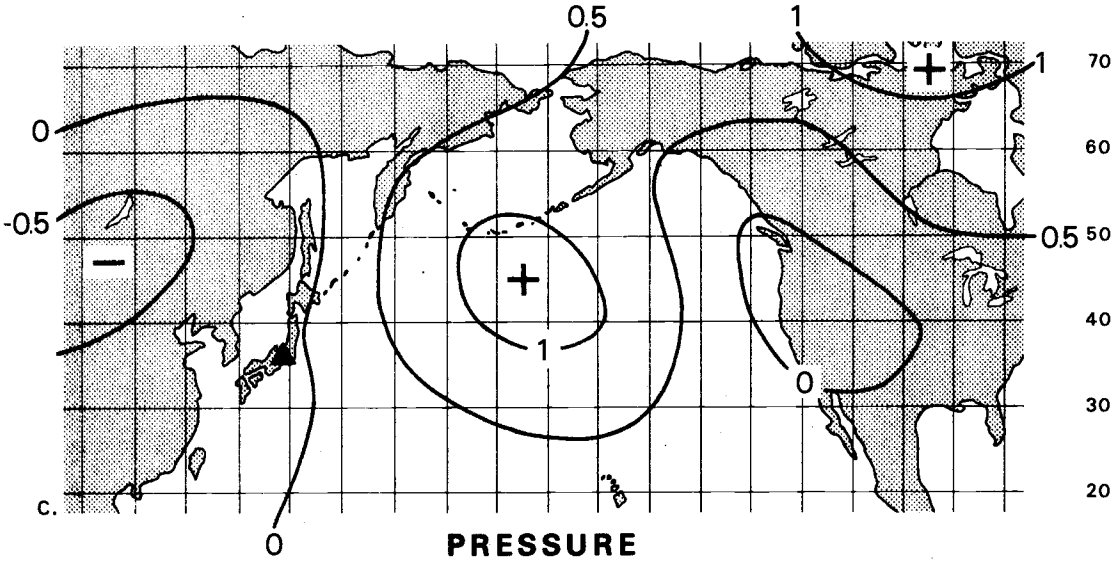
Figure 1. The average difference in surface pressure between years of unusually early freeze and years of unusually late freeze at Lake Suwa, Japan (triangle) for: a) actual pressure data within the interval 1900-1962, b) pressure reconstructions within the interval 1900-1962, and c) pressure reconstructions for the independent interval 1602-1899. Each map represents the difference calculated from pressure anomalies for years of late freeze minus pressure anomalies for years of early freeze. Each map can be interpreted as the mean circulation anomaly associated with late freeze or warm winters. Dots indicate grid points for which the difference is significant ($P \geqslant 0.95$). The standard error for reconstruction includes both the variance among the reconstructed values and their standard error of estimate. The east-west pressure gradient shown on the maps is used to convert the reconstructions to a time series of predicted warm and cold winters in Japan. These data are in turn compared directly with all the Lake Suwa data and with other meteorological data for Japan. This and other methods are used to identify the consistency and evaluate the reasonableness and reliability of the reconstructions as compared to independent information on past climate. One explanation for the relationship between freezing dates on one side of the ocean and tree-ring widths on the other is that there is some general circulation connection between the climates of the two areas and that both proxies express similar time integrals of related climatic factors.



ACTUAL DATA, 1900-1962



RECONSTRUCTIONS, 1900-1962



RECONSTRUCTIONS, 1602-1899

Fig. 1 (cont.)

Science Foundation for additional workshops and for a larger research project.

4. It is important in this next year to determine the particular methods and potential problems of a collaboration group before beginning such a large project.
5. The collaboration group is most likely to succeed if it includes those who have the data readily available as well as the time, interest, and capability to begin work on the particular project.
6. A working group must be open with complete sharing of preliminary data or results so that there is a chance for discussion. Final publication must be shared proportionately to the contribution of each to the work effort.
7. Some restriction of the basic data before publication will be necessary. A suggestion was made to share the data only with those core group members committed to working with the data.
8. Another existing project or publication should not be used to accomplish the task of the proposed collaboration, as the purposes of different projects are likely to become mixed and confused.
9. The task for the first year should be to test data, to improve models, etc.
10. When the group is ready to look at larger spatial areas, we should take the broader data base and make new calibrations, rather than interpolating "teleconnections" between reconstructions in two or more areas (see comments by J. M. Mitchell, Jr., in Appendix 3).

There were many other worthwhile suggestions regarding technical problems, the improvement of existing work, and possible directions for future research. As many of these as possible were recorded for consideration by the project staff at the Laboratory of Tree-Ring Research.

V. CONCLUSIONS

On the final day the participants offered to write individual reviews of the workshop including their general assessment of its worth in terms of the stated goals, the work accomplished, and the time spent; an appraisal of the work being done; and recommendations for the future. A copy of each participant's review is included in Appendix 3. The participants were generally favorable and supportive of the research being done and considered the workshop to have been a valuable experience.

One important result of the workshop was an informal agreement among five participants outlining future collaboration aimed at reconstructing the climate of the Northern Hemisphere during the nineteenth century. Members of this group include: Raymond Bradley, Henry Diaz, Michael Kelly, Albert Swain, and H. C. Fritts. A copy of the agreement is contained in Appendix 4. Since the workshop, T. J. Blasing has moved to Oak Ridge National Laboratory and he has asked to be added as a sixth participant.

The general conclusion at this point is that we should begin to formulate the goals of the group and to determine the form it will eventually take. It is conceivable that in three to five years the effort could grow as large as CLIMAP, so that now is the time to establish the groundwork and learn from the successes and mistakes of other organizations. This particular collaboration is unique, however, in that unlike other climatic proxy data, tree rings are time-specific. Also, we are one of the largest

groups working on this specific time domain.

In terms of funding, any moves toward a major effort should be outlined as soon as possible. In addition, future collaboration should be written into the participants' grants for next year. There was some debate on whether to begin making long-term plans now or to wait until next year to draw up a major proposal. Many participants did not seem ready to begin major plans at this time, but rather felt that time was needed to decide upon the methodology and which problems to address.

The tentative plans for the participating group for the next year are to continue to test and verify models and to bring to the next workshop a concrete set of ideas on how to proceed. By December 1, 1977, brief reports on progress in these tasks will be requested from all participants and mailed to Ray Bradley in Amherst, Massachusetts. All participants also will keep Ray informed of changes of address, telephone, etc. In addition, the reports written for the present workshop by the participants and a summary of the proceedings will be circulated to all those who attended the workshop. Also, a report on the Paleoclimatology Workshop at the National Science Foundation is forthcoming and will be circulated among members of the group.

The deadline of 1980 was suggested as a target date for the completion of the first climatic approximation since it coincides with the International Climate Decade and provides a reasonable amount of time for the completion of the project.

In summary, it can be said that the workshop was considered by everyone involved to have been a valuable experience. In response to the interest expressed by the participants, and in an effort to follow through with the proposed collaboration, another workshop was tentatively scheduled to be

held in Tucson in April, 1978. However, due to scheduling problems of the research project and the need for more time to make the necessary data comparisons, this meeting has been postponed at least until early autumn, 1978. The second workshop will emphasize the attempted collaboration by the designated collaborators.

APPENDIX 1

DENDROCLIMATOLOGY WORKSHOP PARTICIPANTS

June 1-5, 1977

Laboratory of Tree-Ring Research
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Harold C. Fritts

John Hunt

Valmore C. LaMarche, Jr.

G. Robert Lofgren

Judi Sherwood

Donald W. Stevens

Charles W. Stockton

Larry Winter

Martha Wiseman

APPENDIX 2

DENDROCLIMATOLOGY WORKSHOP
June 1-5, 1977
Laboratory of Tree-Ring Research
University of Arizona
Tucson, Arizona 85721

Wednesday, June 1

Morning: Registration

Welcome by Bryant Bannister, Director
Laboratory of Tree-Ring Research
University of Arizona

Introduction by H. C. Fritts

Description of Basic Data Set (G. R. Lofgren)

Calibration (T. J. Blasing)

Afternoon: Verification (G. R. Lofgren)

Description of Results (H. C. Fritts)

A. Analysis of Variance
B. Frequencies of Types
C. Probability
D. Mean Pressure Reconstructions
E. Regional Averages

Dinner at Robert Lofgren's

Thursday, June 2

Morning: Announcements

Summaries by Participants:

J. M. Mitchell, Jr. and C. W. Stockton: Reconstructing the Palmer Drought Severity Indices from Tree Rings and Their Relationship to Solar Activity

R. S. Bradley: Climatic History of the Western United States and Rocky Mountains Using Climatic Data from 1851 to Present

R. G. Barry: A Synoptic Catalogue of Daily Circulation Features for 1899-1974

J. E. Kutzbach: General Circulation Model Experiments

Thursday, June 2, cont.

A. M. Swain: Reconstruction of Past Climate in the Northern United States from Varve Lake Pollen Cores

Afternoon: P. M. Kelly: North Atlantic Climatic Change During the Last 1,000 Years

H. Van Loon: Connected Changes in Patterns of Mean Temperature and Pressure

W. D. Sellers: Predictability of 700 mb Patterns over the Selected Tree-Ring Pressure Grid and Changes in the Variability of Temperature and Precipitation During the Period 1931-1970

H. Diaz: Studies of Temperature Variations in the United States During the Twentieth Century

D. Drury: Data Base Development

J. Jordan: Status and Activities of the NSF Climate Dynamics Program and Status of Climate Legislation

Friday, June 3

Morning: Announcements

Results of Tests by Participants

V. C. LaMarche, Jr.: Reconstructing Temperature and Precipitation at San Diego, California and Santiago, Chile

V. C. LaMarche, Jr.: Dendrochronology in the Southern Hemisphere

H. C. Fritts: Capabilities of Data Retrieval and Testing with Climatic Reconstructions

A. Mean maps and differences
B. Time series construction
C. Tree-growth maps and differences
D. Tree-ring chronologies and amplitude comparisons
E. Probability computations
F. Correlation spectrum and cross-power spectrum analysis

Afternoon: Possible Future Directions in Research (H. C. Fritts)

A. Reconstruction

1. Temperature and precipitation reconstructions
2. Different grids
3. Logarithmic transformation for precipitation
4. Regional vs. individual station data
5. New data and update of old data

B. Verification (H. C. Fritts)

1. Climatic data from other time periods not used for calibration and spatial smoothing
2. Classify pressure types and check against temperature and precipitation
3. Independent tree-ring data from other sources
4. Mapping of verification statistics
5. Extending existing climatic data back in time
6. Historical and other proxy data

C. Combination of Seasons

1. Averaged existing reconstructions for seasons
2. Calibration for longer or different seasons

D. Mapping Climate

1. Time series
   a. 700 mb heights, Palmer Drought Severity Index, Sea Surface Temperatures, 500 mb heights
   b. Crop data and other related phenomena

2. Synoptic interpretations of types to accompany description

E. Time Series Analysis

1. Analysis of regional temperature and precipitation (power, cross-power, filters)
2. Other time series
3. Deterministic predictive models
4. Stochastic predictive models

F. Transfer Functions

1. Manipulate climate data
2. Manipulate tree-ring data
3. Detailed study of transfer function structure
4. Error analysis

G. Climatic Prediction--Stochastic Modeling

H. Hypothesis Testing

Dinner at La Fuente

Saturday, June 4

Morning: Announcements

Results of Tests by Participants

Saturday, June 4, cont.

Preliminary Proposal for Future Dendroclimatological Effort (H. C. Fritts)

Discussion

Lunch at Bum Steer Restaurant

Afternoon: Continued Discussion of Morning's Topics

A. Timetable for possible project
B. Possibilities for collaboration with other organizations
C. Attempts at definition of tasks and goals for coming year
D. Communication problems
    1. Among members of working group
    2. Between working group and other workshop participants
    3. Between working group and scientific community

Sunday, June 5

Morning: Field Trip to Kitt Peak National Observatory

Evening: Bradley, Diaz, Fritts, Kelly and Swain met to form a working group focusing on the climate of the nineteenth century north of 30^{o}N latitude. This group will work together mostly with data that already are available to see what can be assembled for this particular time period. They plan to present results at the next workshop to be held in Tucson around April, 1978.

APPENDIX 3

COMMENTS ON DENDROCLIMATOLOGY WORKSHOP

By

Roger G. Barry

General Comments

By the presentation of an overview of the achievements of, and possibilities for, dendroclimatic reconstructions, the basic objectives of the Workshop seem to have been met as fully as could be expected under any circumstances. The format of the meeting and its detailed organization were outstanding.

Assessment

My assessment of the "state of the art," as it emerged from the presentation and discussions, is as follows:

1. The Tree-Ring Laboratory has evolved a sophisticated technology for extracting climatic information from tree-ring records. Considerable progress appears to have been made in the last year or so in the development and application of the quantitative models.
2. The dendroclimatic approach has several unique features--notably its ability to provide estimates of climatic parameters on a seasonal unit of measurement spanning several centuries. The spatial coverage of the ring records can be extended to a large fraction of the land surface of the globe, and the interpretations can cover even larger areas through the spatial coherence of climate.
3. Preliminary verifications with observations indicate that the methodology has greater potential in the lower frequency bands (10-100 years), and this is a time scale scarcely covered by any other proxy variable.
4. At present, the scope of the verification is still limited, and there are some conflicting results as well as some positive agreements.
5. The Tree-Ring Laboratory group is to be congratulated on its accomplishments under the initiatives of Hal Fritts and Val LaMarche and their personnel.

6. There was little mention of densitometric work (for reasons I'm aware of). It would be useful to have information on its status and potential for adding additional information.

Recommendations (no order of priority)

1. This type of meeting (including the present attendees, but not necessarily limited to them) could usefully be repeated in twelve months.
2. The continuation of verification efforts, stimulated by this meeting, is essential. These should not delay application of the present "best" models to interpretation of the records, however.
3. A first-approximation climatic interpretation of the ring records, via maps and selected time series, should be published.
4. Interpretation should cover both hypothesis generation based on examination of the above products (i.e., empirical interpretations) as well as hypothesis testing (solar, volcanic dust forcing, stochastic interactions such as air-sea processes, etc.). Collaboration with the outside representatives can assist in structuring appropriate questions to ask. These might include the rate and magnitude of climatic shifts, the role of displacements in wave troughs versus latitudinal displacements in fluctuations of 10-100 year scale; further delimitation of type persistences over long time intervals and of spatial boundaries of particular regimes; and teleconnections and their time persistence.
5. The verification methodologies should be reviewed in terms of statistical viability (in one case). Are all possibilities of matching being considered, e.g., what about cases where the gradient, but not the sign, of the anomaly is correctly depicted?

6. Some special studies of no-match situations should be made to try and pin down the causes.
7. It might be more effective for the next one to two years to focus on the better-reconstructed seasons and parameters.
8. The data banking plans seem useful. Thought needs to be given to the question of "priority rights", in terms of suitable time restrictions, appropriate acknowledgement, etc.
9. The idea of a "working group" on climatic variability over the last millennium (MILCLIM?) seems useful. Its exact nature and structure, etc., should be allowed to evolve, although obviously this will only happen if a lead position is adopted. The Tree-Ring Laboratory is the obvious place for this.
10. The search for other historical evidence of past climate to help substantiate dendroclimatic reconstructions should be encouraged by trying to involve historians and others.

COMMENTS ON DENDROCLIMATOLOGY WORKSHOP

By

Raymond Bradley

Status of Work to Date

It is clear that researchers at the Tree-Ring Laboratory have the unique capability of providing reconstructions of past climate on the scale of a season or more for an extensive geographical area. As a result there is the potential of not only documenting past climatic variability per se but of testing hypotheses of why climate has changed (i.e., causal mechanisms such as volcanic dust effects, variations in solar output, sea surface temperature anomalies, etc.). The Tree-Ring Laboratory has recognized this capability and built a superb interactive computational system which permits rapid testing of both models and hypotheses.

Future Work

The continuing work on model development and verification are the key areas to be focused on in the future. Workers at the Laboratory recognize this. However, it is critical to decide whether work in the immediate future should turn to expanding reconstructions to a larger geographical area (the Northern Hemisphere) or whether more emphasis should be put on improving current models to increase confidence in reconstructions over the present geographical area. My feeling is that further refinement of the present models is needed coupled with extensive testing of reconstructions using independent data. The present verification tests are not sufficiently consistent to warrant expansion of reconstructions to a larger area. I would recommend:

1. Increasing the tree-ring network density to improve explained variance. A "limited-sample" comparison of pressure reconstructions using tree-ring grid networks of 40, 65, and 89 trees showed a mean increase in explained

variance of +9% (40 to 65) and +7% (65 to 89) (see Progress Report on ATM75-22378 and ATM75-17034, Table I). Presumably, there will be diminishing returns as the network increases in density (and will extend across North America), but I feel that substantial increases in confidence about reconstructions could be made by investing effort into expanding the tree-ring grid. I believe this is of more importance than updating the present network through the early 1970's.

2. Extensive verification of reconstructions. I feel this must be the second major focus of workers in the Tree-Ring Laboratory as well as those workshop participants (and others) who have pertinent data. Verifications should focus on those parts of the grid where explained variance is maximized to avoid rejection of a reconstruction based on independent data from areas where explained variance is relatively low. Particular emphasis should be placed on all available independent time series, not only from the conterminous United States but also from eastern Asia, Siberia, Alaska, Hawaii, etc. In this regard I think it would be useful to compile maps of temperature and precipitation departure maps for pressure anomaly types (as done by Blasing) not only for the United States but for the entire area of the pressure grid, 100°E-80°W.

In short, with reference to page 92 of the Proposal, "Climatic Reconstructions of Northern Hemisphere Tree Rings," I am in strong agreement with objectives 3 and 5 (documentation and improvement of first approximations of climatic variability). I would also agree with objectives 1 and 4 if the emphasis were on the area of the present pressure grid (100°E to 80°W); more work is needed in this area before this group expands reconstructions to Europe. Objective 2 (developing applications

to crops, food reserves, etc.) should be pursued cautiously in view of the relatively low explanation and limited verification of reconstructions to date.

I would like to note that members of the Tree-Ring Laboratory seem to be fully aware of these problems and of the work needed to solve them!

COMMENTS ON DENDROCLIMATOLOGY WORKSHOP

By

Henry Diaz

Ultimately our efforts must be focused on the task of solving one of the fundamental problems of our times: the task of projecting the economic impact on agriculture, on commerce, and in our lifestyles that significant future changes in the climate patterns would bring.

The following suggestions are divided into Administrative and Scientific Operations sections since "management" and "program" must be tied together.

Administration:

Interdisciplinary exchange and collaboration

I suggest that an ad hoc working group be formed with input from the various working organizations (University/NCAR/NOAA, etc.). The goal is to develop a blueprint for future interactive work, i.e., set benchmark goals, work with funding agencies, and, in general, to serve as a channel of information and to facilitate the exchange process. This group would also help in bridging the gap between analysis and interpretation and in providing a framework for data indexing, archiving and retrieving (perhaps involving several data types and sets).

Scientific Operations:

This should take two forms; joint efforts and individual initiatives.

We should:

1. Establish time scales of prediction, e.g., decades, half-centuries, centuries.
2. Develop probability (actuarial) models for forecasting temperature and precipitation patterns in the future. I suggest

that the forecasting skill score be set at the 65% level (minimum).

I suggest verification by using the period of record minus the last decade for forecasting the patterns of the last decade, the period of record minus the last 50 years for forecasting the pattern of the last 50 years, and so forth. Thus, "prediction" should proceed by attempting to reproduce the recent past from the period of record prior to it.

Comment:

The goal of the reconstructions from ring-width data should not be to nail down the exact temperature or precipitation over given areas in a given year but to give a reasonably accurate picture of the broad climatic features over North America and later of other areas of the world over a time span of several centuries. The priority should be in predicting the shortest time span possible and gradually working toward a longer time framework (lower frequencies). The point is that mankind must traverse the next hundred years of time before it can the next thousand and that future progress will be better able (presumably) to answer the question of longer term predictability. In other words, it does us little good to predict the climate over the next thousand years if we cannot predict the climate of the next hundred years.

COMMENTS ON DENDROCLIMATOLOGY WORKSHOP

By

David Drury

The workshop provided valuable insight into the status of data availability and various data problems which have implications far beyond the scope of this group and highlighted those data problem areas which will require some immediate action if the goals of the research groups are to be met. The following summarizes some of the problem areas and makes broad recommendations toward solutions:

Data Problems

(1) Knowledge of Data: It is evident that there are a number of data sources about which little is known but which could have significant impact on the program. Information in some of these sources is little more than heresay. It is suggested that an inventory be made by members of this group of suspected useful sources, for current or projected research, and an effort made to further define the information content, potential usefulness, and time and resources required to recover the data as and when needed.

Our knowledge of existing data in national centers and other institutions is incomplete and not well presented to the research community. This group should formalize its requirements for presentation to appropriate planning and administrative groups.

(2) Data Exchange: While researcher-to-researcher exchange (sometimes ad hoc) has apparently served the research community well in the past, the magnitude of the task in hand and ahead, as evidenced by the context of this workshop, calls for a more formal structure in the future. International exchange will obviously be more important

both for data derived from this program and from other climate groups in the United States and for obtaining calibration and verification data from other global areas. Efforts to locate and obtain this data should begin as soon as possible so as to be available in the desired form and media in phase with results of projected field data activities.

(3) Standards for Intercomparison: The multiplicity of scales, spatial resolution, time averaging, units, grids, etc., evident at this workshop and the attendant problems and uncertainties of intercomparisons flag this as an area for immediate attention. Although the nature of particular research tasks may dictate some of these differences, an attempt should be made to agree on presentation and comparison standards. These should relate to probable interfaces with other basic climatic research and modeling activities which will require the data derived from this program. Efforts towards this end should begin now with more exact definitions of these problems within this group and increased communication with other groups. Other related areas where problems and recommended standards should be documented and developed are: quality control, editing, gap-filling (interpolation, etc.), and documentation.

COMMENTS ON DENDROCLIMATOLOGY WORKSHOP

By

Michael Kelly

I'm making these comments as a climatologist interested in explaining long period climatic change and therefore interested in the low-frequency, broad space scale information in the reconstructions. My comments are limited to the pressure reconstructions. They are in no particular order of importance.

1. The lack of independent pressure data to verify the pressure reconstructions is a considerable restriction in verification. I suggest that you use proxy data for the Asian sector. It should not be difficult to compare these records even if the link between the proxy data and pressure patterns is not well known. As many climatologists are suspicious of the use of western United States tree-ring data to reconstruct western Pacific pressure, this may prove to be a very convincing test.

   For example, there is an $\simeq$ 800-year record of annual freezing dates of Lake Suwa in Japan. By differencing pressure maps for positive and negative extremes in the freezing record, a pressure pattern responsible for freezing variations may be obtained.* Indices of significant features of the pressure pattern can then be used for further comparison with the freezing record.

2. It is important to look at the variation of explained variance with frequency. Spectral and cross-spectral analysis should be useful.

3. Publication of long-term average anomaly maps is essential. The final verification of the reconstructions is their usability by other researchers. However much verification is undertaken at the Tree-Ring Laboratory, if other researchers do not use the reconstructions, they are of little value.

4. It may be useful to look in more detail at the transfer functions, particularly at the statistical significance of the canonical correlation coefficients.
5. It would be useful to look in more detail at the unexplained variance during the calibration period. This may give some idea of possible improvements.
6. The increase of variance pre-1900 in the temperature reconstructions is disturbing. The correspondence with the beginning of the calibration period may be fortuitous, but....
7. Verification of spatial averages of temperature and precipitation are of greater importance than point estimates.
8. Collaboration between those working in the field of climatic change over the last 500 to 1,000 years is essential.

   I suggest: (1) Immediate informal collaboration;

   (2) Publication of joint papers;

   (3) Joint research proposals in three years (1980);

   (4) Workshop next year to review progress and plan (3).
9. Pressure data from the United Kingdom from 1873 should be used if at all possible, even if only to test group features.

*See Fig. 1.

COMMENTS ON DENDROCLIMATOLOGY WORKSHOP

By

John Kutzbach

General

I feel that the research work has reached a significant milestone in terms of: (1) the size and length of the tree-ring data set; (2) the understanding of tree-growth response to climate; (3) the techniques for transforming tree growth patterns to pressure, temperature and precipitation patterns; (4) the statistical procedures for assessing and validating models.

I fully concur with the plan to publish the basic "level 1" (tree rings) and "level 2" (climatic reconstructions) information, the latter as a "first approximation of climate." The results have reached a stage where complete documentation and publication will be useful for at least two purposes: (1) hypothesis testing and preliminary "applications," and (2) permitting detailed study of the models that have been developed.

At the same time, work should proceed on the preliminary analysis and synthesis studies (times of climatic change, characteristics, implications, etc.).

The sorts of collaborative studies exemplified by various initial studies at this workshop should be encouraged. For example, I'm excited by the sorts of tentative agreement that we noted between the pollen record from laminated lakes and the tree-ring record (both the 400-year record of Fritts and the 2,000-year record of LaMarche). I look forward to continued collaboration. Although focus of the meeting was on the work of the Fritts group, the work of LaMarche and Stockton is of high quality and relevance to overall goals.

Specific Comments

(1) I see a need for continued study, summarization, and publication of the "level 1" data. For example: a new set of maps of the growth variations themselves; estimates of tree-growth variance as a function of time; time series work; estimation of natural variability analogous to work of Madden at NCAR (MWR 1975). I talked to Fritts about the last item last fall and think something could be done in this area. I will correspond further on this.

(2) With regard to the typing scheme for representing past circulations, we haven't had time to go into this in detail, but I'm concerned (as is the Fritts group) about the problem (common to all typing) that relatively low similarities (correlations) must be accepted in order to "type" most of the maps.

(3) a) The point of validation with respect to frequency characteristics of reconstructions was stressed by many of us. As a result, I feel that the validation of short records (10-20 years) is of limited importance (except for the opportunity of validating how the model reconstructs extremes). The importance of validating against century or longer regional chronologies was stressed. (I started a small attempt at this at the workshop and will be interested in the results. I refer to the sum of New York, New Haven, Charleston records; time series, probability and cross-spectrum analysis, 1830-1900.) The initial attempt illustrated the need for revising a program to permit the use of stations that have minor gaps in record--especially since emphasis is on the low frequencies.

b) Also with regard to validation, I was very impressed with the examples of validation against independent tree-ring chronologies. It was mentioned that LaMarche had a long record that was not in

the "Fritts" network and that it nicely validated with the "Fritts" reconstructions. Since instrumental records will almost always be too short, I feel that perhaps even greater emphasis should be placed on developing validation techniques from independent tree chronologies.

(4) With regard to future work on the "second approximation of climate,"

a) I feel you should consider a series of simulation studies in order to estimate the amount of improvement in representation of large-scale pressure patterns to be expected with increased tree-site density and/or increased geographical coverage. I think this could be done using point pressure records calibrated against functions of large-scale pressure patterns. It could be done easily with your existing computer programs. This procedure should help to define future sampling strategy. It might also help to define a more optimum pressure grid for the present tree network. I would be willing to help with this.

(b) Consider alternative methods for selecting the number of canonical functions in calibration procedures such as splitting the data into two sets (like Webb and Bryson) in addition to the F-test. Also, how does calibrated variance change in its <u>spatial</u> distribution as more canonical variates are added?

(5) Display of Results: I mentioned the use of "trough-ridge" (Hövemiller) diagrams. These would provide a lot of information about the longwave patterns in the pressure field and their variation with time without resorting to typing. It might provide an alternative means for identifying climatic regimes over the same period (1600-present). By superimposing a measure of pattern variance on a smoothed version of

a trough-ridge diagram, it might also be possible to depict high- and low- frequency variability simultaneously.

I will be glad to help with details.

(6) Ideas on collaboration: a) Let's develop a schedule of joint work for the next two years including proposed joint papers. We can do this in the next few weeks.

b) Val LaMarche's comment on moving toward a large "CLIMAP-type" effort, say within three to five years, is a good idea. We should aim at this more formal collaboration in the future. Focus should be on the last 1-2,000 years.

COMMENTS ON DENDROCLIMATOLOGY WORKSHOP

By

J. Murray Mitchell, Jr.

General Statement

As an interested follower (and supporter) of the Tree-Ring Laboratory's dendroclimatology program for the past 16 years, I have never wavered in my enthusiasm for this program as the key to reconstructing many details of the earth's climate chronology over the past several centuries. It would indeed seem that the present status of this program (primarily that part under the leadership of Dr. Fritts, hereafter referred to as the HCF group) indicates that a plateau has been reached in the ability to specify general climate parameters (in the North American-North Pacific region) from the Laboratory's tree-ring data base. Roughly 50% of the large-scale climate variance in that region is "explained." This, together with the evident pressure being placed on the HCF group to apply this research, has apparently tended to encourage the HCF group to think in terms, while maintaining those parts of the program concerned with extending and refining the "Level 1" tree-ring data base (to other areas of North America and elsewhere), of adding new emphasis to those parts of the program concerned with applying the interim transfer deductions about climate to as many problems in the climate arena per se as possible.

While I am in agreement that the transfer results to date should be applied to studies of climate per se, I suggest that any major change of emphasis in this direction, on the part of the HCF group, be resisted on the basis of the following considerations:

1. The climatic variance thus far explained is encouraging but in my view is not overwhelming. Much more in the direction of verification of model proficiency can and should be documented in quantitative

detail in the period of meteorological data where verification would be relatively unambiguous.

2. There is a pressing need to extend the geographical coverage of the tree-ring data base beyond the western North America domain thus far used as the basis of the climate reconstructions. This Laboratory should, in my view, be the place to coordinate the collection of the supplemental tree-ring data (either through its own field work or through collaboration with other groups nationally and internationally) and to become the central data repository for all the tree-ring data along the lines now contemplated. With regard to longer-range planning, this data-base extension should remain a principal focus of the Laboratory in general, if not necessarily the principal focus of the HCF group in particular.

3. In my view, the time to press hardest for the best climate reconstructions is not yet at hand. The main effort of the HCF group in the climate reconstruction arena should come later, not now, when:

    (a) The data base for the enlarged geographical domain referred to above in (2) will be in hand;

    (b) High-resolution data from other sources (e.g., pollen profiles, isotope profiles from both tree rings and ice-cap cores) will have become available to merge into the climate reconstruction transfer functions; and

    (c) Better climate diagnostic models will have become available to help in the process of deciding to what extent the tree-ring and other data bases are geographically adequate to capture all of the most significant modes of (large-scale) climatic variation over large geographical domains.

The appropriate time for the really major push in the climate reconstruction arena, then, might come three to five years hence. This is not to say that reconstructions done to date should not be exploited to the hilt, only to say that present reconstructions (and the models on which they are based) should be viewed not as a fait accomplis but as a stepping stone to better reconstructions (and models) that will become feasible in the next few years.

The bottom line of my concern on these points is that I hope the HCF group will be inclined to keep pushing forward its vital work in dendroclimatology, and to delve more into climatology and climate hypothesis testing per se only at a measured pace that will not interfere with ongoing work at the more fundamental level of broadening the basic data base needed for doing a better job of climate reconstructions.

The work of the HCF group should, then, be clearly separated into two parts:

(a) The broadening of the tree-ring data base and the verification of tree-ring-climate relationships; and

(b) Applied climatological applications of the present data base that the HCF group staff would like to make (either on its own or with collaboration of other individuals or groups).

In my judgment, part (a) should remain the principal focus of the HCF group's effort in the next few years in order to take best advantage of the mix of the very unique and special talents and expertise of both the HCF group and the Laboratory as a whole.

SUPPLEMENT TO COMMENTS

By

J. Murray Mitchell, Jr.

Following the informal discussion held between many of the workshop participants late Saturday afternoon (after the departure of Jordan, Kutzbach and Barry), I feel that some very essential conclusions reached in that discussion are worthy of summarizing here--realizing that I can do so only through the color of glasses that is uniquely mine. These conclusions tend to reinforce some conclusions I have already volunteered in my critique submitted earlier today.

(1) The most significant part of the Laboratory's and the HCF group's contributions to date is the basic "Level 1" data base, i.e., the tree-ring indices themselves for the 89+ sites that are demonstrated to be climate-sensitive plus other indices from more geographically far-flung sites that are available but not yet incorporated into the basic 89-site base.

(2) The transfer-function models, together with the reconstructed fields of climate per se that are based on these models, undoubtedly have considerable value in their own right, both in indicating the general chronology of climate in the North American and North Pacific sectors since 1700 or so, and in indicating the general quality and diversity of information about climate that can be inferred from the tree-ring data base.

(3) The reconstructions referred to under (2) should properly be regarded as the nucleus of an in-house HCF group program for drawing further conclusions about the nature of historical climatic variations, and

the Laboratory should be encouraged to pursue this work as its staff sees fit with whatever outside collaboration that seems appropriate and mutually desirable.

(4) From the viewpoint of other individuals or groups that have their own specific interests in climate and climatic variability and who might have access to the Laboratory's basic tree-ring data base, it would seem preferable that those other individuals or groups attack their problems not by going indirectly from the tree-ring data through the HCF group's transfer models to their end product, but rather by going directly from the tree-ring data to their end product. This means developing their own transfer models, utilizing all the expertise developed by the HCF group in designing optimum transfer models in each case, and capitalizing on the fact that a direct transfer of this kind would be highly likely to "explain" a greater total climatic variance (in the form of direct concern to them) than would an indirect transfer through the HCF group's own models.

(5) At some appropriate future time the tools developed in the HCF group to infer past climates through tree rings should be brought alongside tools developed elsewhere to infer past climates through other proxy media, such as pollen analysis, ice-cap core analysis, and even tree-ring isotope analysis, and applied in concert with these other tools to undertake an interdisciplinary program aimed at doing the best possible job of reconstructing the climatic history of large areas of the world in the past several hundred years. This is more than simply a matter of verifying what tree-ring materials have to say about climatic history; it is a matter of using all proxy material to arrive at a best estimate of that history, on a coequal basis to whatever extent each proxy data form is found able to contribute.

(6) It is not until such time as we have arrived at (5) that a consortium of dedicated individuals and organizations should be formed to undertake a definitive, "state of the art" climate reconstruction effort along the lines of CLIMAP.

(7) I see no reason why we cannot reach stages (5) and (6) within a very few years, and I see no reason why the HCF group would make anything less than an absolutely vital and pivotal contribution to these efforts when the right time comes. Meanwhile, there is no end of preparatory work to be done, on a relatively individual and unstructured basis, to occupy HCF's group in particular, and the Laboratory in general, with these goals in mind.

COMMENTS ON DENDROCLIMATOLOGY WORKSHOP

By

Albert M. Swain

I am impressed with the current status of tree-ring reconstructions regarding temperature and precipitation. The fact that pollen data verify with the tree-ring reconstructions at least in western North America is certainly encouraging. The results appear to indicate that there is potential for much finer resolution of climatic changes with pollen data than previously realized. In regard to verifying pollen results from Pennsylvania and New York, I noted that there is a conspicuous absence of climatic stations along the Hudson River and into northeastern Pennsylvania in the array of climatic stations that you have been using for reconstructions. Will stations such as Albany or Philadelphia, which have long records, be added to the array, or will they be used for verification purposes?

The idea of publishing your initial maps of tree-ring data and reconstructions in 30-year modes seems attractive from a biologist's viewpoint since the minimum response time for some forest ecosystems to respond to perturbations resulting from climatic change is about thirty years.

A strong effort should be made to increase the network of tree-ring chronologies in the eastern United States and to develop techniques for analysis (density measurements, isotope studies, etc.). This data set should improve the reconstructions for the East and should verify results from the western data set. More use of the long climatic records of meteorological data from various meteorological stations in eastern North America should be used for verifying climatic reconstructions.

In some areas direct climatic reconstruction may not be possible from pollen data because of the lack of modern analogs. Perhaps a tree-ring reconstruction

can be used to calibrate fossil pollen data in such cases provided that the subjective reconstructions based on pollen analysis verify with the tree-ring reconstructions.

Based on our success with the use of transfer functions with pollen data and log transforms of precipitation in northwest India, I would encourage you to attempt some reconstructions with tree rings where you have transformed rainfall data to logarithms.

COMMENTS ON DENDROCLIMATOLOGY WORKSHOP

By

Harry Van Loon

I cannot pass judgment on the dendroclimatic project without first trying to use some of its data in experiments where some control can be made. I therefore propose to do the following three experiments:

(1) We have a list of all winters and all Januaries from 1840 to 1975 when the West-Greenland/North European temperature anomalies were $\geqslant 4^{\circ}C$ apart, one region being below and the other above the long-term mean. We know which pressure anomalies are associated with the two states of temperature anomalies. We would like to see if the reconstructions will reproduce the pressure anomalies since 1899 and to see if the reconstructed anomalies from 1840 to 1898 will show the same pattern. At the moment, we are trying to extend the series of opposing temperature anomalies back to 1700 by means of proxy data. When the list is ready, we'd like to test this period too.

(2) Harry Wexler (*Tellus*, 1956) used the 1899-1939 daily sea-level pressure series to examine the differences between mean pressure patterns at sunspot maxima and sunspot minima. His work could be extended to the 1970's with Roy Jenne's accumulated pressures to see if his results are still valid or are owing to sampling. If they are valid, the reconstructed pressure grid <u>for the same period</u> (1899-1975) can be applied to see if that will reproduce the results. If so, the reconstructed pressures can be used for the entire period when reasonably trustworthy sunspot numbers can be obtained (for example, after 1750 or 1820, or both) to see if the patterns produced are the same as those from actual pressure in the twentieth century

record. The tree-ring index can also be used directly to see if there is any significant difference between sunspot maxima and sunsot minima.

(3) Ebdon (*Met. Magazine*, 1975) investigated the difference in sea-level pressure between the periods when the Quasi-Biennial Oscillation was in its westerly phases, and found statistically significant differences between the two modes. This can be tried with the reconstructed grid for the same period; if it reproduces Ebdon's results, the reconstructed period can be scanned for maps similar to Ebdon's, and a spectral analysis can be made on them to see if they have a two to three year period. Perhaps the tree-ring index proper can (and should) be used for this purpose too.

APPENDIX 4

THE UNIVERSITY OF ARIZONA
TUCSON, ARIZONA 85721

LABORATORY OF TREE-RING RESEARCH

June 28, 1977

MEMORANDUM TO: Dendroclimatology Workshop Participants

REGARDING: Guidelines for "Climate of the Nineteenth Century Project" evolving primarily from a working group which met at the end of the workshop and which included Bradley, Swain, Diaz, Kelly, and Fritts, June 5, 1977 (prepared initially by Kelly and edited and revised by Fritts).

The following are suggestions and guidelines which the above participants agreed upon to facilitate collaboration work:

1. It is worth as a start to attempt to put together all existing data to see how well we can reconstruct a century of climatic variation.

2. The groups or projects associated with Swain, Bradley, Fritts, and Diaz will start assembling climatic data for the nineteenth century, will periodically inform other members of the particular groups of the status of their work, and will attempt to put together the details of a climate chronology of the nineteenth century.

3. The next Tucson workshop in April, 1978, (rescheduled for early autumn, 1978) should focus at least in part on the climate of the nineteenth century.

4. A project will be initiated to cover the Northern Hemisphere north of 30°N latitude where data are available, but the effort will focus on fitting existing data into a framework, rather than first trying to expand the existing data base. This will serve as a test of how well we can do with existing evidence and will help us to identify the major problems in such a collaborative effort. We will keep expanding all data as we are doing now.

5. No firm suggestions about seasons were made, except that the tree-ring reconstructions are based upon winter: December-February; spring: March-June; summer: July-August; and autumn: September-November. Since it will be difficult for recalibration of tree rings for this project, tentative acceptance of the above seasons on the first go around would be preferable.

6. Winters will be dated by the year of the January.

7. Anomalies will be based on the period 1931-60, wherever possible, although it is not as climatically homogeneous as 1901-30. The later period is advantageous in that more data are available. The period 1941-70 presents problems for the tree-ring based reconstructions because the last date of common tree-ring cores is 1962.

8. Ten-year, 30-year, and 50-year averages are to be calculated, if possible.

9. The 1801-1900 mean is to be presented so that nineteenth century short period anomalies can also be considered relative to that mean.

10. Decades will run from 1 to 10, e.g., 1801-10, 1811-20.

11. In order to facilitate communications, letters and data shared between members of the group should be copied and sent to all members of the active working group. Therefore, each one will be kept abreast with the other's efforts.

12. Diaz agreed to help obtain a continuous statewide mean, but the effort proved too expensive. Therefore, the 1900-1940 series was to be compared with the 1931-1960 series by Fritts' group to see if the two could be merged. An earlier plan to calculate statewide averages under the old method was abandoned as too costly for Asheville to undertake.

13. Bradley reminded the others that he did not have a large staff and could not respond as thoroughly and as quickly as some of the larger groups.

14. Kelly and Fritts agreed to make a number of verification tests. They reported (after the conference) that a significant pressure anomaly was found over Japan related to freezing data of Lake Suwa.

15. Fritts agreed to send, and subsequently mailed, the correlation coefficients of each pressure type with the actual pressure data and with the reconstructions.

Harold C. Fritts
Professor of Dendrochronology

HCF:dd