http://www.eastangliaemails.com/emails.php?eid=105&filename=925829267.txt

From: mann@xxxxxxxxx.xxx
To: k.briffa@xxxxxxxxx.xxx
Subject: Re: Perspective Science piece
Date: Tue, 4 May 1999 10:47:47 -0400 (EDT)
Cc: mhughes@xxxxxxxxx.xxx, rbradley@xxxxxxxxx.xxx, t.osborn@uea

Hi Keith,

Thanks very much for the update. Sounds like everything should be
good here. I'm sorry If I might have seemed to over-react, but
it was just to make sure we avoided the scenario of last year
where we had to end up publishing a followup letter because
we and Phil hadn't had adequate communication before the piece
was published. I'll look forward to seeing the piece in print.
It sounds like you guys have a done a very good job. Indeed,
Tim and we had a very constructive dialogue about things in
your absence. Will be in touch.

best regards,

mike

p.s. I mentioned to Phil it would be nice to get at least one
spatial pattern of your summer dendro temperature estimates into
IPCC, along with a pattern or two from our multiproxy recons.
I haven't heard back to Phil, but perhaps you can make a specific
suggestion, and send me an appropriate postscript file? It's
not too late to get this to Chris Folland for inclusion in the
initial draft. Thanks in advance...

==========
From: sdecotii@xxxxxxxxx.xxx
To: christy@xxxxxxxxx.xxx, clarkea@xxxxxxxxx.xxx, climate@xxxxxxxxx.xxx,
pfrich@xxxxxxxxx.xxx, pgroisma@xxxxxxxxx.xxx, jwhurrell@xxxxxxxxx.xxx,
m.hulme@xxxxxxxxx.xxx, p.jones@xxxxxxxxx.xxx, Jouzel@xxxxxxxxx.xxx, mann@xxxxxxxxx.xxx,
j.oerlemans@xxxxxxxxx.xxx, deparker@xxxxxxxxx.xxx, tpeterso@xxxxxxxxx.xxx,
drind@xxxxxxxxx.xxx, drobins@xxxxxxxxx.xxx, j.salinger@xxxxxxxxx.xxx, walsh@xxxxxxxxx.xxx,
swwang@xxxxxxxxx.xxx
Subject: Plan of action for Chapter 2
Date: Mon, 21 Jun 1999 13:12:34 -0400

Below is the text and attached is a file in MSWord regarding a plan of
action for Chapter 2 leading up to the IPCC Meeting in Arusha, Tanzania.

June 21, 1999

Dear Lead Authors and Key Contributors,

This note is to outline a plan of action for Chapter 2 leading up to the IPCC meeting in Arusha, Tanzania to take place 1-3 September. As you know, we are now in the midst of a "friendly review" from our colleagues of the strawman draft of our chapter. We expect to receive comments from these reviews through middle or even late July. These reviews will include some from people other than our nominated reviewers, like Sir John Houghton, from whom we have just had a brief review. Please check regularly with the Tar02.meto.gov.uk email site to cover this aspect.

Accordingly we ask each of the individuals listed below to revise the draft section as suggested below, and to indicate their response to reviewer's comments. The first person listed is to take the lead, and individuals with an asterisk by his name are to prepare the material for presentation in Arusha. We would ask that a provisionally revised part of your chapter be completed by 20 August and emailed to Tom Karl or placed on the web-site so that Sylvia Decotiis can create a new version of Chapter 2 for Tom to bring to Tanzania. Tom will bring one paper copy of the provisional new "Arusha" version of chapter 2 to Tanzania, and a complete series of electronic files which can be input to PCs via 1.4MB floppy disks. It would be a considerable advantage for attendees to bring portable PCs, though we expect some IPCC PCs to be available at the Arusha International Conference Centre.

Chris Folland will be leaving for Tanzania early (24 Aug) whereas Tom Karl will still be available until 29 Aug for urgent interactions. We will decide later as to whom, and how many of us, should actually make presentations, noting that Hans Oerlemans is not likely to be present. But all attendees be prepared, and bring appropriate visual material and of course, further suggestions. We have listed assignments next to each section.

Section 2 ----- Tom Karl* and Chris Folland* Executive Summary — total revision and update
Section 2.1 ---- Chris Folland* Changes needed regarding uncertainty guidelines
Section 2.2.1 ---- Chris Folland* Okay for now
Section 2.2.2 ---- David Parker, Phil Jones, Tom Peterson, Chris Folland*
Length okay, but reduce number of figures.
Section 2.2.3 ---- John Christy* Check for accuracy
Section 2.2.4 ---- John Christy* Check for accuracy
Section 2.2.5 to 2.2.6 ---- Oelermans*, Nick Rayner, John Walsh, David Robinson, Tom Karl and Chris Folland. Glacier section needs to be updated
Section 2.2.7 ---- Oelermans, Tom Karl* Check for accuracy
Sections 2.3 through Section 2.3.5---- Mike Mann*, Phil Jones Reduce in size by about 10%

Section 2.4 through Section 2.4.5 ----Jean Jouzel* Reduce in size about 10%
Section 2.5 through 2.5.4 ---- Jim Salinger*, Pasha Groisman, Mike Hulme, Wang. Provide a better context for why this section is important, more on upper tropospheric water vapor if possible
Section 2.5.5 ---- Steve Warren, Dale Kaiser, Tom Karl* Add new analyses of cloud amount
Section 2.5.6 ----Jim Salinger*
Section 2.6 through 2.6.6 ----Jim Salinger*, George Gruza, Alynn Clarke, Wang. Reduce in size by at least 50%. Identify a rationale section at the beginning. IPCC 1995 will help here. Some material may go elsewhere. May need to consult Mike Mann or Jean Jouzel. Please send revised section to Chris Folland to finally review (even if not complete) by 16 August. Chris will feed back changes to Jim by 23 August. Jim Salinger should interact with Chris during this work too. Jim should prepare presentational material
Section 2.7 through 2.7.4 ----David Easterling, Pasha Groisman, Tom Karl* Review for accuracy
Povl Frich: please interact and be prepared to present extremes parts. Jim Salinger: you may have more material on extremes in the South Pacific. Please feed this to Tom Karl and Povl Frich.
Section 2.8 ---- Tom Karl, Chris Folland* Develop a summary, including strawman cartoon

In addition we have about twice the number of figures that will be allowed so everyone should identify figures that can be removed or combined to reduce the size. The latter can sometimes be very effective. At the present time we are about 1/3 over our word limit so everyone will have to respond to the reviewers (often requesting more), and yet being more judicious in the words we use. Please consult the 1995 IPCC Report as a guide.

Please do not hesitate to comment on these plans, preferably as soon as possible, so that holiday arrangements etc do not cause problems.

Cheers and thanks,

Chris and Tom

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At 01:07 PM 9/22/99 +0100, Folland, Chris wrote:
Dear All

A proxy diagram of temperature change is a clear favourite for the Policy Makers summary. But the current diagram with the tree ring only data somewhat contradicts the multiproxy curve and dilutes the

message rather significantly. We want the truth. Mike thinks it lies nearer his result (which seems in accord with what we know about worldwide mountain glaciers and, less clearly, suspect about solar variations). The tree ring results may still suffer from lack of multicentury time scale variance. This is probably the most important issue to resolve in Chapter 2 at present.

Chris

At 04:19 PM 9/22/99 +0100, Keith Briffa wrote:

Hi everyone

Let me say that I don't mind what you put in the policy makers summary if there is a general concensus. However some general discussion would be valuable . First , like Phil , I think that the supposed separation of the tree-ring reconstruction from the others on the grounds that it is not a true "multi-proxy" series is hard to justify. What is true is that these particular tree-ring data best represent SUMMER temperatures mostly at the northern boreal forest regions. By virtue of this , they also definately share significant variance with Northern Hemisphere land and land and marine ANNUAL temperatures - but at decadal and multidecadal timescales - simply by virtue of the fact that these series correlated with the former at these timescales. The multi proxy series (Mann et al . Jones et al) supposedly represent annual and summer seasons respectively, and both contain large proportions of tree-ring input. The latest tree-ring density curve (i.e. our data that have been processed to retain low frequency information) shows more similarity to the other two series- as do a number of other lower resolution data (Bradley et al, Peck et al ., and new Crowley series - see our recent Science piece) whether this represents 'TRUTH' however is a difficult problem. I know Mike thinks his series is the 'best' and he might be right - but he may also be too dismissive of other data and possibly over confident in his (or should I say his use of other's). After all, the early (pre-instrumental) data are much less reliable as indicators of global temperature than is apparent in modern calibrations that include them and when we don't know the precise role of particular proxies in the earlier portions of reconstruction it remains problematic to assign genuine confidence limits at multidecadal and longer timescales. I still contend that multiple regression against the recent very trendy global mean series is potentially dangerous. You could calibrate the proxies to any number of seasons , regardless of their true optimum response . Not for a moment am I saying that the tree-ring , or any other proxy data, are better than Mike's series - indeed I am saying that the various reconstructions are not independent but that they likely contribute more information about reality together than they do alone. I do believe , that it should not be taken as read that Mike's series (or Jone's et al. for that matter) is THE CORRECT ONE. I prefer a Figure that shows a multitude of reconstructions (e.g similar to that in my Science piece). Incidently, arguing that any particular series is probably better on the basis of what we now about glaciers or solar output is flaky indeed. Glacier mass balance is driven by the difference mainly in winter accumulation and summer ablation , filtered in a complex non-linear way to give variously lagged tongue advance/retreat .Simple inference on the precidence of modern day snout positions does not translate easily into absolute (or relative) temperature levels now or in the past. Similarly, I don't see that we are able to substantiate the veracity of different temperature reconstructions through reference to Solar forcing theories without making assumptions on the effectiveness of (seasonally specific) long-term insolation changes in different parts of the globe and the contribution of solar forcing to the observed 20th century warming . There is still a potential problem with non-linear responses in the very recent period of some biological proxies (or perhaps a fertilization through high CO2 or nitrate input) . I know there is pressure to present a nice tidy story as regards 'apparent unprecedented warming in a thousand years or more in the proxy data' but in reality the situation is not quite so simple. We don't have a lot of proxies that come right up to date and those

that do (at least a significant number of tree proxies) some unexpected changes in response that do not match the recent warming. I do not think it wise that this issue be ignored in the chapter.
For the record, I do believe that the proxy data do show unusually warm conditions in recent decades. I am not sure that this unusual warming is so clear in the summer responsive data. I believe that the recent warmth was probably matched about 1000 years ago. I do not believe that global mean annual temperatures have simply cooled progressively over thousands of years as Mike appears to and I contend that that there is strong evidence for major changes in climate over the Holocene (not Milankovich) that require explanation and that could represent part of the current or future background variability of our climate. I think the Venice meeting will be a good place to air these isssues.

Finally I appologise for this rather self-indulgent ramble, but I thought I may as well voice these points to you . I too would be happy to go through the recent draft of the chapter when it becomes available.

cheers to all
Keith

--
Dr. Keith Briffa, Climatic Research Unit, University of East Anglia,
Norwich, NR4 7TJ, United Kingdom
Phone: +44-1603-592090 Fax: +44-1603-507784

From: "Michael E. Mann" <mann@xxxxxxxxx.xxx
To: Keith Briffa <k.briffa@xxxxxxxxx.xxx, "Folland, Chris" <ckfolland@xxxxxxxxx.xxx, 'Phil Jones' <p.jones@xxxxxxxxx.xxx
Subject: RE: IPCC revisions
Date: Wed, 22 Sep 1999 12:35:24 -0400
Cc: tkarl@xxxxxxxxx.xxx, mann@xxxxxxxxx.xxx

Thanks for your response Keith,

For all:

Walked into this hornet's nest this morning! Keith and Phil have both raised some very good points. And I should point out that Chris, through no fault of his own, but probably through ME not conveying my thoughts very clearly to the others, definitely overstates any singular confidence I have in my own (Mann et al) series. I believe strongly that the strength in our discussion will be the fact that certain key features of past climate estimates are robust among a number of quasi-independent and truly independent estimates, each of which is not without its own limitations and potential biases. And I certainly don't want to abuse my lead authorship by advocating my own work.

I am perfectly amenable to keeping Keith's series in the plot, and can ask Ian Macadam (Chris?) to add it to the plot he has been preparing (nobody liked my own color/plotting conventions so I've given up doing this myself). The key thing is making sure the series are vertically aligned in a reasonable way. I had been using the entire 20th century, but in the case of Keith's, we need to align the first half of the 20th century w/ the corresponding mean values of the other series, due to the late 20th century decline.

So if Chris and Tom (?) are ok with this, I would be happy to add Keith's series. That having been said, it does raise a conundrum: We demonstrate (through comparining an exatropical averaging of our

nothern hemisphere patterns with Phil's more extratropical series) that the major discrepancies between Phil's and our series can be explained in terms of spatial sampling/latitudinal emphasis (seasonality seems to be secondary here, but probably explains much of the residual differences). But that explanation certainly can't rectify why Keith's series, which has similar seasonality *and* latitudinal emphasis to Phil's series, differs in large part in exactly the opposite direction that Phil's does from ours. This is the problem we all picked up on (everyone in the room at IPCC was in agreement that this was a problem and a potential distraction/detraction from the reasonably concensus viewpoint we'd like to show w/ the Jones et al and Mann et al series.

So, if we show Keith's series in this plot, we have to comment that "something else" is responsible for the discrepancies in this case. Perhaps Keith can help us out a bit by explaining the processing that went into the series and the potential factors that might lead to it being "warmer" than the Jones et al and Mann et al series?? We would need to put in a few words in this regard. Otherwise, the skeptics have an field day casting doubt on our ability to understand the factors that influence these estimates and, thus, can undermine faith in the paleoestimates. I don't think that doubt is scientifically justified, and I'd hate to be the one to have to give it fodder!

The recent Crowley and Lowery multiproxy estimate is an important additional piece of information which I have indeed incorporated into the revised draft. Tom actually estimates the same mean warming since the 17th century in his reconstruction, that we estimate in ours, so it is an added piece of information that Phil and I are probably in the ballpark (Tom has used a somewhat independent set of high and low-resolution proxy data and a very basic compositing methodology, similar to Bradley and Jones, so there is some independent new information in this estimate.

One other key result with respect to our own work is from a paper in the press in "Earth Interactions". An unofficial version is available here:

http://www.ngdc.noaa.gov/paleo/ei/ei_cover.html

THe key point we emphasize in this paper is that the low-frequency variability in our hemispheric temperature reconstruction is basically the same if we don't use any dendroclimatic indicators at all (though we certainly resolve less variance, can't get a skillful reconstruction as far back, and there are notable discrepancies at the decadal and interannual timescales). A believe I need to add a sentence to the current discussion on this point, since there is an unsubstantiated knee-jerk belief that our low-frequency variability is suppressed by the use of tree ring data.

We have shown that this is not the case: (see here:
http://www.ngdc.noaa.gov/paleo/ei/ei_datarev.html
and specifically, the plot and discussion here:
http://www.ngdc.noaa.gov/paleo/ei/ei_nodendro.html
Ironically, you'll note that there is more low-frequency variability when the tree ring data *are* used, then when only other proxy and historical/instrumental data are used!

SO I think we're in the position to say/resolve somewhat more than, frankly, than Keith does, about the temperature history of the past millennium. And the issues I've spelled out all have to be dealt with in the chapter.

One last point: We will (like it or not) have SUBSTANTIAL opportunity/requirement to revise much of this discussion after review, so we don't have to resolve everything now. Just the big picture and the important details...

I'm sure we can can up with an arrangement that is amenable to all, and I'm looking forward to hearing back from Keith, Phil, and Chris in particular about the above, so we can quickly move towards finalizing a first draft.

Looking forward to hearing back w/ comments,

mike

--

Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903

e-mail: mann@xxxxxxxxx.xxx Phone: (804) 924-7770 FAX: (804) 982-2137
http://www.evsc.virginia.edu/faculty/people/mann.html

From: Phil Jones <p.jones@xxxxxxxxx.xxx>
To: "Michael E. Mann" <mann@xxxxxxxxx.xxx>, "Folland, Chris" <ckfolland@xxxxxxxxx.xxx>, Keith Briffa <k.briffa@xxxxxxxxx.xxx>, "Folland, Chris" <ckfolland@xxxxxxxxx.xxx>
Subject: RE: IPCC revisions
Date: Thu, 23 Sep 1999 17:20:56 +0100
Cc: tkarl@xxxxxxxxx.xxx

Mike,
Here are my thoughts on the text you sent. Keith will be sending some as well hopefully later today. One important aspect Keith will address is whether you're using the latest Briffa et al curve. We know you're not but the one with the greater low frequency and therefore much better chance of looking much better with the other two series, isn't yet published. We know it looks better in plots we have here.

Specifics :

p1 line 10 - say mid-19th century rather than the 20th century

lines 18-20 - seems a bit too much here with three refs on laminated sediments.

line 46 Add Briffa et al (1998b) to Cook(1995).

p2 line 59 - I would suggest changing 'a particularly' to 'the most' .

line 64 - I would add a reference here to the paper by Crowley and Kim (1999) in GRL (July) where this aspect is also discussed.

p3 line 101 - I would add Argentina as well as Chile adding a ref to Villalba (1990) in QR.

line 108 change 'key' to 'vital'

line 119 'have providing' to 'provide' . There are several instances where the text doesn't read that well. I suspect as there are several iterations to go it is not that important yet !

The coral section is just about the right size now and is justly devoid of references !

p4 line 151 I would add a reference here to Morgan and van Ommen (1997) 'Seasonality in late-Holocene climate from ice core records', The Holocene 7, 351-4. This is the Law Dome core which is the best available with regards to dating in either hemisphere. It should be there.

As with the coral section the ice core section expresses some cautionary notes with regard to dating etc which I think are justified.I suspect teh contrast with the tree-ring section will draw some criticism. Just a warning !

As none of the multiproxy reconstuctions use any sediment information this section seems overlarge and could be reduced.

p189 century-scale add in the 'y'

p5 The borehole section is also a bit overlong. I don't know whether the map really adds something. Not that vehement on this.

With respect to comapring high and low frequency aspects the diagram comparing CET with the UK boreholes is now out. I've sent a copy to Chris. It is in :

Jones PD, 1999 : Classics in physical geography revisited - Manley's
CET series. Progress in Physical Geography 23, 425-428.

line 245 the 'is' is not needed.

p6 I still think that a reference to Raper et al (1996) would be good here. This models a glacier in northern Sweden using the northern Fennoscandian temperature reconstructions since AD 500. Again it shows how a low frequency estimate (the glacial snout position) can be compared
with a high-frquency temperature reconstruction from trees.

Raper, SCB, Briffa KR and Wigley TML, 1996: Glacial change in northern Sweden from AD 500: a simple geometric model of Storglaciaren. Journal of Glaciology 42, 341-351.

line 268 IPCC(1996) earlier - is it 95 or 96

p 7 line 295 I would like to add my paper in Reviews of Geophysics in 1999 as that also says that 1998 was likely to be the warmest year of the millennium.

line 334 I would like to see Bradley (1999). I must get a copy from Ray in Venice.

p7-9 All need a careful read through for English and the arguments.

At the bottom of p8 I think you make too much of the differences in the ranking of the centuries. The boreholes would agree with my series with the 17th being colder than the 19th, although they may not be able to resolve the timescales then.

Is the Crowley and Lowery (1999) the paper Tom's submitted to Ambio ?

I've not commented much on this final section as again I suspect there are many things you will have to justify in the next two sets of reviews. On the whole I think most is OK and I support the final paragraph. I don't believe the astronomical argument as an explaination over the last 1000 years but we can differ on that.

I know I would have written this final section 2.3.3 somehat differently with different emphases and slants but the basic final conclusion would have been the same.

Cheers
Phil

From: "Michael E. Mann" <mann@xxxxxxxxx.xxx>
To: Phil Jones <p.jones@xxxxxxxxx.xxx>, "Folland, Chris" <ckfolland@xxxxxxxxx.xxx>, Keith Briffa <k.briffa@xxxxxxxxx.xxx>, "Folland, Chris" <ckfolland@xxxxxxxxx.xxx>
Subject: RE: IPCC revisions
Date: Thu, 23 Sep 1999 13:34:14 -0400
Cc: tkarl@xxxxxxxxx.xxx, mann@xxxxxxxxx.xxx

Thanks for your comments Phil,

They look quite reasonable, and I will seek to incorporate them. I'll need Keith's comments by tomorrow morning (my time) at the very latest if I am to have time to assess them and incorporate them.

Some important specifics:

1) I am definitely using the version of the Briffa et al series you sent in which Keith had restandardized to retain *more* low-frequency variability relative to the one shown by Briffa et al (1998). So already, the reconstruction I'm using is one-step removed from the published series (as far as I know!) and that makes our use of even this series a bit tenuous in my mind, but I'm happy to do it and let the reviewers tell us if they see any problem. If I understand you correctly, there is yet a new version of this series that is two steps removed from Briffa et al (1998)?

Frankly, at this stage I think we have to go w/ what we have (please see Ian Macadam's plot when it is available--I think the story it tells isn't all that bad, actually) for the time being. Things as you say will change following review anyways.

2) One other thingp--I'm actually averse to shortening the section on sediments. Even if they haven't contributed to some of the multiproxy studies (they certainly *did* contribute to Overpeck et al!) there are some important results there irrespective of the role of the proxies in multiproxy studies. Lets, again, wait for reviews before shortening this...

3) We could eliminate the map of the boreholes, although I actually think it is essential to see what the contributing spatial sampling (and, accordingly, the potential bias of that sampling in determining "global mean temperature") actually is. So I vote for keeping it for the time being. Again, it's an extremity that we can afford to lose if necessary in the end..

4) One important note on references: We don't have time at this late stage to dig up incomplete citations, so you'll need to give me full citations for any suggested added references (e.g. the Villalba paper). FYI, the Crowley and Lowery paper is Tom's Ambio paper. He observes a mean warming of about 0.5 C since the 17th century giving us yet another datapoint in the scatter of estimates...

5) I agree, the ranking of centuries is more specific than it needs to be. I don't know what I was thinking. You sure that didn't come from the text you originally contributed?? In any case, we can eliminate much of it in my opinion too...

On the whole, I have never been under the assumption that you and I would have independently assessed the evidence quite the same way. I would hope we would have come up w/ the same key points, and so your comments in that regard are reassuring. I feel confident in my ability to defend the science that is presented here, so let the reviews fall where they may. I'm sure we will be forced to admit some changes, as well as "minority viewpoints" and alternative interpretations along the way. That's what will make this all interesting...

mike

From: Keith Briffa <k.briffa@xxxxxxxxx.xxx>
To: "Michael E. Mann" <mann@xxxxxxxxx.xxx>, "Folland, Chris" <ckfolland@xxxxxxxxx.xxx>, 'Phil Jones' <p.jones@xxxxxxxxx.xxx>
Subject: RE: IPCC revisions
Date: Thu Sep 23 18:29:05 1999
Cc: tkarl@xxxxxxxxx.xxx, mann@xxxxxxxxx.xxx

Dear Mike (and all)

Some remarks in response to your recent message

I believe strongly that the strength in our discussion
>will be the fact that certain key features of past climate estimates are
>robust among a number of quasi-independent and truly independent estimates,
>each
>of which is not without its own limitations and potential biases

Mike , I agree very much with the above sentiment. My concern was motivated by the possibility of expressing an impression of more concensus than might actually exist . I suppose the earlier talk implying that we should not 'muddy the waters' by including contradictory evidence worried me . IPCC is supposed to represent concensus but also areas of uncertainty in the evidence. Of course where there are good reasons for the differences in series (such as different seasonal responses or geographic bias) it is equally important not to overstress the discrepancies or suggest contradiction where it does not exist.

And I
>certainly don't want to abuse my lead authorship by advocating my own work.
>

I sincerely hope this was not implied in anything I wrote - It was not intended

>I am perfectly amenable to keeping Keith's series in the plot, and can ask
>Ian Macadam (Chris?) to add it to the plot he has been preparing (nobody
>liked my own color/plotting conventions so I've given up doing this myself).
>The key thing is making sure the series are vertically aligned in a reasonable
>way. I had been using the entire 20th century, but in the case of Keith's,
>we need to align the first half of the 20th century w/ the corresponding mean
>values of the other series, due to the late 20th century decline.
>

Again I agree. Also , I am not sure which version of the curve you are now refering to. The original draft did show our higher frequency curve i.e. the version with background changes effectively filtered out (intended to emphasise the extreme interannual density excursions and their coincidence with volcanic eruptions) . The relevant one here is a smoothed version in which low-frequency changes are

preserved. I can supply this and it will be in press by the time of the next reworking of the text.

Your above point on correct scaling is relevant also to Phil's curve which was not originally calibrated (in a formal regression sense) with the summer temperature data - it was just given the same mean and standard deviation over a specific period. Hence the issue of equivelent scaling of all series is vital if we are to discuss specific period temperature anomalies in different series or compare temperature trends in absolute degrees.

>So if Chris and Tom (?) are ok with this, I would be happy to add Keith's
>series. That having been said, it does raise a conundrum: We demonstrate
>(through comparining an exatropical averaging of our nothern hemisphere
>patterns with Phil's more extratropical series) that the major
>discrepancies between Phil's and our series can be explained in terms of
>spatial sampling/latitudinal emphasis (seasonality seems to be secondary
>here, but probably explains much of the residual differences). But that
>explanation certainly can't rectify why Keith's series, which has similar
>seasonality
>*and* latitudinal emphasis to Phil's series, differs in large part in
>exactly the opposite direction that Phil's does from ours. This is the
>problem we
>all picked up on (everyone in the room at IPCC was in agreement that this
>was a problem and a potential distraction/detraction from the reasonably
>concensus viewpoint we'd like to show w/ the Jones et al and Mann et al
>series.
>

I am not sure this is true if the relevant series of ours is used. We need to reexamine the curves and perhaps look at the different regional and seasonal data in the instrumental record and over common regions in the different reconstructed series. We would be happy to work with you on this. Also remember that our (density)series does not claim hemispheric or annual coverage.

>So, if we show Keith's series in this plot, we have to comment that
>"something else" is responsible for the discrepancies in this case. Perhaps
>Keith can
>help us out a bit by explaining the processing that went into the series
>and the potential factors that might lead to it being "warmer" than the Jones
>et al and Mann et al series?? We would need to put in a few words in this
>regard. Otherwise, the skeptics have an field day casting
>doubt on our ability to understand the factors that influence these estimates
>and, thus, can undermine faith in the paleoestimates.

The best approach here is for us to circulate a paper addressing all the above points. I'll do this as soon as possible.

I don't think that
>doubt is scientifically justified, and I'd hate to be the one to have
>to give it fodder!
>
>
>The recent Crowley and Lowery multiproxy estimate is an important

>additional piece of information which I have indeed incorporated into the
>revised draft.
>Tom actually estimates the same mean warming since the 17th century in his
>reconstruction, that we estimate in ours, so it is an added piece of
>information that Phil and I are probably in the ballpark (Tom has used
>a somewhat independent set of high and low-resolution proxy data and a very
>basic compositing methodology, similar to Bradley and Jones, so there is
>some independent new information in this estimate.
>

fair enough - but I repeat that the magnitude of the observed warming in the 20th century is different in summer and annual data

>One other key result with respect to our own work is from a paper in the
>press in "Earth Interactions". An unofficial version is available here:
>
>http://www.ngdc.noaa.gov/paleo/ei/ei_cover.html
>
>THe key point we emphasize in this paper is that the low-frequency
>variability in our hemispheric temperature reconstruction is basically the
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>certainly resolve less variance, can't get a skillful reconstruction as far
>back, and there are notable discrepancies at the decadal and interannual
>timescales). A believe I need to add a sentence to the current discussion
>on this point,
>since there is an unsubstantiated knee-jerk belief that our low-frequency
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>
>We have shown that this is not the case: (see here:
>http://www.ngdc.noaa.gov/paleo/ei/ei_datarev.html
>and specifically, the plot and discussion here:
>http://www.ngdc.noaa.gov/paleo/ei/ei_nodendro.html
>Ironically, you'll note that there is more low-frequency variability when
>the tree ring data *are* used, then when only other proxy and
>historical/instrumental data are used!
>

This is certainly relevant and sounds really interesting. I need to look at this in detail. The effect of the including tree-ring data or not, is moderated by the importance of the particular series in the various reconstructions (relative coefficient magnitudes). There is certainly some prospect of affecting (reducing) the apparent magnitude of the 20th century warming by loading on high-pass filtered chronologies , but equally a danger of exagerating it if the series used or emphasised in th calibration have been fertilized by CO2 or something else. As you know we (Tim, Phil and I) would love to collaborate with you on exploring this issue (and the role of instrumental predictors) in the various approaches.

The key here is knowing much more about the role of specific predictors through time and their associated strengths and weaknesses.

>SO I think we're in the position to say/resolve somewhat more than, frankly,
>than Keith does, about the temperature history of the past millennium.
>And the issues I've spelled out all have to be dealt with in the chapter.
>

I certainly do not disagree with you - the scale of your input data undoubtedly must contain more information than our set . I have never implied anything to the contrary. I do not believe that our data are likely to tell us more than summer variability at northern latitudes . The discussion is only about how close our and your data likely represent what they are calibrated against , back in time. Let's not imagine a disagreement where there is none.

>One last point: We will (like it or not) have SUBSTANTIAL
>opportunity/requirement to revise much of this discussion after review, so
>we don't have to resolve everything now. Just the big picture and the
>important details...
>
>I'm sure we can can up with an arrangement that is amenable to all, and I'm
>looking forward to hearing back from Keith, Phil, and Chris in particular
>about the above, so we can quickly move towards finalizing a first draft.
>
>

Yes indeed. The reviewing will lead to much comment and likely disagreement by the masses. This is the way of these things. It is always a thankless task undertaking these drafting jobs and I think you are doing a good job. Tommorrow I'll send some very minor comments on typos and the like if you want them - or have you picked many of them up? Anyway , keep up the good work .

best wishes
Keith

From: "Michael E. Mann" <mann@xxxxxxxxx.xxx>
To: Keith Briffa <k.briffa@xxxxxxxxx.xxx>, "Folland, Chris" <ckfolland@xxxxxxxxx.xxx>, 'Phil Jones' <p.jones@xxxxxxxxx.xxx>
Subject: RE: IPCC revisions
Date: Thu, 23 Sep 1999 13:47:22 -0400
Cc: tkarl@xxxxxxxxx.xxx, mann@xxxxxxxxx.xxx

Thanks alot Keith,

Your comments and suggestions sound good on all counts.

Clearly there is one overiding thing to make sure of here: that we have the right version of your series. I *think* that we do, and you might have been looking at an old version of the omparison Figure??

Please check out the data here ASAP:

ftp://eclogite.geo.umass.edu/pub/mann/IPCC/MILLENNIUM/

This directory has all the series, aligned as I described to have a 1961-90 base climatology (or in the case of your series, a pseudo 1961-90 base climatology achieved by actually matching the mean of your series and the instrumental record over the interval 1931-60 interval). These are the data that Ian Macadam is hopefully presently plotting up, and I don't think the discrepancies between the different series are as bad as we percieved earlier (other than the late 19th century where you are somewhat on the warm side relative to the rest). Please confirm
ASAP that we have the right version of the series (note, these have all been 40 year lowpassed)...

One other thing, I think you misinterpreted my statement:

>
>SO I think we're in the position to say/resolve somewhat more than, frankly,
>than Keith does, about the temperature history of the past millennium.
>And the issues I've spelled out all have to be dealt with in the chapter.
>

I wasn't talking about the comparison of our two series! I was talking about our two different opinions on how confident we are about our ability, as a community, to assess the actual climate changes over this timeframe. And perhaps we're closer here than I assumed anyways. Sorry about the misunderstanding. With your interpretation, my comment must I have sounded
really obnoxious!

==========
From: Tim Osborn <t.osborn@xxxxxxxxx.xxx>
To: mann@xxxxxxxxx.xxx,imacadam@xxxxxxxxx.xxx
Subject: Briffa et al. series for IPCC figure
Date: Tue, 05 Oct 1999 16:18:29 +0100
Cc: k.briffa@uea,p.jones@uea

Dear Mike and Ian

Keith has asked me to send you a timeseries for the IPCC multi-proxy reconstruction figure, to replace the one you currently have. The data are attached to this e-mail. They go from 1402 to 1995, although we usually stop the series in 1960 because of the recent non-temperature signal that is superimposed on the tree-ring data that we use. I haven't put a 40-yr smoothing through them - I thought it best if you were to do this to ensure the same filter was used for all curves.

The raw data are the same as used in Briffa et al. (1998), the Nature paper that I think you have the reference for already. They are analysed in a different way, to retain the low-frequency variations. In this sense, it is one-step removed from Briffa et al. (1998). It is not two-steps removed from Briffa et al. (1998), since the new series is simply a *replacement* for the one that you have been using, rather than being one-step further.

A new manuscript is in preparation describing this alternative analysis method, the calibration of the resulting series, and their comparison with other reconstructions. We are consdering submitting this manuscript to J. Geophys. Res. when it is ready, but for now it is best cited as: Briffa KR, Osborn TJ, Schweingruber FH, Harris IC and Jones PD (1999) Extracting low-frequency temperature variations from a northern tree-ring density network. In preparation.

Keith will be sending you a copy of the manuscript when it is nearer to completion.

I have also attached a PS file showing the original Briffa et al. (1998) curve, with annotation of cold years associated with known volcanic eruptions. Overlain on this, you will see a green curve. This is the new series with a 40-yr filter through it. This is just so that you can see what it should look like (***ignore the temperature scale on this figure***, since the baseline is non-standard).

With regard to the baseline, the data I've sent are calibrated over the period 1881-1960 against the instrumental Apr-Sep tempratures averaged over all land grid boxes with observed data that are north of 20N. As such, the mean of our reconstruction over 1881-1960 matches the mean of the observed target series over the same period. Since the observed series consists of degrees C anomalies wrt to 1961-90, we say that the reconstructed series also represents degrees C anomalies wrt to 1961-90. One could, of course, shift the mean of our reconstruction so that it matched the observed series over a different period - say 1931-60 - but I don't see that this improves things. Indeed, if the non-temperature signal that causes the decline in tree-ring density begins before 1960, then a short 1931-60 period might yield a more biased result than using a longer 1881-1960 period.

If you have any queries regarding this replacement data, then please e-mail me and/or Keith.

Best regards

Tim

Calibrated against observed Apr-Sep temperature over 1881-1960 averaged over all land grid boxes north of 20N

Year Reconstructed temperature anomaly (degrees C wrt 1961-90)
1402 -0.283
1403 -0.334
1404 -0.286
1405 -0.350
1406 -0.152
1407 -0.124
1408 -0.220
1409 -0.175
1410 -0.100
1411 -0.129
1412 -0.226
1413 -0.115
1414 -0.386
1415 -0.319
1416 -0.277
1417 -0.136
1418 -0.172
1419 -0.294
1420 -0.280
1421 -0.335
1422 -0.406
1423 -0.312
1424 -0.207

1425 -0.136
1426 -0.354
1427 -0.222
1428 -0.305
1429 -0.322
1430 -0.282
1431 -0.143
1432 -0.212
1433 -0.234
1434 -0.076
1435 -0.309
1436 -0.411
1437 -0.122
1438 -0.272
1439 -0.159
1440 -0.330
1441 -0.160
1442 -0.105
1443 -0.080
1444 -0.308
1445 -0.138
1446 -0.317
1447 -0.270
1448 -0.301
1449 -0.357
1450 -0.137
1451 -0.183
1452 -0.207
1453 -0.485
1454 -0.265
1455 -0.358
1456 -0.241
1457 -0.199
1458 -0.366
1459 -0.397
1460 -0.252
1461 -0.230
1462 -0.252
1463 -0.209
1464 -0.174
1465 -0.174
1466 -0.280
1467 -0.256
1468 -0.256
1469 -0.222
1470 -0.237
1471 -0.094
1472 -0.122
1473 -0.056
1474 -0.320
1475 -0.376

1476 -0.133
1477 -0.075
1478 0.037
1479 -0.161
1480 -0.379
1481 -0.513
1482 -0.286
1483 -0.354
1484 -0.327
1485 -0.208
1486 -0.125
1487 -0.380
1488 -0.193
1489 -0.245
1490 -0.466
1491 -0.244
1492 -0.146
1493 -0.278
1494 -0.394
1495 -0.526
1496 -0.275
1497 -0.264
1498 -0.233
1499 -0.169
1500 -0.128
1501 -0.415
1502 -0.306
1503 0.011
1504 -0.013
1505 -0.378
1506 -0.226
1507 -0.428
1508 -0.192
1509 -0.312
1510 -0.157
1511 -0.162
1512 -0.188
1513 -0.135
1514 -0.418
1515 -0.258
1516 -0.381
1517 -0.134
1518 -0.180
1519 -0.166
1520 -0.035
1521 -0.384
1522 -0.302
1523 -0.541
1524 -0.371
1525 -0.183
1526 -0.289

1527 -0.224
1528 -0.247
1529 -0.432
1530 -0.291
1531 -0.467
1532 -0.343
1533 -0.586
1534 -0.183
1535 -0.417
1536 -0.350
1537 -0.257
1538 -0.451
1539 -0.398
1540 -0.497
1541 -0.406
1542 -0.584
1543 -0.448
1544 -0.317
1545 -0.312
1546 -0.289
1547 -0.114
1548 -0.459
1549 -0.335
1550 -0.009
1551 -0.074
1552 -0.047
1553 -0.207
1554 -0.285
1555 -0.116
1556 -0.141
1557 -0.419
1558 -0.174
1559 -0.465
1560 -0.287
1561 -0.169
1562 -0.231
1563 -0.270
1564 -0.347
1565 -0.116
1566 -0.202
1567 -0.278
1568 -0.445
1569 -0.488
1570 -0.465
1571 -0.434
1572 -0.674
1573 -0.324
1574 -0.493
1575 -0.273
1576 -0.623
1577 -0.483

1578 -0.521
1579 -0.551
1580 -0.473
1581 -0.436
1582 -0.382
1583 -0.345
1584 -0.280
1585 -0.565
1586 -0.409
1587 -0.580
1588 -0.530
1589 -0.534
1590 -0.354
1591 -0.377
1592 -0.407
1593 -0.337
1594 -0.591
1595 -0.459
1596 -0.436
1597 -0.475
1598 -0.152
1599 -0.134
1600 -0.381
1601 -1.169
1602 -0.403
1603 -0.414
1604 -0.472
1605 -0.393
1606 -0.564
1607 -0.529
1608 -0.822
1609 -0.789
1610 -0.617
1611 -0.681
1612 -0.670
1613 -0.364
1614 -0.733
1615 -0.428
1616 -0.698
1617 -0.479
1618 -0.485
1619 -0.524
1620 -0.706
1621 -0.671
1622 -0.714
1623 -0.662
1624 -0.387
1625 -0.566
1626 -0.671
1627 -0.665
1628 -0.759

1629 -0.654
1630 -0.379
1631 -0.466
1632 -0.330
1633 -0.377
1634 -0.521
1635 -0.222
1636 -0.265
1637 -0.252
1638 -0.396
1639 -0.382
1640 -0.400
1641 -1.152
1642 -1.067
1643 -1.092
1644 -0.649
1645 -0.588
1646 -0.632
1647 -0.554
1648 -0.368
1649 -0.572
1650 -0.215
1651 -0.317
1652 -0.529
1653 -0.268
1654 -0.343
1655 -0.400
1656 -0.372
1657 -0.332
1658 -0.359
1659 -0.182
1660 -0.260
1661 -0.258
1662 -0.433
1663 -0.433
1664 -0.353
1665 -0.440
1666 -0.837
1667 -0.857
1668 -0.816
1669 -0.779
1670 -0.871
1671 -0.463
1672 -0.434
1673 -0.631
1674 -0.663
1675 -0.870
1676 -0.523
1677 -0.670
1678 -0.794
1679 -0.768

1680 -0.701
1681 -0.380
1682 -0.518
1683 -0.364
1684 -0.369
1685 -0.688
1686 -0.178
1687 -0.481
1688 -0.351
1689 -0.229
1690 -0.254
1691 -0.221
1692 -0.545
1693 -0.263
1694 -0.316
1695 -0.955
1696 -0.816
1697 -0.687
1698 -1.054
1699 -1.005
1700 -0.630
1701 -0.818
1702 -0.510
1703 -0.377
1704 -0.420
1705 -0.527
1706 -0.328
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1708 -0.465
1709 -0.493
1710 -0.288
1711 -0.344
1712 -0.345
1713 -0.242
1714 -0.390
1715 -0.305
1716 -0.390
1717 -0.309
1718 -0.270
1719 -0.194
1720 -0.110
1721 -0.427
1722 0.005
1723 -0.193
1724 -0.249
1725 -0.497
1726 -0.381
1727 -0.241
1728 -0.133
1729 -0.261
1730 -0.633

1731 -0.723
1732 -0.426
1733 -0.371
1734 -0.104
1735 -0.373
1736 -0.330
1737 -0.206
1738 -0.557
1739 -0.291
1740 -0.734
1741 -0.594
1742 -0.808
1743 -0.378
1744 -0.372
1745 -0.418
1746 -0.501
1747 -0.150
1748 -0.389
1749 -0.328
1750 -0.168
1751 -0.343
1752 -0.227
1753 -0.218
1754 -0.377
1755 -0.328
1756 -0.221
1757 -0.259
1758 -0.431
1759 -0.340
1760 -0.335
1761 -0.261
1762 -0.466
1763 -0.291
1764 -0.473
1765 -0.378
1766 -0.212
1767 -0.429
1768 -0.544
1769 -0.343
1770 -0.341
1771 -0.265
1772 -0.547
1773 -0.421
1774 -0.048
1775 -0.289
1776 -0.186
1777 -0.288
1778 -0.178
1779 -0.550
1780 -0.339
1781 -0.251

1782 -0.164
1783 -0.757
1784 -0.142
1785 -0.141
1786 -0.179
1787 -0.432
1788 -0.207
1789 -0.235
1790 -0.612
1791 -0.163
1792 -0.086
1793 -0.023
1794 -0.030
1795 -0.243
1796 -0.028
1797 -0.565
1798 -0.049
1799 -0.228
1800 -0.287
1801 -0.413
1802 -0.117
1803 0.020
1804 0.036
1805 -0.094
1806 -0.251
1807 -0.089
1808 -0.241
1809 -0.460
1810 -0.582
1811 -0.353
1812 -0.459
1813 -0.545
1814 -0.458
1815 -0.588
1816 -0.855
1817 -0.861
1818 -0.629
1819 -0.680
1820 -0.289
1821 -0.351
1822 -0.159
1823 -0.246
1824 -0.276
1825 -0.263
1826 -0.140
1827 -0.293
1828 -0.033
1829 -0.087
1830 -0.173
1831 -0.045
1832 -0.621

1833 -0.660
1834 -0.141
1835 -0.647
1836 -0.775
1837 -0.771
1838 -0.359
1839 -0.267
1840 -0.144
1841 -0.077
1842 -0.337
1843 -0.435
1844 -0.101
1845 -0.412
1846 0.106
1847 -0.079
1848 -0.346
1849 -0.393
1850 -0.261
1851 -0.165
1852 -0.100
1853 -0.174
1854 -0.138
1855 -0.418
1856 -0.250
1857 -0.538
1858 -0.126
1859 -0.195
1860 -0.231
1861 -0.029
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1867 -0.413
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1872 -0.163
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1881 -0.184
1882 -0.200
1883 -0.183

1884 -0.717
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1886 -0.485
1887 -0.281
1888 -0.261
1889 -0.153
1890 -0.341
1891 -0.313
1892 -0.138
1893 -0.301
1894 -0.134
1895 -0.128
1896 -0.241
1897 -0.016
1898 0.065
1899 -0.574
1900 -0.218
1901 -0.049
1902 -0.287
1903 -0.142
1904 -0.205
1905 -0.308
1906 -0.034
1907 -0.412
1908 -0.048
1909 -0.214
1910 -0.147
1911 -0.194
1912 -0.631
1913 -0.161
1914 -0.294
1915 -0.074
1916 -0.277
1917 -0.297
1918 -0.460
1919 -0.013
1920 -0.272
1921 -0.114
1922 -0.036
1923 -0.305
1924 -0.141
1925 -0.258
1926 -0.115
1927 -0.198
1928 -0.018
1929 -0.161
1930 0.086
1931 0.104
1932 0.081
1933 -0.057
1934 0.007

1935 -0.037
1936 -0.019
1937 0.060
1938 0.163
1939 -0.075
1940 0.113
1941 -0.200
1942 0.128
1943 0.053
1944 -0.080
1945 0.059
1946 -0.016
1947 -0.188
1948 -0.038
1949 -0.107
1950 -0.269
1951 -0.100
1952 -0.118
1953 0.161
1954 -0.235
1955 -0.127
1956 -0.308
1957 -0.194
1958 -0.308
1959 -0.224
1960 0.076
1961 -0.104
1962 -0.289
1963 -0.173
1964 -0.479
1965 -0.474
1966 -0.171
1967 -0.200
1968 -0.599
1969 -0.355
1970 -0.353
1971 -0.328
1972 -0.563
1973 -0.262
1974 -0.336
1975 -0.507
1976 -0.558
1977 -0.363
1978 -0.698
1979 -0.289
1980 -0.612
1981 -0.195
1982 -0.522
1983 -0.234
1984 -0.335
1985 -0.423

1986 -0.430
1987 -0.424
1988 -0.161
1989 -0.286
1990 -0.275
1991 -0.169
1992 -0.175
1993 -0.341
1994 -0.320

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From: "Michael E. Mann" <mann@xxxxxxxxx.xxx>
To: Tim Osborn <t.osborn@xxxxxxxxx.xxx>, imacadam@xxxxxxxxx.xxx
Subject: Re: Briffa et al. series for IPCC figure
Date: Tue, 05 Oct 1999 12:31:56 -0400
Cc: k.briffa@uea, p.jones@uea, ckfolland@xxxxxxxxx.xxx, tkarl@xxxxxxxxx.xxx

Dear Tim,

Thanks for the information. I don't want to speak for Tom Karl, but I think it may be a bit too late (past the Oct 1 deadline) to make further revisions in the draft 1.0. It would be a bit of an imposition on Tom at this point given what he's been through in finalizing the draft. However, I see no reason that we can't make that revision when the paper comes back from expert review in a couple months. We'll have the further advantage that the supporting manuscript you describe should be available at that point (a requirement in the IPCC peer-review process). I think we'll all be looking forward to updating the plot w/ the latest series you describe...

As for decisions about the most appropriate baseline period to use for the series, that is as you point out an important issue and one we have to consider with some circumspection, especially if a "modern" calibration (e.g., 1931-1960) to the instrumental record gives a substantially
different alignment from the more 19th century-oriented calibration you describe. The tradeoff
of course is that the instrumental series itself is considerably less certain prior to the 20th century while, as you point out, the non-climatic influence on tree growth may be setting in by the mid 20th century. Something I think we can iron out satisfactorily at the next juncture.

I hope the above sounds ok to you guys. Let me know.

Thanks,
mike

From: Tim Osborn <t.osborn@xxxxxxxxx.xxx>
To: "Michael E. Mann" <mann@xxxxxxxxx.xxx>
Subject: Re: newest reconstruction
Date: Mon Feb 28 13:50:17 2000
Cc: k.briffa@uea, t.osborn@uea

At 11:56 25/02/00 -0500, you wrote:
>I need your newest northern hemisphere density-based tree-ring reconstruction
>and appropriate reference for updating IPCC. Please send in ASCII format as

>soon as possible so we can incorporate. I hope all is well. Thanks,

Hi Mike

Keith asked me to get back to you on this. The reconstruction is the same as the one I sent on the 5th October 1999, but I'm sending it again in case that e-mail isn't handy. The reconstruction has now been published, in the following paper:

Briffa K.R. (2000) Annual climate variability in the Holocene: interpreting the message of ancient trees. Quaternary Science Reviews 19, 87-105.

This paper does not, however, give full details about how the reconstruction was obtained. The details are not yet published, but will soon be submitted:

Briffa KR, Osborn TJ, Schweingruber FH, Harris IC, Jones PD, Shiyatov SG and Vaganov EA (2000) Low-frequency temperature variations from a northern tree-ring density network. In preparation (to be submitted to Journal of Geophysical Research).

Details about the file I'm sending you (repeated from 5th Oct 99):

The data are attached to this e-mail. They go from 1402 to 1994, although we usually stop the series in 1960 because of the recent non-temperature signal that is superimposed on the tree-ring data that we use. I haven't put a 40-yr smoothing through them - I thought it best if you were to do this to ensure the same filter was used for all curves. The data I've sent are calibrated over the period 1881-1960 against the instrumental Apr-Sep tempratures averaged over all land grid boxes (that have observed data) that are north of 20N. As such, the mean of our reconstruction over 1881-1960 matches the mean of the observed target series over the same period. Since the observed series consists of degrees C anomalies wrt to 1961-90, we say that the reconstructed series also represents degrees C anomalies wrt to 1961-90.

(I've already truncated the series at 1960 because of the problems with the recent period.)

Best regards

Tim