2 KYR PROJECT MEETING SUMMARY
SKAFTAFELL, 5 MAY 2007

32 participants, including 8 PIs:
Ammann, Anderson, Axford, Bensonen, Black, Bradley, Briner, Clegg, Cook, Francis, Geirsdottir, Helfrich, Jennings, Kaufman, Larsen, Leon, Lewis, McKay, Miller, Olafsdottir, Olafsdottir, Palsson, Porinchu, Quillmann, Retelle, Roop, Schiff, Schneider, Sowder, Szymanksi, Thomas, Werner

Decisions:
We voted to use Arctic Workshop 2008 as manuscript deadline, and an opportunity for a special session and discussions. At a minimum, we should be prepared to undertake intraregional syntheses at AW '08. It was also proposed that we should each provide excel files of our records in std dev units before the AW '08, to create a strawman compilation.

For those who will attend AGU, the meeting will provide an opportunity to get together (eg to brainstorm intraregional comparisons)? ARCUS could provide facilities for a meeting. We could have a video/webcast-conference so project members could participate from afar.

We voted to use the AD scale for presenting time series, with time progressing from left to right (ie we'll emulate IPCC and recent syntheses).

Actions Items:
Please update info links on the 2k project web site. If you have a project web site of your own, link to it.

Contribute data to the 2k website (password-protected data area) as they become available. Contact Matt Duvall to make this happen.

2k researchers can e-mail Caspar/Dave the coordinates of our sites, and which climate parameter(s) we think we're reconstructing, and he can provide a time series of that climate parameter generated from the recent model run of the last 1750 years (model results are published so these data could go in manuscripts)

Summary of Discussion:
A reminder of the timeline given in the original proposal:

- Yr 2 (by 3/07): complete majority of analyses, meet to compile results, finalize modeling design, hire postdoc
- Yr 3 (by 3/08): final analytical results, produce prelim synthesis, begin modeling
- Yr 4 (by 3/09): complete modeling and data-model comparisons; integrate results with international results

The ideal strategy for a synthesis would be: publish records → synthesis → modeling. But we probably require a phased synthesis, e.g., starting with intra-regional then inter-regional syntheses, leading to a high-profile synthesis of all sites and perhaps a larger international/circum-Arctic synthesis.

Eventually, in constructing a synthesis, we'll have to grapple with some big questions:

- Realistically, what type of synthesis are we shooting for? Given that:
  - some records are <2000 years
  - some records are not annually resolved
  - some records are not quantitative climate records
  - some records may not be published before the synthesis
- Are we aiming for an "average" record? Or do we want to preserve & examine spatial variability?
- How will we deal with including unpublished data? The synthesis is likely to be heavily scrutinized, and published records (and data that are publicly available) will be easiest to defend.
- Do we want to recruit/include other records (in addition to our lake sites)? Expanding the record is appealing (more sites, more proxies, broader geographic coverage), but on the other hand, we may want the first-step synthesis to include only lake sediments.
- How we decide to analyze and synthesize the paleodata will depend upon the questions driving the synthesis… so decisions about these things may need to wait until we see what data we have to work with (eg at AW '08)

There was an interesting and animated discussion about geochronology. Clearly chronology is one of the big challenges of the project (probably rivaled only by the challenge of generating well-calibrated quantitative paleo-temperature inferences). It was agreed that, as with temperature inferences, it is essential to characterize errors as best we can. But how to reflect age-model errors in time series/syntheses?

- give greater weight to records with lowest uncertainties
- report "average error is __; ranges from __ to __" with each record
- use uncertainties to evaluate, eg, "with what certainty can we say that the LIA occurred at the same time at different sites?"

Given inherent problems with ^{14}C and ^{210}Pb, tephra and anthropogenic markers (e.g., SCPs) are of increasing interest and importance.

Darrell demonstrated age models created with OxCal and the Heegaard et al. (2005) spline fit routine that can be run using R. Darrell volunteered Caleb to take our ^{14}C ages as tables and run the Heegaard spline function in R for us – send data if you want to. In return you'll get a table of ages and uncertainties with depth.

Caspar suggested that it is not too soon to get started with some model-data experiments, even with preliminary time series. To begin, we can try model-data comparisons of our paleodata reconstructions vs existing model reconstructions since 1750 (see "Action Item" above). We can also aim to compare spatial patterns and amplitudes of reconstructed vs modeled climate change in recent times as a first test.

Project involves 14 PIs, 11 collaborators, 22 grad students, 18 undergrads/techs; 8 PIs are present today. We proposed 30 new 2kyr records. There were only a few at the time of the proposal, so even half this (one solid record per PI) would be a major improvement.

What's next for this project? Are we building momentum and should we plan for another proposal?

2 KYR PROJECT MEETING NOTES (COMPLETE)
SKAFTAFELL, 5 MAY 2007

Project History/Goals:
Project involves 14 PIs, 11 collaborators, 22 grad students, 18 undergrads/techs; 8 PIs are present today.

We proposed 30 new 2kyr records. There were only a few at the time of the proposal, so even half this (one solid record per pi) would be a major improvement.

Our budget boils down to ~$40k direct costs per lake. Maybe we undersold this, but maybe it's a foot in the door to something bigger if we're successful.

Our primary questions:

1. Is the 20-th C warming unprecedented in last 2 kyr?
2. what are spatial-temporal modes of climatic var, including AO?
3. how do the extreme mean climate states (eg LIA, MA) relate to these modes?
4. What part of clim var is explained by changes in solar irrad and volcanic activity?

Proxies:

Primary: laminations, chironomids, isotopes
Secondary: BSi, OM, pollen, diatoms, C/N

Proposal timeline:
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Yr 3 (by 3/08): final analytical results, produce prelim synthesis, begin modeling
Yr 4 (by 3/09): complete modeling and data-model comparisons; integrate results with international results

We initially proposed a first synthesis in spring 2007; priority was to be 850-1250 AD ("longest interval of relative warmth during the last 2kyr")

Reminder: We have a web site, useful for coordination and also for outreach/exposure.

- There is passwd-protected data area, and noone has yet used it.
    - Clegg points out the data need to be in final form to be shared on web site

AI: Can people update site info links? If you have a project web site, link to it.

Project future:

Caspar: on a global scale, it's typically only the last 30 yr or so that are unusual, NOT literally 20th C. and uppermost part of record may often be missing/disturbed/etc.

- Maybe our question should be "is the last part of the 20th c unprecedented?"
- We've mostly been doing well with getting intact surface, but there are issues of diagenesis, density changes, etc at surface
- Calibrations may be problematic because the modern system is not 'normal' – ie there's no baseline left; separating effects of warming from other anthropogenic overprinting (eg nitrogen dep) is a challenge for some sites

Should we consider including records that don't meet minimum resolution (30 yrs)? Records that are shorter than 2000 yrs?

For the synthesis, do we want to solicit other records? Expanding the record is appealing (including tree rings, etc).

- But shouldn't we aim to do a synthesis that is only lake seds (at least as first step)?
- On the other hand, some modeling experiments will require tests vs paleodata from other parts of the world (not just our lake sites)

How to handle data that aren't published before synthesis?

- We need to be very careful that all data included in the synthesis are publicly available, and preferably peer-reviewed. We will be SCRUTINIZED. Ideally as many *published* records as possible.
- We may be a lightning rod – and therefore need to be extremely careful to document our decisions and be ready to publicly defend them.

<u>PI UPDATES</u>

Giff Miller
Baffin Island ice caps indicate sustained warming of recent years exceeds MWP (some presently disappearing ice caps have persisted since 1650)
Ice caps indicate MWP AD 1000-1200 (abrupt onset of ice cap growth at 1200 AD – early onset of LIA)

Chironomid record from Stora Vidarvatn

Midge people need to help us understand the meaning of the error bars

Plot as delta T instead of T? = smaller error bars

Importance of publishing taxonomic data along with temp inferences; and those data should indicate which taxa are cold/warm

Hvitarvatn

Beautiful laminations, but struggle to correlate their thickness with any climate parameter – can have multiple "spring melts," duplicate varves, etc

Ray: ice core folks use many diff proxies to resolve annual layers; we should use multiple parameters too (eg ITRAX scanner)

Record of HTM & Neoglaciation; but chronology is problematic (tephras will help)

Haukadalsvatn

High-sed rate organic lake (2 cm/yr); BSi and TC are completed at hi-res; Kristin & Olafur are doing time-series analyses on these data

Summary:

Glaciers are simpler and more reliable than biology
Secure geochronology better than +-200 yr is often tough
Signal to noise is a challenge: can we quantify bio proxies with sufficient precision?
Are varved Icelandic seds just a pretty face?

Ray Bradley

Some points about prior synthesis reconstructions: differing numbers of records for different intervals; many records end in 1980s so recent part of curve is instrumental; there have been changes in how some proxies track climate lately, especially at high lats and altitudes (due to ozone depletion, nitrogen dep, loss of anoxia in varved lakes, etc), so it's hard to extend records to finish line (2007).

Some geochemical proxies are very promising. We haven't done much of this yet in the arctic. Eg uk37, tex86

Could jokulhlaup events (recorded in lake sediments) provide a history of agassiz ice cap fluctuations? Apparently recent decades, which have seen these jokulhlaups, are unique.

On laminations: microtopography in lakes, by creating sites of anoxia, has huge effect on whether you find laminations

Murray lake varve thickness shows a hockey stick pattern – new data from last decade indicate warmer temps

No obvious way to put a number on temps from this record; weather stations too far away to calibrate

14C doesn't work here, but varve counts match well with independent magnetic data
How to adjust for effects of water content in upper seds?

Sawtooth lake varve thickness has been measured; image analysis also done; interpretation yet to come

Pierre has identified sand layers that represent summer rainfall events; can remove their thickness from varve thicknesses; important to think about how to deal with short-lived events in varve stratig

Lakes C1, C2, C3

Radar images from 2001, 2002 showed ice-free conditions in summer; project aimed to see if lake conds have changed dramatically in recent years

Challenges:

Sparse spacing of weather stations hinders calibrations
Need independent chronological marker to test varve chronologies (cryptotephra?)

Al Werner
Linne

Laminations are complex, and it's hard to find a consistent way to identify an annual layer (not simple, obvious couplets)

Efforts to compare lamination thickness records with glacier mass balance, climate data, etc. Results equivocal

Studies of sedimentation processes show: spatial differences in laminae; notion of "winter clay, summer silt" not correct for Linne, where spring brings coarse seds but sed traps show clay deposited in summer; sed storage in upstream braid plain complicates transport of glacial seds to lake, making high-Q events very important (small fluvial systems = better)

Kongress sinkhole may have better potential for varve work?

Mike Retelle
Study sites on devon, Cornwallis, and Bathurst islands; some may be "collaborative sites"

Cape hurd lake is at sea level; saline & meromictic

Upper unit is massive, overlying laminated seds; chronology also from 210Pb, paleomag, 14C on fish parts

Laminae are very fine – 0.5-0.6 mm per couplet – and complex, with interbedded turbidites, etc; multiple cores might help to develop a master chronology

Lamina counts & thickness measurements are highly sensitive to how you define a couplet

C, N, biogenic silica show high-amplitude changes in last ~400 yrs, bigger than changes between 2000-400 BP

ITRAX Ca data may be good proxy for rock flour in this carbonate terrain?

Temperature reconstructions can be calibrated with Resolute met data

Lots of work on Depot Point Lake, too

Jason Briner
Baffin Island
Chironomid records anticipated for 3 lakes; maybe some unexpected varve records too

Two major findings:

1. secondary proxies (bsi, tc) seem to generally work – should we keep thinking about what to do with secondary proxies?
    a. Giff – can we use midge-inferred temps to "train" BSi to get a hi-res record?
2. chironomids are sensitively showing change – two cold stenotherms disappear in last few decades at Lake CF8

3.

Timeline of results:

One midge record finished; aim is to do two additional

Varve records in progress

- plutonium and 14c appear to confirm that couplets at Big Round Lake are annual; good overlap with longer (laminated) core so can probably go back 2kyr; this lake is 1 km from a forthcoming midge record so can be compared
- Lake Longspur record shows promise of responding to summer temp (based on comparisons with met data); unfortunately lake is not varved back to 2kyr

Two big limitations:

1. chronology (210Pb results from one target lake were not promising; macrofossils are few and far between; experiments with ages on humics from bulks suggest there might be a consistent macro-humic offset but it's a big offset and risky to assume it's constant)
2. midge training set is limited on the cold end – looking for surface seds from colder training set lakes

Donna Francis
Chironomid analysis:
Greenland: Sved Lake (ongoing midge work; awaiting uppermost seds for last 600 yrs), Qipisarqo? (Holocene profiles generated; last 2kyr may be Peck's project), Tuq?

Ellesmere: C2 (surface seds have no heads), Upper Murray & Lower Murray (some midge data generated; Lower Murray has more heads than Upper; sample size in Lower is very small but there are hints of interesting changes downcore)

Yukon: Squanga (future work with Abbott)

Training set/transfer function: currently adding samples from Ellesmere; awaiting data/mud from some Greenland sites; lumping of training set data from several studies over the years means temps aren't interpolated in the same way (revise with same algorithm for all modern temps)

Challenge: very low head capsule concentrations require large sample sizes; tough for high-res work

Core a nearby small, midge-friendly lake in the future? (Retelle & Bradley: we just found seds in the fridge that are from a lake that might be suitable)

DK: Is there a timeline for improvements to the transfer function?

Dave Porinchu
Midges from Central Canadian Arctic
90-lake calibration set

S41=high-res core (50-yr sampling res); site at treeline; has 14C age model on bulk seds; midge temp record has been generated; after 1kyr midge-inferred and BSi-inferred temps diverge

V57=lower-res core with poor age control but some existing midge data

Whitecap=paleoeskimo site, sectioned at quarter-cm intervals, but v poor age control (pick out head capsules for 14c?)

Glenn has a regression to relate BSi to met temp data; created BSi-inferred temp curve for 2kyr

DK: these sites fill an important gap (interior Canada), so they do have a high priority

Bradley: What do we do when two proxies from a site disagree?

Darrell Kaufman
Alaska
Goat Lake: record of glacier advances
800-yr tree ring record from hemlock at this same site
Pollen diagram for hemlock records treeline

Cascade Lake
Great Pb and Cs but no 14C (3600-yr BP Aniakchak tephra is only other chronology)

Greyling & Hallet lakes
OM records very similar between the two lakes
BSi is looking like a reliable climate proxy
BSi record shows cooling at AD 500 (agrees with other AK records), late "MWP" (actually later than Medieval), cold LIA and recent warming

Mica Lake
Oxygen isotope efforts – Diatom d18O

Chronology:
Tests of Pu vs Cs show excellent agreement, and Pu requires less sample, less mobile than Cs; BUT the most recent test (from Hallet Lake) shows poor agreement between the two, and with Pb

From Feng Sheng via the information superhighway:
Chronologies are our biggest challenge. Our chronologies are quire good compared to what has been published, but standards of a 2ka-focused project ought to be much higher.

Hopes to publish Moose (chironomids) & Ongoki (BSi & diatom assemblage) lakes by end of year; Hudson Lake (shallow organic lake near Greyling & Hallet so good for comparison; proxies are carbonate isotopes & midges) may be ready for publication

<u>Modeling</u>

Caspar presents The Perfect (virtual) World

- actually it's not so perfect, eg

- the problem of spatial resolution and its effects on simulations for any one grid cell (high-res models are extremely computer-intensive; eg 25 days run per 24hrs vs 4 days run per 24hrs)

- pattern of el nino is well represented in simulations, but frequency does not match reality

Forcings

Volcanic and solar variations = major forcings

We have pretty good reconstructions of the forcings; no need for new

- be vs 14c and n vs s hemisphere comparisons of solar variability reconstructions are in good agreement
- volcanics: record is well-known back to ~AD 600; biggies are big eruption from unknown location AD 1258, 1450 south pacific eruption, 1815 Tambora; not so secure back to AD 0.
- Comparing major cold & warm anomalies in Osborn & Briffa (paleo data) with model reconstructions based on radiative forcing shows a good correspondence. Radiative forcing seems to be main driver.

We want to understand spatial variability – apparent spatial variability in paleorecords (eg of MWP) are completely consistent with the way climate works, ie modes of variability

Some recent experiments:

1. Effects of eruption on European temps: pattern of warming/cooling in years 0 and 1 compares very well with documented effects of eruptions, but impacts appear faster in the model than in reality
2. Solar signal?

Forcing & climate

- direct forcing (mean, variability)
- small forcing, local and regional feedbacks
- teleconnected forcing: circulation
- internal variability

Caspar is starting some data compilation/comparison:

- timing of LIA, MCA (is it spatially consistent? are there coherent spatiotemporal patterns?)
- relative amplitudes?

Experiments listed in the proposal (but this is flexible)

– NCAR-CCSM-3

Climate in warm period (MWP)

What does climate look like during this warm period?

850-1250 coupled, full forcing

Volcanic influence for LIA

Look at deterioration of climate following warmth

1250-1300 large tropical eruptions (3x)

1250-1300 large high-latitude eruptions – simulate effects of Icelandic eruptions

Solar forcing and MWP-LIA

One challenge is to see what onset of MWP looks like

900-1000 ozone feedback

Feedbacks: sea ice

What happens if we force sea ice to expand (eg we raise the sea ice albedo)? Does adding this component (ie intensifying the feedback) yield a more accurate simulation of LIA?

1225 low sea ice – 1250 high sea ice

Ideas for experiments:

- nested, high resolution component inside GCM to look at local dynamics during second year of modeling? eg test for regional-scale feedbacks?
- Compare spatial patterns and amplitudes of climate change in synthesized paleo data (EOF? Plus/minus anomalies?) over recent time with respect to model – should this be a first test before we go further back in time?
- A series (hopefully) of transient simulations over 1500 years will be run at Oak Ridge and we can use those results
- Do model-data comparison of our paleodata with existing model reconstructions since 1750
    - Caspar can generate time series of climate parameters (eg July T if that's what a proxy is reconstructing) for a spatial data point (he'd suggest 6 grid cells or so) – for comparisons to paleodata

AI: Ray will send paper on new biomarker sea ice reconstruction from Iceland

<u>The Future</u>

The ideal strategy would be: publish records → synthesis → modeling

But we probably require a phased synthesis:

- include previously published sites plus non-lake proxies?
- Intra-regional summaries
    - → is this where modeling can help us? Ideas from intra-regional that would suggest model experiments and lead to inter-regional comparisons
- Inter-regional comparisons (eg compare signs of response?)
- All project sites (high-profile paper)
- International/circum-Arctic

Proposed timeline

Mid December 2007: AGU session

Jan 1, 2008: manuscript deadline for special volume (JoPL?)

Spring 2008: volume and high-profile synthesis

- Should modeling be part of the synthesis?

We voted to use Arctic Workshop 2008 as manuscript deadline & opportunity for a special session & discussions; we should at least be prepared to undertake intraregional syntheses

Contribute data to website as it becomes available! Contact Matt Duvall to make it happen when you're ready.

Caspar: We could get started with some model/data comparisons, even with preliminary time series.

AI: we can email Caspar the coordinates of our lake, and which climate parameters we think we're reconstructing, and he can provide a time series of that climate parameter for model run from last 1750 years (model results are published so those data could go in manuscripts)

Use AGU as an opportunity for those present to get together (eg to brainstorm intraregional comparisons)? ARCUS provides facilities. We could even have a video/webcast-conference so people could participate from afar.

Eventually we'll have to grapple with this question: Realistically, what are we shooting for? Given that:

- some records are <2000 years
- some records are not annually resolved
- some records are not quantitative climate records
- some records may not be published

Are we aiming for an average record? Or do we want to preserve & examine spatial variability?

How will we deal with including unpublished data?

We voted to use the AD scale for presenting time series, with time progressing from left to right (ie we'll emulate IPCC and recent syntheses)

Ideas for our synthesis:

- follow Ray's Climate in Medieval Times strategy?
- Follow Osborn & Briffa by looking at standardized/normalized deviations from mean?
- Need to deal with differing resolution of records – downscale all records to least common denominator?
- How we decide to combine the data will depend upon the questions driving the synthesis… so these decisions may need to wait until we see what data we have to work with (eg at Arctic Workshop)
- Should we be required to provide excel files of our records in std dev units before the AW, to create a strawman compilation?

What questions are we trying to answer?

- Onset, duration, and peak of the LIA and MWP

- For MWP simulation, the question is "how would solar variability drive climate? And does the documented anomaly fit solar forcing?"
- Last 250 yr: time series for comparison with the existing model data
    - Express as deviations from mean for the 250-yr period?

On Chronologies

Clearly we feel passionately about things like treatment of outliers, dating of bulk sediments, etc

How to model chronologies?

Is linear interpolation between ages a good strategy?

Or fit a curve? (and can we use curve fits that generate error bars, vs polynomial curves?)

[raucus discussion ensues, involving dirty words like "stupidity," "flux," and "k-value"]

DK demonstrated age models created with OxCal and the Heegaard et al. (2005) spline fit routine that can be run using R; OxCal error bands show some counterintuitive tendencies (error bands are narrow when we intuitively think they should be wider – eg when two consecutive ages are hard to rectify with each other)

-OxCal is superior in the way it deals with calibrated age distributions, because it considers the age distributions; Heegaard (Heegaard et al., 2005) spline fit just takes 1-sigma range as input

DK volunteered Caleb to take our radiocarbon data as tables and run the Heegaard spline function in R for us – send data if you want to! In return you'll get a table of ages and uncertainties with depth.

Giff suggests a strategy of not using 14c ages <400 years – at least never targeting those ages

How to use the error bands on our age model? Ie how to reflect them in time series/syntheses?

- can give greater weight to records with lowest uncertainties
- report "average error is __; ranges from __ to __" with each record
- can use uncertainties to evaluate, eg, "with what certainty can we say that the LIA occurred at the same time at different sites?"

Ray: all these chronological issues reinforce the importance of finding & id'ing tephra (Caspar suggests contacting NGRIP to get Holocene tephra records; but maybe the Holocene isn't done yet?)

Group discussion topics – questions tabled temporarily

Should we have a standard way of modeling chronologies? Or each deal with geochron in our own separate ways?

We all agree it's important to quantify uncertainties. How to characterize uncertainties in geochron? In temp inferences?

Ideas for chronological markers: tephras, anthropogenic molecules (eg PCBs, vanadium, fly ash)

How to deal with water content in laminations (and differential effects of high water content on uppermost laminae)?

Do we want to look at time windows instead of continuous time series? If so, how to choose time windows?

Should BSi, etc be presented as flux vs concentration?

Lead-210 issues = showstopper?

One possible approach (from Caspar): Can we id at many sites when the drop into the LIA occurred?