CRU CORRESPONDENCE
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From: "Janice Darch" <J.Darch@uea.ac.uk>
To: <env.faculty@uea>, <env.researchstaff@uea>
Subject: Towards a Sustainable Energy Economy deadline
Date: Wed, 7 Jan 2004 10:35:14 -0000

Dear All,
Is any one involved in proposals for this initiative?

Please let me know.
Janice

First call for research proposals
A call for expressions of interest for participation in Consortia, Research Groups, Networks, Collaborative Proposals and Capacity Building
Closing date: 5pm, Monday 19 January 2004

Intending applicants should note that all those receiving funding from this programme will be expected to collaborate with the UK Energy Research Centre following its establishment on 1st April 2004.

Introduction
The Towards a Sustainable Energy Economy programme (TSEC) is aimed at enabling the UK to access a secure, safe, diverse and reliable energy supply at competitive prices, while meeting the challenge of global warming. The Engineering and Physical Science Research Council (EPSRC), Economic and Social Research Council (ESRC) and Natural Environment Research Council (NERC) jointly have funding of £28 million for the programme, which is co-ordinated by NERC on behalf of the three Research Councils, with participation from the Biotechnology and Biological Sciences Research Council (BBSRC) and Council for the Central Laboratory of the Research Councils (CCLRC). The Councils are advised on the use of the programme's funds by the TSEC Scientific Advisory Committee.

TSEC is an interdisciplinary research programme that will adopt whole systems integrated approaches. The Research Councils' working definition of 'a whole systems approach' is: "A whole systems integrated methodology demanding a truly interdisciplinary approach that facilitates the joint working of engineering, technological, natural, environmental, social and economic scientists to tackle fundamental issues (such as sustainable energy)." A whole systems approach should ensure that new work carried out complements current and planned activities of the individual Research Councils in the area concerned and will take into account known understanding for the issues addressed.

The TSEC programme will provide a focus for, but will not be the only source of, energy research in the UK. As such, the TSEC programme will aim to make an impact on UK energy research by promoting this whole systems approach. Proposers wishing to carry out research under TSEC should familiarise themselves with the role of TSEC in the energy research landscape, as described in Annex 1.

What research will TSEC support?
Up to £12 million of the programme's funding will be used to establish the UK Energy Research Centre (UKERC) by 1st April 2004, for which the Councils have already invited full proposals. The Centre's two major activities will

be its own research programme and the co-ordination of a National Energy Research Network.

The remainder of the TSEC programme's funds (at least £16 million) will be used to support research that will operate independently of, but complementary to, the research done by UKERC. Calls for proposals will be broadly under the following themes:

- carbon management
- nuclear power
- renewable energy
- managing new uncertainties.

In keeping with the whole systems approach of the programme, applications are invited from all disciplines that have a research interest in any of the themes (eg the environmental, social, economic and technological aspects of nuclear power).

What areas are covered in this call?
This first call covers all aspects of the TSEC programme but the Research Councils wish to focus initially on two of the themes: nuclear power and managing new uncertainties. It is anticipated that a further call focused in particular on the other two themes - carbon management and renewable energy - will be issued in mid-2004.

The present call invites expressions of interest for participation in:

- Consortia under the theme Nuclear Power - Keeping the nuclear option open
- Research Groups under the theme Managing new uncertainties - The socio-economic challenges and implications of moving towards a sustainable energy economy
- Expressions of interest for Networks and Collaborative proposals will also be considered, under either of the themes Carbon management and Renewable energy.
- Expressions of Interest for preparation for projects (Capacity Building) will also be considered under any of the areas except Nuclear power.

The key features of Consortia, Research Groups, Networks, Collaborative Proposals and Capacity Building are described in the Application Process.

Consortium bids: Nuclear power - Keeping the Nuclear Option Open
The research challenges in fission R&D span areas as diverse as maintaining and extending the life of existing generation plant; management of the

current and future fission waste legacy; technology for future fission power generation; and research that can contribute to an open and informed debate on the current and future role for nuclear power in the UK's energy supply industry. The scope of this theme has been broken down into three main topics:

- maintaining current generation capacity
- fission within a sustainable energy economy
- future fission power.

The sponsors intend to commission one or more large, integrated, multidisciplinary projects that can address the research challenges, with the scope of projects potentially cutting across the three topics.

Further details on the scope of the theme and consortia requirements can be found in Annex 2.

Research Group bids: Managing new uncertainties - The Socio-Economic Challenges and Implications of Moving Towards a Sustainable Energy Economy
The aim of this theme is to facilitate research on the cross-cutting socio-economic challenges and implications of moving towards a sustainable energy economy and their interactions with broader technological, engineering, and environmental issues. It offers opportunities for productive, interdisciplinary research within and beyond the socio-economic field, with the potential to contribute to the development of whole-systems approaches to energy issues. Many of the potential research issues have resonance in a number of other areas of public policy and are not specific to energy. In line with the aims of the programme, this theme is not constrained by traditional disciplinary or Research Council boundaries, whilst focusing on the socio-economic research agenda. Although a number of the proposed topics and questions focus on UK and European issues, many are generic and could be applied to both OECD and developing country contexts.

Possible topics identified under this theme include:

- Processes of long-run change in socio-technical systems
- Vulnerability, resilience and adaptiveness
- Services, systems of provision and consumption practices
- Policies in natural monopolies and liberalised markets
- Public attitudes and processes of governance
- Energy in the global context
- Integrated appraisal of energy systems.

This framework should be regarded as illustrative, not definitive. Researchers are encouraged to define and justify alternative topics and questions which would contribute towards the TSEC programme's overall objectives.

More detail on this theme can be found in Annex 3.

Expressions of Interest under the themes Carbon management and Renewable energy will be considered in this call. However, the following brief indication of the scope of these two themes is given for initial guidance only; a detailed scope will be provided in the next call, expected to be mid 2004.

Carbon management
Conventional energy research is often vertically divided, so that research looks at the use of individual fuels, or energy use in particular industrial, commercial or domestic sectors. There needs to be more "cross-boundary" and "whole systems" research, looking at how different technologies and social/environmental factors might be optimised to deliver the overall objectives. The following are two examples of the type of issues which should be addressed.

Fuel switching and renewables
Displacing coal and petroleum with natural gas and/or biogas, or biofuels, or renewables are alternative ways of reducing carbon dioxide (CO2) emissions. These options require a full whole lifecycle approach to carbon management, integrating environmental, engineering, resource, economic and social dimensions. Issues such as length and type of supply chains, emissions associated with agriculture, fuel processing, infrastructure and construction need to be fully understood to limit the risk that emissions are increased or displaced to another part of the energy/resource chain.

Carbon dioxide capture and storage
The continued use of fossil fuels will demand effective carbon management, particularly through reduction of the associated CO2 emissions. The greatest long-term potential for reduced CO2 emissions to the atmosphere from fossil fuels is likely to be through capturing CO2 from large industrial point sources before it enters the atmosphere, and then sequestering it back into the bio/geosphere by geological means. The research challenges include: the mechanisms of large-scale carbon capture at source, CO2 storage, transport and distribution, and geological sequestration, monitoring and verification technologies as well as modelling the long term fate of CO2 injected into a variety of geological scenarios. Understanding is also needed of the potential risk posed by CO2 leakage into terrestrial and marine settings, and of the economic risks, costs and benefits, public acceptability and regulatory issues associated with moving towards large-scale CO2 capture.

Renewable energy
The objectives for TSEC in this area will centre on work that supports the development of renewable and sustainable energy systems of relevance to the UK economy. Specifically, it will: encourage the introduction of renewable and sustainable energy systems into the UK economy; encourage consideration of renewable energy in the context of social/economic/environmental issues and carbon management; and provide data for the development of policy. TSEC will fund research that is complementary to that supported through other Research Council activities, such as the ongoing Sustainable Power Generation and Supply Programme (SUPERGEN). Again, the following is purely an example of the type of research which could be funded.

Carbon cycle audits
Audits of full lifecycle carbon (or carbon equivalents of other greenhouse

gases emitted in the lifecycle) need to be undertaken, and the energy balances of different renewable energy generating technologies need to be considered and understood, if true impacts on carbon reduction are to be achieved. For example, if energy crops are to be encouraged, then consequences on land use change, aquifer recharge, and rainfall run off need to be fully understood. It would also be important to ensure that the crops are 'low-input' in terms of energy usage and that the energy balance is therefore positive. Environmental impacts of growing energy crops would have to be compared with the alternative land use (food crops, set-aside, etc)), and consideration given to their potential economic and social impacts.

Risks, barriers and incentives in renewables innovation
Innovation will be essential in the renewables industry if the sector is to play a central role in future energy supply. Research is required to understand and quantify the risks inherent in the development of new technology and the barriers preventing its exploitation to inform both the priorities of future renewable energy R&D and the development of future market instruments and incentives that can encourage the effective management of risk and enable the exploitation of the outputs of R&D. In the longer term, new disruptive technology may significantly affect the operation of the energy market, and research is required to investigate how incentives and market instruments can adapt to changing market conditions while still providing a long term framework within which companies can make capital investments requiring a return on capital over long (20-30 year) timescales. (In addition to research on such issues relating specifically to renewables there are opportunities for broader cross-cutting research on these issues under the Managing New Uncertainties Theme).

The Application Process
The schemes and theme areas under which EoIs will be accepted in this call are highlighted in colour in the table below.

	Nuclear power	Managing new uncertainties	Carbon management	Renewable energy
Consortia				
Research groups				
Networks				
Collaborative proposals				
Capacity building				

Characteristics of the schemes

Consortium
A Consortium will comprise a number of academic groups, normally from different disciplines and institutions, working in partnership with appropriate stakeholders and users to design and deliver a collaborative programme of world-class research. It is expected that the consortium will deliver higher quality research outputs than groups working in isolation. This call for expressions of interest is open to all potential partners of a research consortium, irrespective of their existing links to academic research in the field. Consortia may be funded at a value of up to £5m. Expressions of interest can be submitted by individuals, existing groups, and existing or new collaborations. However, where expressions of interest are made by a group or collaboration, the Research Councils reserve the right to take forward those expressions in total or in part during the Consortium-building process, potentially excluding elements of proposed collaborations.

Research Groups
A Research Group will be a national focal point for research where researchers can collaborate on long-term inter-disciplinary projects. It will facilitate the building of strong relationships with research users, international collaboration and the development of the careers of new and

outstanding researchers.

Funded initially for five years, Research Groups will be expected to provide the training for postgraduate students and other new researchers where appropriate, and to improve opportunities for securing co-funding or sponsorship from sources outside the Science Vote. Applications for Research Group funding will normally be expected to be in the range of £200k - £600k per annum although applications outside this range can be considered.

Networks
A major task of UKERC will be to co-ordinate a National Energy Research Network that will draw in all significant research activities. However, once the components of this network are known, the TSEC programme will wish to support new research 'nodes' that complement them. Such complementary activities would normally be UK-based networks that link research groups and industrial organisations, across disciplines, to develop new or enhanced collaborations.

Collaborative Proposals
These will be intended to support focussed, co-ordinated, collaborative research into specific issues and will be expected to enhance opportunities for inter-disciplinary collaboration. A minimum of three eligible institutions are required for a proposal under this scheme, each of which will be separately awarded funds. The consortium will retain ownership and management of the science programme, and a lead institution will be expected to act as co-ordinator.

Collaboration awards will provide funding for up to five years with costs ranging, as required by the research, from modest sums up to approximately £2M. Proposals may include tied research studentships.

Proposers are free to submit expressions of interest for one or more themes.

Capacity building
For projects that require considerable preparation, applicants may submit an Expression of Interest for capacity building, to a maximum of £50k, for:

. support for a researcher to work in a different science department for a period of up to 12 months (eg for a natural scientist to work in a social science department);

. support for an overseas researcher to work in a UK institution, or for a UK researcher to work in an overseas institution, for up to 12 months focusing on interdisciplinary research issues;

. support for a series of four or more interdisciplinary events (involving social and natural scientists) over a 12 month period;

. scoping studies, focusing on any of the TSEC themes. Applicants must demonstrate the interdisciplinary nature of the proposed research. Awards may be up to 12 months in duration

Eligibility
Standard Research Council eligibility criteria will apply to this call; those normally eligible to participate in any Research Council programme can apply. Research Council funding can only be awarded to UK universities, Research Council institutes, Government Research Establishments and

not-for-profit research organisations. Organisations and industry which are themselves ineligible for receipt of Research Council funding may participate, using their own cash or in-kind support.

Applications from members of the public or individuals outside academia will not be accepted.

Academic expressions of interest may be submitted by leaders of individual research groups within one or more universities. While existing groups of researchers are able to apply as a team, it should be recognised that the Research Councils may recommend the building of new partnerships involving only a minority of members from existing collaborations. Where there is scope to do so, it is recommended that individuals submit their own expression of interest on behalf of their group.

The Selection Process
An initial sift of EoIs will be conducted by expert panels established by the Programme Scientific Advisory Committee or by the SAC. Applications will be judged on their quality, innovation, originality and compliance with the objectives of the programme.

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Quality - The proposal should indicate clear potential to support innovative and high quality research of international standing and include information on the capacity and track record of the applicants in delivering such high quality research. This should not rely on publication lists, but present evidence of recognised first-class research, innovation and collaboration.

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Innovation - The proposal should present novel approaches to current research challenges and persuasive approaches to roadmap solutions. This should be in the context of the research theme defined in the technical appendix.

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Originality - The proposal should demonstrate innovative approaches to problem solving with evidence of ability, creativity and vision and added value to current research in the field. The application should be focused toward addressing research challenges of the theme.

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Objectives - The applicant should communicate an enthusiasm for collaboration and ability to contribute to a programme of research that delivers the objectives of the TSEC programme. They should demonstrate awareness of the drivers affecting the research agenda and the potential to contribute to the development of whole-systems approaches to energy issues.

Applicants for consortia will be informed of the outcome of their bids in January 2004 and if successful will be invited to a workshop in March 2004 to facilitate the formation of consortia partnerships. Attendance at the workshops will be mandatory for consortium members, including users and industrial collaborators. Following the workshops, consortium partners will be invited to submit EPSRC grant applications, which will be subject to rigorous peer review.

Applicants for Research Groups will be informed of the outcome of their bids by mid-March 2004 and if successful invited to submit full proposals by mid-June. Assessment of full proposals will entail applicants being interviewed by the assessment panel in September/October 2004.

All other applicants will be informed of the outcome of their bids in

February 2004 and successful applicants invited to submit full proposals as appropriate.

How to Apply

Expressions of Interest
Expressions of Interest must be submitted using the Research Councils' joint application form (available in Word or PDF versions)and (with the exception of proposals for Research Groups on Managing the New Uncertainties - see below) be accompanied by no more than four sides of A4 text (minimum font 12 pt), including diagrams, figures and charts etc. in support of the application. This should include any relevant information that will assist assessment of the project that is not covered in the sections of the application form. It should include

. Details of the track record of the applicant or business and the particular qualities they would bring to the proposal.

. Identification of the broad challenge which the applicant would seek to address or to which they would be able to contribute

. Definition of the perceived key research challenges within the theme.

. Indication of potential deliverables.

. Information on the collaborating organisation in terms of cash or in-kind support and proposed benefits from collaboration.

Expressions of interest for Research Groups under the 'Managing the New Uncertainties' theme must be submitted using the Research Councils joint application form. However instead of the four sides outlined above the form should be accompanied by the following information:

. A research proposal of no more than 3,000 words outlining the main proposed elements of the proposed Group's research programme and how this would contribute towards the achievement of the objectives of the Towards a Sustainable Energy Economy Programme

. Plus the following appendices:

- no more than 1 side of A4 (minimum font 12 pt) providing details of references cited in the research proposal

- no more than 1 side of A4 (minimum font 12 pt) giving details of the proposed strategies for involving non-academic users at all stages and outlining the potential for collaboration and/or co-funding

- no more than two sides of A4 (minimum font 12 pt) outlining the proposed management structure of the Research Group, including time commitments of the proposed Director(s) and abbreviated cvs for all named applicants.

- no more than one side of A4 (minimum font 12 pt) outlining the Group's strategy for contributing to the development of inter-disciplinary research capacity in the field.

In section E of the form, under Scheme applicants should state Consortium, Centre Group, Network, Collaborative proposal, or Capacity building, as appropriate; and under Call should insert 'TSEC call 1': followed by the appropriate theme name: Nuclear; Managing new uncertainties; Carbon Management, or Renewable energy.

As the majority of institutions have not yet registered with the Research Councils for electronic submission, in this call electronic submissions cannot be accepted. An original plus ONE copy are required in hard copy. Faxed copies are not acceptable.

All applications should be submitted to reach the NERC at the address below no later than 5pm on 19th January 2004. Personal callers may deliver applications during normal office hours only (9am - 5pm Monday - Friday). The Research Councils will reject late or incomplete submissions and those that do not comply with the application criteria set out above.

Receipt of applications will be acknowledged after the closing date. It will assist administration of the call if applicants do not telephone to enquire if their proposal has been received.

Applications and administrative queries should be addressed in the first instance to:
Dr Chris Baker (e-mail preferred)
Programme Co-ordinator
Science and Innovation Programmes
NERC, Polaris House, North Star Avenue
SWINDON, Wiltshire SN2 1EU.
Telephone 01793 411758.

Queries regarding the technical aspects of the Nuclear Power theme should be addressed to: Dr Peter Hedges, EPSRC, telephone 01793 444176. Queries regarding the application criteria or eligibility for the Nuclear Power theme should be addressed to the Associate Programme Manager Mr Robert Heathman, Room GFN, EPSRC, telephone 01793 444131.

Queries regarding the application criteria or eligibility for the Managing New Uncertainties theme should be addressed to Mr Paul Rouse, Senior Science and Development Manager, Research Training and Development Directorate (RTD), ESRC, at the above address, telephone 01793 413030, or Mr Oliver Moss, Science and Development Manager, RTD, ESRC, telephone 01793 413064.

All other queries should

_____
Dr. J.P. Darch
Research Administrator
School of Environmental Sciences
University of East Anglia
Norwich
NR4 7TJ
U.K.

Tel : 44 (0)1603 592994
Fax : 44 (0)1603 593035

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From: Jan Esper <esper@wsl.ch>
To: Briffa Keith <k.briffa@uea.ac.uk>, Cook Ed <drdendro@ldeo.columbia.edu>
Subject: EOS revision
Date: Mon, 12 Jan 2004 10:26:27 +0100

<x-flowed>
Hi Ed and Keith

for your information, I attached the revision of the EOS article. In
this version we added some lines about the data-overlap between the
MBH and ECS records.

I also attached a figure showing a comparison between MBH and
EsperFULL (using all data) and EsperSUB (without Tornetraesk and the
Polar Urals).

Take care
Jan
--
Dr. Jan Esper
Swiss Federal Research Institute WSL
Zuercherstrasse 111, 8903 Birmensdorf
Switzerland
Phone: +41-1-739 2510
Fax: +41-1-739 2215
Email: esper@wsl.ch
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Attachment Converted: "c:\eudora\attach\Response_Figure.eps.pdf"

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From: Phil Jones <p.jones@uea.ac.uk>
To: mann@virginia.edu
Subject: CLIMATIC CHANGE needs your advice - YOUR EYES ONLY !!!!!
Date: Fri Jan 16 13:25:59 2004

    Mike,
       This is for YOURS EYES ONLY. Delete after reading - please ! I'm trying to
redress the
    balance. One reply from Pfister said you should make all available !! Pot
calling the
   kettle
    black - Christian doesn't make his methods available. I replied to the wrong
Christian
   message
    so you don't get to see what he said. Probably best. Told Steve separately and
to get
   more
    advice from a few others as well as Kluwer and legal.

PLEASE DELETE - just for you, not even Ray and Malcolm

Cheers
Phil

Date: Fri, 16 Jan 2004 12:37:29 +0000
To: Christian Azar <christian.azar@fy.chalmers.se>, christian.pfister@hist.unibe.ch
From: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: AW: CLIMATIC CHANGE needs your advice
Cc: "'David G. VICTOR'" <dgvictor@stanford.edu>, 'Katarina Kivel' <kivel@stanford.edu>, N.W.Arnell@soton.ac.uk, frtca@fy.chalmers.se, d.camuffo@isac.cnr.it, scohen@sdri.ubc.ca, pmfearn@inpa.gov.br, jfoley@facstaff.wisc.edu, pgleick@pipeline.com, harvey@geog.utoronto.ca, ahs@ansto.gov.au, Thomas.R.Karl@noaa.gov, rwk@ucar.edu, rik.leemans@rivm.nl, diana.liverman@eci.ox.ac.uk, mccarl@tamu.edu, lindam@atd.ucar.edu, rmoss@usgcrp.gov, ogilvie@spot.colorado.edu, barrie.pittock@dar.csiro.au, pollard@essc.psu.edu, nj.rosenberg@pnl.gov, crosenzweig@giss.nasa.gov, j.salinger@niwa.co.nz, santer1@llnl.gov, h.j.schellnhuber@uea.ac.uk, F.I.Woodward@sheffield.ac.uk, gyohe@wesleyan.edu, leonid@atmosp.physics.utoronto.ca, shs@stanford.edu

Dear Steve et al,
I've been away this week until today. Although the responses so far all make valid points, I will add my thoughts. I should say I have been more involved in all the exchanges between Mike and MM so I'm probably biased in Mike's favour. I will try and be impartial, though, but I did write a paper with Mike (which came out in GRL in Aug 2003) and we currently have a long paper tentatively accepted by Reviews of Geophysics. With the latter all 4 reviewers think the paper is fine, but the sections referring to MM and papers by Soon and Baliunas are not and our language is strong. We need to work on this.
Back to the question in hand:
1. The papers that MM refer came out in Nature in 1998 and to a lesser extent in GRL in 1999. These reviewers did not request the data (all the proxy series) and the code. So, acceding to the request for this to do the review is setting a VERY dangerous precedent. Mike has made all the data series and this is all anyone should need. Making model code available is something else.
2. The code is basically irrelevant in this whole issue. In the GRL paper (in 2003 Mann and Jones), we simply average all the series we use together. The result is pretty much the same as MBH in 1998, Nature and MBH in 1999 in GRL.
3. As many of you know I calculate gridded and global/hemispheric temperature time series

each month. Groups at NCDC and NASA/GISS do this as well. We don't exchange codes
- we do occasionally though for the data. The code here is trivial as it is in the
paleo work.
MBH get spatial patterns but the bottom line (the 1000 year series of global temps) is
almost the same if you simply average. The patterns give more, though, when it comes to
trying to understand what has caused the changes - eg by comparison with models. MM
are only interested in the NH/Global 1000-year time series - in fact only in the MBH
work
from 1400.
4. What has always intrigued me in this whole debate, is why the skeptics (for want of
a better term) always pick on Mike. There are several other series that I've produced,
Keith Briffa has and Tom Crowley. Jan Esper's work has produced a slightly different
series
but we don't get bombarded by MM. Mike's paper wasn't the first. It was in Nature and
is well-used by IPCC. I suspect the skeptics wish to concentrate their effort onto one
person as they did with Ben Santer after the second IPCC report.
5. Mike may respond too strongly to MM, but don't we all decide not to work with or
co-operate with people we do not get on with or do not like their views. Mike will say
that MM are disingenuous, but I'm not sure how many of you realise how vicious the
attack on him has been. I will give you an example.
When MM came out, we had several press calls (I don't normally get press calls about
my papers unless I really work at it - I very rarely do). This was about a paper in
E&E, which when we eventually got it several days later was appalling. I found out
later that the authors were in contact with the reviewers up to a week before the
article
appeared. So there is peer review and peer review !! Here the peer review was done by
like-minded colleagues. Anyway, I'm straying from the point. Tim Osborn, Keith Briffa
and I felt we should put something on our web site about the paper and directs people
to Mike's site and also to E&E and the MM's site. MM have hounded us about this for
the last four months. In the MM article, they have a diagram which says 'corrected
version' when comparing with MBH. We have seen people refer to this paper (MM)
as an alternative reconstruction - yet when we said this is our paragraph MM claim they
are not putting forward a new reconstruction but criticizing MBH 1998 !! We have
decided to remove the sentence on our web page just to stop these emails. But if a
corrected version isn't a new or alternative reconstruction I don't know what is.
So, in conclusion, I would side with Mike in this regard. In trying to be

scrupulously
fair, Steve, you've opened up a whole can of worms. If you do decide to put the Mann
response into CC then I suspect you will need an editorial. MM will want to respond
also.
I know you've had open and frank exchanges in CC before, but your email clearly shows
that you think this is in a different league. MM and E&E didn't give Mann the chance
to
respond when they put their paper in, but this is a too simplistic. It needs to be
pointed
out in an editorial though - I'm not offering by the way.
I could go on and on ....
Cheers
Phil
At 10:36 15/01/2004 +0100, Christian Azar wrote:

Dear all,
I agree with most of what has been said so far. Reproducibility is the key word. If the
Mann el al material (to be) posted on the website is sufficient to ensure
reproducibility, then there is no compelling need to force them to hand it out. If not,
then the source code is warranted. Also, even if there is no compelling need to make the
source code public, doing it anyway would clearly be beneficial for the entire debate.
Yours,
Christian

Christian Azar
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[1]www.frt.fy.chalmers.se
[2]www.miljo.chalmers.se/cei

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Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

References

1. http://www.frt.fy.chalmers.se/
2. http://www.miljo.chalmers.se/cei

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From: Edward Cook <drdendro@ldeo.columbia.edu>
To: "Art Johnson" <ahj@sas.upenn.edu>
Subject: RE: Seminar
Date: Sat, 17 Jan 2004 07:55:24 -0500
Cc: druid@ldeo.columbia.edu, druidrd@ldeo.columbia.edu, k.briffa@uea.ac.uk

<x-flowed>
Hi Art,

Sorry for the lack of response to your emails. Been over the top as usual on things. I go off to Tasmania and New Zealand on Jan 20 and return on Feb 15. Bhutan was a bit strange this time. I was sick most of the time, but we did get some useful stuff done nonetheless.

>Hi Ed,
>
>I hope your trip to Bhutan went well. We did OK in Chile but encountered
>some glitches. I am emailing about a three things to see if you are
>interested:
>
>1) What does Gordon know about the big white spruce in the Mackenzie R.
>basin of the northern NWT? I am going to be in Alberta this summer and it is
>one plane ride and a few hundred $ from those big spruce. If I can get the
>cores, are you interested in collaborating on their measurement and
>analysis? If I can track down the person that told us that some of the trees
>were 600 y old, we might be able to find some of them. There are many spruce
>pilings in town that were probably cut in the 50's-70's and some of those
>might have been pretty old trees given their size. What is the availability
>of climate data? Inuvik probably has records back into the 50's when they
>rebuilt the town. Dick Jagels is interested in those trees too, as we are
>led to believe that they need 24 hr photoperiods when they are seedlings.
>Could this be a race of trees that respond to differences in growing-season
>sunlight?

I am cc'ing this email to Gordon and Rosanne. I think that they would be interested in what you describe. They also know what climate data are available. I recall that Aklavik has a older record that was discontinued a few years back. It may be possible to merge Aklavik with Inuvik temperature records to cover most of the 20th century.

>
>2) The Forest Service has an RFP out for projects in the "northern forest"
>I think this is defined as mostly Vermont and New Hampshire since it is a
>Senate-funded program sponsored by senators from those states. The "threat"
>(their term) of global warming to forest health is one of the themes that
>Chris Eagar is in charge of. We have been working with Vermont northern
>hardwood data collected by Post and Curtis in the 1950's and redone by us in
>the early 90's. There is a very nice multiple regression model that shows
>clearly that temperature (altitude/latitude) and soil moisture are very good
>predictors of site index (height at 75 yrs. e.g. productivity potential).
>Nutrients do not explain any additional variance. This model would suggest

>that warming would improve productivity, not decrease it. I am wondering if
>a dendroclimatological analysis of maple, beech and ash and yellow birch
>would show a response of growth to summer temperatures? I think we have all
>the cores from our 1990 study, and it would be an easy matter to get more. I
>stll owe the Forest Service a couple of papers from the 90-91 work which
>they funded, but I am actually working on them now, and could have them done
>by the March 30 deadline for the full proposal, if not for the Feb. 13
>preproposal deadline. I'm sure I could talk to Chris to see if our ideas are
>viable, and if we would be penalized for not publishing the Vermont stuff in
>a timely manner.

This sounds interesting. Are you measuring up all of the tree cores?
I wouldn't have the resources to do that without some technician
support, but I could participate in some dendroclimatic analyses of
the data with you.

>
>3) We are running cellulose O reasonably well at this time, and are still
>interested in seeing if cellulose O is useful in determining whether the
>temperature signal in mideval wood is similar to that of the past century,
>and if there is an isotopic signature in the Little Ice Age wood that
>indicates it was cold. What do you think about the availability of wood
>samples from dated rings from those periods? Is any of the Esper wood
>available? When we talked after your seminar, it seemed to me that the
>Scandanavian wood collection might be useful.

I did ask Keith Briffa about this stuff. He is tied in closely with
much of the work that has been done in Fennoscandia and even over to
the Polar Urals. He also said that there has been some isotopic work
done on wood, but he wasn't sure about results. I suggest that you
contact Keith directly (k.briffa@uea.ac.uk) and maybe he can direct
you to sources of wood for your proposed study. It is interesting, if
a bit chancy in my estimation.

Cheers,

Ed

>
>
>What do you think?
>
>Art
>
>
>
>-----Original Message-----
>From: Edward Cook [mailto:drdendro@ldeo.columbia.edu]
>Sent: Saturday, October 11, 2003 2:28 PM
>To: Art Johnson
>Subject: RE: Seminar
>
>
>Hi Art,
>
>I will be driving down to your digs on Friday, Oct 17 to give the
>seminar I promised. When is it scheduled so I know how early I
>definitely have to leave. I need directions to get there as well, as
>I have never been to Penn before. Also, it would be useful to have a
>place to stay Friday night, I suppose. My wife is off to CT to
>celebrate a 50th birthday with a friend that weekend, so there is no
>point in zipping back in any case.
>

>Cheers,
>
>Ed
>--
>================================
>Dr. Edward R. Cook
>Doherty Senior Scholar and
>Director, Tree-Ring Laboratory
>Lamont-Doherty Earth Observatory
>Palisades, New York 10964 USA
>Email: drdendro@ldeo.columbia.edu
>Phone: 845-365-8618
>Fax: 845-365-8152
>================================

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================================
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Palisades, New York 10964 USA
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Fax: 845-365-8152
================================
</x-flowed>

389. 1074609944.txt
##
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Malcolm Hughes" <mhughes@ltrr.arizona.edu>, "Malcolm Hughes"
<mhughes@ltrr.arizona.edu>, Tim Osborn <t.osborn@uea.ac.uk>,"Michael E. Mann"
<mann@virginia.edu>
Subject: Re: J. Climate paper - in confidence
Date: Tue, 20 Jan 2004 09:45:44 +0000
Cc: Scott Rutherford <srutherford@rwu.edu>

<x-flowed>
Malcolm seems to have done a good job sorting out these constituent sets ,
and I don't have anything to add other than agreeing that as a general
principal , where possible, original chronologies should be used in
preference to reconstructed temperature series (the latter having been
already optimized using simple or multiple regression to fit the target
temperature series). This applies not only to our western US
reconstructions (which it should be stressed are based on very flexible
curve fitting in the standardisation - and inevitably can show little
variance on time scales longer than a decade or so) but also to the
Tornetrask and Polar Urals reconstructions (each of which was based on ring
width and density data , but standardised to try to preserve centennial
variability - though the density series had by far the largest regression
coefficients). There is though a question regarding the PCs of the Siberian
network (presumably provided by Eugene?) . The correlation between density
and ring width can get high in central and eastern parts of the network ,
so even though these are different variables , it might not be strictly
true to think of them as truly independent (statistically) of the density
chronologies we use from the Schweingruber network (there may also be a
standardisation issue here , as the density chronologies were standardised
with Hugershoff functions for our initial network work (as reported in the

Holocene Special Issue) whereas your PC amplitudes may be based on
"Corridor Standardisation" - which likely preserves less low frequency?) .
These remarks are simply for clarification and discussion , and I too will
wait on your response draft , though I would throw in the pot the fact that
omitting the time dependent stuff would simplify the message at his stage.
cheers
Keith

At 01:42 PM 1/19/04 -0700, Malcolm Hughes wrote:
>Mike - there are the following density data in that set:
>1) 20 Schweingruber/Frttss series from the ITRDB (those that
>met the criteria described in the Mann et al 2000 EI paper)
>2) Northern Fennoscandia reconstruction (from Keith)
>3) Northern Urals reconstruction (from Keith)
>4) 1 density series for China (Hughes data) and one from India
>(also Hughes data) - neither included in Keith's data set, I think.
>5) To my great surprise I find that you used the Briffa gridded
>temperature reconstruction from W. N. America (mis-attributed
>to Fritts and Shao) - of course I should have picked up on this 6
>years ago when reading the proofs of the Nature sup mat. It was
>my understanding that we had decided not to use these
>reconstructions, as the data on which they were based were in the
>ITRDB, and had been subject to that screening process. So
>depending on whether you used the long or the shorter versions
>of these, there will have been a considerable number of density
>series included , some of them twice. It means that there is
>considerably more overlap between the two data sets, in North
>America, than I have been telling people. I stand corrected.
>Cheers, Malcolm
>.
>.Malcolm Hughes
>Professor of Dendrochronology
>Laboratory of Tree-Ring Research
>University of Arizona
>Tucson, AZ 85721
>520-621-6470
>fax 520-621-8229

--
Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909
Fax: +44-1603-507784

http://www.cru.uea.ac.uk/cru/people/briffa/

</x-flowed>

390. 1074612429.txt
##########

From: "Malcolm Hughes" <mhughes@ltrr.arizona.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, "Malcolm Hughes" <mhughes@ltrr.arizona.edu>,
Tim Osborn <t.osborn@uea.ac.uk>, "Michael E. Mann" <mann@virginia.edu>
Subject: Re: J. Climate paper - in confidence
Date: Tue, 20 Jan 2004 10:27:09 -0700
Cc: Scott Rutherford <srutherford@rwu.edu>, mann@virginia.edu

Mike - you are right that we should probably leave the network uncahnged for this mss. In fact, however, as Keith indicated, the Vaganov data probably retained a fair amount of low frequency because of the use of the corridor method (i.e. were not "heavily standardized"). CHeers, Malcolm
On 20 Jan 2004 at 7:58, Michael E. Mann wrote:

> Thanks Keith,
>
> I agree w/ this--I think the Vaganov chronologies were pretty heavily
> standardized, and the other issues you raise are important. In the
> future, we would (and will) be a bit more circumspect about the use of
> some of these data.
>
> In the present case, however, I think we are forced to use the exact
> same network.
>
> Re, the omission of some results. I think we can probably keep them.
> Simply by cleaning up the text, removing redundancy, etc. I've
> shortened and tightened the manuscript considerably, and I think I've
> improved the logical flow a bit in the process. So my feeling is that
> we will not have to split this up, but I'll leave this to all of you
> to decide after you see the revised draft from Scott and me...
>
> Thanks,
>
> mike
>
> At 09:45 AM 1/20/2004 +0000, Keith Briffa wrote:
>   Malcolm seems to have done a good job sorting out these
>   constituent sets , and I don't have anything to add other than
>   agreeing that as a general principal , where possible, original
>   chronologies should be used in preference to reconstructed
>   temperature series (the latter having been already optimized
>   using simple or multiple regression to fit the target temperature
>   series). This applies not only to our western US reconstructions
>   (which it should be stressed are based on very flexible curve
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>   the Tornetrask and Polar Urals reconstructions (each of which was
>   based on ring width and density data , but standardised to try to
>   preserve centennial variability - though the density series had by
>   far the largest regression coefficients). There is though a
>   question regarding the PCs of the Siberian network (presumably
>   provided by Eugene?) . The correlation between density and ring
>   width can get high in central and eastern parts of the network ,
>   so even though these are different variables , it might not be
>   strictly true to think of them as truly independent
>   (statistically) of the density chronologies we use from the
>   Schweingruber network (there may also be a standardisation issue
>   here , as the density chronologies were standardised with
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> series included , some of them twice. It means that there is
> considerably more overlap between the two data sets, in North
> America, than I have been telling people. I stand corrected.
> Cheers, Malcolm . .Malcolm Hughes Professor of Dendrochronology
> Laboratory of Tree-Ring Research University of Arizona Tucson, AZ
> 85721 520-621-6470 fax 520-621-8229
>
> --
> Professor Keith Briffa,
> Climatic Research Unit
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>
> Phone: +44-1603-593909
> Fax: +44-1603-507784
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> http://www.cru.uea.ac.uk/cru/people/briffa/
>
> _____
> __
> Professor Michael E. Mann
> Department of Environmental Sciences, Clark Hall
> University of Virginia
> Charlottesville, VA 22903
> _____
> _ e-mail: mann@virginia.edu Phone: (434) 924-7770FAX: (434) 982-2137
> http://www.evsc.virginia.edu/faculty/people/mann.shtml

Malcolm Hughes
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Laboratory of Tree-Ring Research
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Tucson, AZ 85721
520-621-6470
fax 520-621-8229

391. 1075297872.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: p.jones@uea.ac.uk
Subject: Fwd: EOS revision
Date: Wed Jan 28 08:51:12 2004

X-Sender: esper@mail.wsl.ch
Date: Mon, 12 Jan 2004 10:26:27 +0100
To: Briffa Keith <k.briffa@uea.ac.uk>,

Cook Ed <drdendro@ldeo.columbia.edu>
From: Jan Esper <esper@wsl.ch>
Subject: EOS revision
Hi Ed and Keith
for your information, I attached the revision of the EOS article. In this version we
added some lines about the data-overlap between the MBH and ECS records.
I also attached a figure showing a comparison between MBH and EsperFULL (using all data)
and EsperSUB (without Tornetraesk and the Polar Urals).
Take care
Jan
--
Dr. Jan Esper
Swiss Federal Research Institute WSL
Zuercherstrasse 111, 8903 Birmensdorf
Switzerland
Phone: +41-1-739 2510
Fax: +41-1-739 2215
Email: esper@wsl.ch

--
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Phone: +44-1603-593909
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[1]http://www.cru.uea.ac.uk/cru/people/briffa[2]/

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/
2. http://www.cru.uea.ac.uk/cru/people/briffa/

392. 1075393544.txt
##########

From: Iain Brown <Iain.Brown@uea.ac.uk>
To: a.watkinson@uea.ac.uk
Subject: Inter-reg proposal update
Date: Thu, 29 Jan 2004 11:25:44 +0000
Cc: m.hulme@uea.ac.uk, s.jude@uea.ac.uk

Andrew,

Here is an update on the Inter-reg proposal, based upon the recent Oxford workshop.

Organisations involved:
EA, EN, Oxford ECI, Oxford Brooks (Planning), Alterra (Netherlands), Hampshire CC, Kent CC, Conservatoire de Littoral, Clare CC, Maynooth U., Tyndall

Funding:
Aiming for a 3 year project of 3-4 million Euros. Inter-reg 3B most closely fits project objectives but still unknown whether sufficient funds remain for this. Inter-reg 3C represents an alternative, but requires more high-level policy. Inter-reg deadline is April 29th. Other alternatives are LIFE and Framework VI.

Key issue:
Are Tyndall to be included as a Partner or a Contractor? Partners have more influence on project development but would require 50% matched funding (however this can be met through including other contributing R&D projects). Contractors do not need matched funding but may have to officially tender for sub-contract.

Proposed Work Packages:
1 Policy Review of spatial planning mechanisms for biodiversity (European, national, regional, local). How will this cope with climate change? Oxford Brooks & Oxford ECI to lead on developing this WP.
2 Broad-scale Review of impacts of climate change on biodiversity in NW Europe. To identify main drivers, issues and vulnerabilities on a network basis. Lead: Alterra, Oxford ECI, Tyndall
3 Coastal case studies - Hamble (England), Shannon (Ireland), Baie de Vaie (France). Objectives to evaluate local management issues with regard to simulation of future coastal evolution. Lead: EA, Hampshire CC
4 Terrestrial case studies - 2 regions: SE England, Limburg. Lead Alterra, ECI
5 Policy Development & Guidance - based on review of research outputs. Lead EN
6 Dissemination

Cross-cutting issues - stakeholder engagement, assessment/management of key habitats

Next steps - develop WPs, workplans and costing of proposal by 27th Feb.

Next meeting 4th/5th March, Oxford.

regards,

Iain

393. 1075403821.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@virginia.edu
Subject: Fwd: John L. Daly dead
Date: Thu Jan 29 14:17:01 2004

> From: Timo Hämeranta <timo.hameranta@pp.inet.fi>
> To: <timo.hameranta@pp.inet.fi>
> Subject: John L. Daly dead
> Date: Thu, 29 Jan 2004 12:04:28 +0200
> X-Mailer: Microsoft Outlook, Build 10.0.4510
> Importance: Normal

Mike,
In an odd way this is cheering news ! One other thing about the CC paper - just found
another email - is that McKittrick says it is standard practice in Econometrics journals
to give all the data and codes !! According to legal advice IPR overrides this.

Cheers
Phil

"It is with deep sadness that the Daly Family have to announce the sudden death of John

Daly.Condolences may be sent to John's email account (daly@john-daly.com)
"

Reported with great sadness

Timo Hämeranta
xxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxx

Timo Hämeranta, LL.M.
Moderator, Climatesceptics
Martinlaaksontie 42 B 9
01620 Vantaa
Finland, Member State of the European Union

Moderator: timohame@yahoo.co.uk
Private: timo.hameranta@pp.inet.fi

Home page: [1]http://personal.inet.fi/koti/hameranta/climate.htm

Moderator of the discussion group "Sceptical Climate Science"
[2]http://groups.yahoo.com/group/climatesceptics

"To dwell only on horror scenarios of the future
shows only a lack of imagination". (Kari Enqvist)

"If the facts change, I'll change my opinion.
What do you do, Sir" (John Maynard Keynes)

xxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxxx

Prof. Phil Jones
Climatic Research Unit Telephone +44 (0) 1603 592090
School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

References

1. http://personal.inet.fi/koti/hameranta/climate.htm
2. http://groups.yahoo.com/group/climatesceptics

394. 1075750656.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Rashit Hantemirov <rashit@ecology.uran.ru>
Subject: Re[2]: Stephen McIntyre
Date: Mon Feb 2 14:37:36 2004

Rashit
that sounds great - at least I am happy you are working on the sub fossil material still. I
have done some work comparing the Swedish and Finnish long series after standard RCS
detrending and there is good similarity at the century timescale for some considerable
periods - but significant differences over some others , even allowing for uncertainty in

the series These are only 300 km separated so this is an interesting indication of changes
in continentality perhaps. I am also interested in extending the high-frequency density
series before 1400 AD , to show earlier volcanoes , even though the spatial coverage is
poor. It would be interesting to see your extreme year series - do you have a preprint of
your paper? I would really like to get support to continue a wider collaboration ,
including other northern long series to produce wide scale integrated series . What is the
latest state of your tree-line reconstruction , for periods earlier than you showed in the
Holocene paper? I am still hoping such support may come again from Europe.
very best wishes
Keith
At 07:28 PM 2/2/04 +0500, you wrote:

> Dear Keith,
> it is very nice to hear from you.
> We live and work in the old way. Stepan has been updated his woody
> vegetation descriptions in the Polar Urals to reconstruct dynamics of
> forest structure near upper timberline for the last century.
> Because of some reasons (sometimes without any reasons) the work on
> constructing Yamal chronology is going not very well. Duration of
> chronology is now 7315 years (7314 BC - AD 2000). The last valuable
> field work has been realized in 2000, when we have collected 370
> subfossil samples. Half of them have been dated. Now I successfully
> collect money for field work (for helicopter rent). I hope this field
> season will be fruitful. Meantime we have analyzed frost- and
> light-ring frequency in Yamal tree rings for the last 2100 years to
> reconstruct extreme events. The later half of this reconstruction, I
> hope, will be published this year in Palaeo3. Now I contracted
> (together with Stepan) to write by June something like textbook on
> tree-ring dating for archeologists (in Russian). Then I'm going to
> return to work on Yamal chronology. It would be pleasure to keep on
> our joint work.
> Best regards
> Rashit Hantemirov
> Institute of Plant and Animal Ecology
> 8 Marta St., 202
> Ekaterinburg, 620144
> Russia
> Tel: +7(3432)51-40-92
> Fax: +7(3432)51-41-61
> E-mail: rashit@ecology.uran.ru
> Monday, February 2, 2004, 1:57:37 PM, you wrote:
> KB> Dear Rashit
> KB> thanks for this - these people ask many questions as they try constantly to
> KB> attack the global warming proponents . I answer sometimes , but it usually
> KB> means they come back with many more questions. All part of science I suppose.
> KB> How are you , and Stepan? I have a student working on trying to refine the
> KB> RCS approach , to allow less trees and reduce bias that comes from using
> KB> only recent data . Hope to get him to test new methods on your and
> KB> Vaganov's data if that is OK with you . I wish to work towards a new
> KB> EuroSiberian series for several millennia at least. Are you still adding
> KB> new data? How are you all?
> KB> Keith

--
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References

1. http://www.cru.uea.ac.uk/cru/people/briffa/
2. http://www.cru.uea.ac.uk/cru/people/briffa/

395. 1075768111.txt
##########

From: Rashit Hantemirov <rashit@ecology.uran.ru>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re[2]: Stephen McIntyre
Date: Mon, 2 Feb 2004 19:28:31 +0500
Reply-to: Rashit Hantemirov <rashit@ecology.uran.ru>

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it is very nice to hear from you.

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Because of some reasons (sometimes without any reasons) the work on constructing Yamal chronology is going not very well. Duration of chronology is now 7315 years (7314 BC - AD 2000). The last valuable field work has been realized in 2000, when we have collected 370 subfossil samples. Half of them have been dated. Now I successfully collect money for field work (for helicopter rent). I hope this field season will be fruitful. Meantime we have analyzed frost- and light-ring frequency in Yamal tree rings for the last 2100 years to reconstruct extreme events. The later half of this reconstruction, I hope, will be published this year in Palaeo3. Now I contracted (together with Stepan) to write by June something like textbook on tree-ring dating for archeologists (in Russian). Then I'm going to return to work on Yamal chronology. It would be pleasure to keep on our joint work.

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KB> Keith

396. 1075836638.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Rashit Hantemirov <rashit@ecology.uran.ru>
Subject: Re[3]: Stephen McIntyre
Date: Tue Feb 3 14:30:38 2004

Rashit
thanks for these - I think you are making magnificent progress , and I wish you the very
best . I would like to see the information you mention if you do not mind . It would be
useful to compare with the long density data.
cheers again
Keith
At 07:20 PM 2/3/04 +0500, you wrote:

Content-Type: text/plain; charset=Windows-1251
X-MIME-Autoconverted: from 8bit to quoted-printable by alanllein.uran.ru id i13EL9co081373
Dear Keith,
attached manuscript concerning frost and light rings has been submitted to Paleo3 special issue (PAGES conference in Moscow in 2002). I'm still waiting for final decision.
Meantime we prepare next version of extremes reconstruction (on the base of Yamal data only) for the last 2100 years using frost, light, missing and very narrow rings. Unfortunately, I could not find time to prepare even draft version of this paper. I can send to you the picture and list of the "extreme" years for this period, if you are interested. Now analysis is going on, little by little. Most probably, we will prepare for publication data for longer reconstruction (up to 4000 years).
As to tree-line reconstruction, we have almost no progress. To get more reliable reconstruction we need more samples from sites northwards of 68°N. In 2002 we have sampled subfossil wood in this area. However, without success (only 30 samples, only 5 of them I was able to date). Now we have in all 30 dated samples from the area to the north of 68°. Attached .pcx files show reconstructions that have been published before in the local publications. Only one correction we can do after 2002 field season, namely that big shift of tree line took place after 2420 BC. Hope I will succeed finally in dating of rest of samples to improve reconstruction.
Best regards
Rashit Hantemirov
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8 Marta St., 202
Ekaterinburg, 620144

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KB> indication of changes in continentality perhaps. I am also interested in
KB> extending the high-frequency density series before 1400 AD , to show
KB> earlier volcanoes , even though the spatial coverage is poor. It would be
KB> interesting to see your extreme year series - do you have a preprint of
KB> your paper? I would really like to get support to continue a wider
KB> collaboration , including other northern long series to produce wide scale
KB> integrated series . What is the latest state of your tree-line
KB> reconstruction , for periods earlier than you showed in the Holocene paper?
KB> I am still hoping such support may come again from Europe.
KB> very best wishes
KB> Keith

--
Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909
Fax: +44-1603-507784
[1]http://www.cru.uea.ac.uk/cru/people/briffa[2]/

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/
2. http://www.cru.uea.ac.uk/cru/people/briffa/

397. 1075931629.txt
##########

From: Rashit Hantemirov <rashit@ecology.uran.ru>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re[4]: Stephen McIntyre
Date: Wed, 4 Feb 2004 16:53:49 +0500
Reply-to: Rashit Hantemirov <rashit@ecology.uran.ru>

Dear Keith,
attached file contains results of analysis of anomalous rings in Yamal
material for 100BC - 2000 AD.

I forgot to inform you about one more thing. We have
organized data bank of Russian tree-ring chronologies.
Unfortunately (for you), in Russian.
http://ipae.uran.ru/dendrochronology/
(and then click on the icon in the bottom (in center) of page).
This databank is made for archeologists and people that need to date
woody constructions and etc. The aim is to give them information about
where and what kind of chronologies there are in Russia. For some
locations chronology is available or links to other databanks, for

others - information only. Site is still filling up. If you are
interested to see you can ask Vladimir Shishov to translate. By the
way, you can remind him about my request to place chronologies of their
lab in this bank.

Best regards

Rashit Hantemirov

Institute of Plant and Animal Ecology
8 Marta St., 202
Ekaterinburg, 620144
Russia
Tel: +7(3432)51-40-92
Fax: +7(3432)51-41-61
E-mail: rashit@ecology.uran.ru

Tuesday, February 3, 2004, 7:30:38 PM, you wrote:

KB> Rashit
KB> thanks for these - I think you are making magnificent progress , and I wish
KB> you the very best . I would like to see the information you mention if you
KB> do not mind . It would be useful to compare with the long density data.
KB> cheers again
KB> Keith
Attachment Converted: "c:\eudora\attach\Extreme2100.pdf"

398. 1076083097.txt
##
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: "Peter H. Gleick" <pgleick@pipeline.com>, Mearns Linda O
<lmearns@ictp.trieste.it>
Subject: Re: MBH Submission (fwd)
Date: Fri Feb 6 10:58:17 2004
Cc: Stephen H Schneider <shs@stanford.edu>, N.W.Arnell@soton.ac.uk,
frtca@fy.chalmers.se, d.camuffo@isac.cnr.it, scohen@sdri.ubc.ca,
pmfearn@inpa.gov.br, jfoley@facstaff.wisc.edu, harvey@geog.utoronto.ca,
ahssec@ansto.gov.au, Thomas.R.Karl@noaa.gov, rwk@ucar.edu, rik.leemans@wur.nl,
diana.liverman@eci.ox.ac.uk, mccarl@tamu.edu, lindam@atd.ucar.edu, rmoss@usgcrp.gov,
ogilvie@spot.colorado.edu, pfister@hist.unibe.ch, barrie.pittock@csiro.au,
pollard@essc.psu.edu, nj.rosenberg@pnl.gov, crosenzweig@giss.nasa.gov,
j.salinger@niwa.co.nz, santer1@llnl.gov, h.j.schellnhuber@uea.ac.uk,
dgvictor@stanford.edu, F.I.Woodward@sheffield.ac.uk, gyohe@wesleyan.edu,
yurganov@hotmail.com

    Dear All,
         So now it seems that we're separating 'providing the code' from 'running
the code'. I
   can't
    see the purpose of one without the other. Even if Mike complies I suspect there
will need
    to be several sessions of interaction, which neither side will be very keen on.
As I said
   before
    I know the code will involve lots of combinations (for different periods with
different
   proxies).
    Also I would expect, knowing the nature of the PC-type regression approach, that
there
   will

be library routines. If the code is sent, there needs to be conditions. We don't want
McIntyre
(MM) to come out and say he can't get it to work after a few days.
So, it is far some simple. I'm still against the code being given out. Mike has made
the
data available. That is all they should need. The method is detailed in the original
paper -
in the online (methods) and also in several other papers Mike has written.
As an aside, Mike is now using a different method from MBH98. Also, as an aside,
whilst we've been deliberating, MM have submitted another comment on MBH98 to another
journal. In this they say they have a program that replicates MBH98 (although it isn't
very convincing that they have it exactly right, as they never show a like for like
comparison) , but
most of the comment goes on about the results being different due to different
combinations of
proxies. The latter isn't surprising.
It might appear they want the code to check whether their version works properly. If
this
is the case, then there are issues of IPR. So, if they get the code, how do we stop them
using it for anything other than this review.
Cheers
Phil
At 11:40 04/02/2004 -0800, Peter H. Gleick wrote:

Yes, excellent point. This should be what we do. Further, we can point out that we've
bent over backward here and provided more than typically necessary in order to satisfy
persistent but inappropriate demands.
Peter
At 08:46 PM 2/4/04 +0100, Mearns Linda O wrote:

Peter et al.,
Thanks for reminding me about the new email list.
My point about the code is still that 'providing the code' can be
interpreted alot of ways. I have thought about this, and imagined if in
one of my larger and more complex projects, I was asked to provide all
code. I could do that just by sending the pieces with a summary file
explaining what each piece was used for. It still theoretically allows
someone to see how coding was done. And I do think that is a far sight
easier than providing stuff that can be run, etc. I am suggesting that
one could do the minimum. Then the point is, one isn't faced with garish
headlines about 'refusal to provide code'. I think it is harder to come
up with a garish headline about 'refusal to provide completely documented
code with appropriate readme files and handholding for running it'.
Linda

Dr. Peter H. Gleick
Director, 2003 MacArthur Fellow
Pacific Institute for Studies in Development, Environment, and Security
654 13th Street
Oakland, California 94612
510 251-1600 phone
510 251-2203 fax

[1]www.worldwater.org (World Water site)
[2]www.pacinst.org (Pacific Institute site)

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References

1. http://www.worldwater.org/
2. http://www.pacinst.org/

399. 1076336623.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: "Tas van Ommen" <tas.van.ommen@utas.edu.au>
Subject: Re: FW: Law Dome O18
Date: Mon Feb 9 09:23:43 2004
Cc: mann@virginia.edu

Dear Tas,
Thanks for the email. Steve McIntyre hasn't contacted me directly about Law Dome (yet), nor about any of the series used in the 1998 Holocene paper or the 2003 GRL one with Mike. I suspect (hope) that he won't. I had some emails with him a few years ago when he wanted to get all the station temperature data we use here in CRU. At that time, I hid behind the fact that some of the data had been received from individuals and not directly from Met Services through the Global Telecommunications Service (GTS) or through GCOS.

I've cc'd Mike on this, just for info. Emails have also been sent to some other paleo people asking for datasets used in 1998 or 2003. Keith Briffa here got one, for example. Here, they have also been in contact with some of Keith's Russian contacts. All seem to relate to trying to get series we've used. In the Russian case, issues relate to the Russian (Rashit Hantemirov) having a paper out with the same series Keith used (for the Yamal Peninsula). Series are different for two reasons. One Keith used the RCS standardization method and secondly Rashit has added some series since Keith got the data a couple of years ago.

I'll just sit tight here and do nothing. Mike will likely do the same, but we'll expect another publication in

the nearish future.
As for the series for LD you sent us, we used it in the paper for Reviews of Geophysics. This paper has had 4 good reviews and we've just sent back a revised version. This will likely get reviewed by 1 or 2 of the same reviewers of the editor, but I think it will come out this year some time. When it does, we will put all the series onto a web site. Hope this is OK with you. It will unlikely be before our summer months.
Cheers
Phil

At 17:56 09/02/2004 +1100, you wrote:

Dear Phil,

What you will find below is (in reverse chronological order) an email interchange between Steve McIntyre and myself. He has been asking for LD data for a while (since your GRL paper came out) and to my chagrin, I have put him off once already, for reasons I spell out below. For your information, I am close to submitting the full LD isotope record, which I hope to present at SCAR Bremen, along with some interesting spectral analyses and comparison to EPICA Dome C.

Anyway, I am aware of McIntyre's controversial history and am trying to handle things in a non-inflammatory way. He seems not to be troubling me over my own delay, but has asked for data that was used in your Holocene paper of 1998. For this, I have referred him to you. I expect he wants to replicate your synthesis, and so he should use the identical data set, and I give you permission to pass on whatever it was I gave you for that work - with the caveat that it is representative of where the LD proxy record was in 1997, not 2004. I leave it to you to decide how to deal with this - you may prefer to ignore the issue, and I would understand.

Let me know if there is anything I can do to assist.

Cheers,
Tas

Dr Tas van Ommen, Principal Research Scientist | Postal Address:
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Antarctic Climate & Ecosystems CRC | Private Bag 80
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[1]www.antcrc.utas.edu.au/~tas | Tasmania 7001
[2]tas.van.ommen@utas.edu.au | Australia

-----Original Message-----
From: Tas van Ommen [[3]mailto:tas.van.ommen@utas.edu.au]
Sent: Monday, 9 February 2004 17:46
To: 'Steve McIntyre'
Subject: RE: Law Dome O18
Dear Stephen,

I suggest you ask Phil Jones for a copy of that older data set. Jones et al cite Morgan and van Ommen 1997, although that data set was heavily smoothed (gaussian of rms=13 years from memory), so the one they show is not a direct version of Morgan and van Ommen 1997. I think that I provided them with a high resolution version, and from their notation, it seems that they are using a November-April subset, but you would have to ask Phil - especially if what you seek is to replicate their analyses. Apart from anything else, our set has been continually in a state of development, which is why I have not wanted to widely circulate it until now. Over this period we have had made new measurements (which improved our layer counted dating and filled the gap that you see in Jones et al.), retreived more cores using better technology and derived a robust gas-tied flow-model that dates the core to 90ky. Now that the new development has ceased, we will soon be releasing the full data set, as I have indicated to you. This is the set I would want to see in wider use, and it is worth noting that it is essentially the same as the portion used by Mann and Jones in their GRL paper in 2003.

All the best,

Tas

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[4]www.antcrc.utas.edu.au/~tas | Tasmania 7001
[5]tas.van.ommen@utas.edu.au | Australia

-----Original Message-----
From: Steve McIntyre [[6]mailto:stephen.mcintyre@utoronto.ca]
Sent: Monday, 9 February 2004 09:46
To: Tas van Ommen
Subject: Re: Law Dome O18
There is a Law Dome O18 data set which was used in Jones et al (Holocene 1998) and printed as a graphic. Is this one available? Regards, Steve McIntyre

----- Original Message -----
From: [7]Tas van Ommen
To: [8]'Steve McIntyre'
Sent: Saturday, February 07, 2004 11:15 PM
Subject: RE: Law Dome O18
Dear Stephen,

The 18O data used in Mann and Jones 2003 was provided as an advance copy in 2003, and you are welcome to have access to it and it will certainly be placed in public archives.

The data in question is part of the full 90 ky isotope record from Law Dome, for which a peer-reviewed dating scale has only recently been published (actually it is in press see van Ommen et al, in press Annals of Glaciology 39 at [9]http://www.antcrc.utas.edu.au/~tas/home/openaccess.html#vanommen04LD1). Now this job is done, I am finalizing a paper that will allow me to release the isotope record more widely.

It is this next paper that controls the timeframe for release to you and archives. While I should await peer review for a release to the archives, I am happy to pass on a copy of the data set to you on an advance basis as soon as the paper is submitted I expect in a couple of months. You will appreciate that at this time of the year, we in the south are in our vacation season, not to mention dealing with our Antarctic Summer field program, so I thank you for your patience. Do check back with me in a while if you dont hear more.

Regards,

Tas

-----Original Message-----
From: Steve McIntyre [[10]mailto:stephen.mcintyre@utoronto.ca]
Sent: Sunday, 8 February 2004 6:29 AM
To: Tas Van Ommen
Subject: Law Dome O18

Dear Dr van Ommen,

some time ago I inquired as to the availability of the O18 data set which was used in Mann and Jones 2003. Is this the same data as was used in Jones et al 1998 (Holocene) . Do you plan to archive this data? Otherwise, I would appreciate an email copy of the data.

Thanks for your consideration.
Stephen McIntyre

Prof. Phil Jones
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University of East Anglia
Norwich Email p.jones@uea.ac.uk
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References

1. http://www.antcrc.utas.edu.au/~tas
2. mailto:tas.van.ommen@utas.edu.au
3. mailto:tas.van.ommen@utas.edu.au
4. http://www.antcrc.utas.edu.au/~tas
5. mailto:tas.van.ommen@utas.edu.au
6. mailto:stephen.mcintyre@utoronto.ca
7. mailto:tas.van.ommen@utas.edu.au
8. mailto:stephen.mcintyre@utoronto.ca
9. http://www.antcrc.utas.edu.au/~tas/home/openaccess.html#vanommen04LD1
10. mailto:stephen.mcintyre@utoronto.ca

400. 1076359809.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Fw: Law Dome O18
Date: Mon Feb 9 15:50:09 2004

Mike,
These were two simple ones to provide. Also Tas told him I had one of them. I guess these are the ones that aren't available on web sites.
Anyway, it is done now. If he starts asking for them in dribs and drabs, I'll baulk at that.
Ben waded in with very positive comments re the CC issue. Steve's going to find it very hard to ask you to send the code. Those that say on the CC board that you should send the code, have little idea what is involved. Most are on the social science side.
Cheers
Phil
At 10:19 09/02/2004 -0500, you wrote:

HI Phil,
Personally, I wouldn't send him anything. I have no idea what he's up to, but you can be sure it falls into the "no good" category.
There are a few series from our '03 paper that he won't have--these include the latest Jacoby and D'Arrigo, which I digitized from their publication (they haven't made it publicly available) and the extended western North American series, which they wouldn't be able to reproduce without following exactly the procedure described in our '99 GRL paper to remove the estimated non-climatic component.
I would not give them *anything*. I would not respond or even acknowledge receipt of their emails. There is no reason to give them any data, in my opinion, and I think we do so at our own peril!
talk to you later,
mike
At 02:46 PM 2/9/2004 +0000, Phil Jones wrote:

Mike,
FYI. Sent him the two series - the as received versions. Wonder what he's up to?
Why these two series ? Used a lot more in the 1998 paper. Didn't want the Alerce series.
Must already have the Tassy series from Ed. I know Ed has a more recent series than we used in 1998. Got this for the 2003 work.
Cheers
Phil

From: "Steve McIntyre" <stephen.mcintyre@utoronto.ca>
To: "Phil Jones" <p.jones@uea.ac.uk>
Subject: Fw: Law Dome O18
Date: Mon, 9 Feb 2004 08:05:23 -0500
X-Mailer: Microsoft Outlook Express 6.00.2800.1158
X-Authentication-Info: Submitted using SMTP AUTH LOGIN at fep04-mail.bloor.is.net.cable.rogers.com from [65.49.25.138] using ID <nmcintyre77@rogers.com> at Mon, 9 Feb 2004 08:02:13 -0500
Dear Phil,
Tas van Ommen has refered me to you for the version of his dataset that you used in Jones et al Holocene 1998 and I would appreicate a copy. I would also appreciate a copy of the Lenca series used in this study. Regards, Steve McIntyre

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Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903
e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[1]http://www.evsc.virginia.edu/faculty/people/mann.shtml

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References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml

401. 1077200902.txt
##

From: Phil Jones <p.jones@uea.ac.uk>
To: Ben Santer <santer1@llnl.gov>
Subject: Pete Mayes
Date: Thu Feb 19 09:28:22 2004

Ben,

Every now and then - generally around an England game (probably now as we've just drawn with Portugal) or lamenting the fall of Liverpool, I get emails and sometimes phone calls from Pete Mayes !! Pete wants to get back into climate change and do some comparisons between real world data and some models. It is a pity he wasn't this keen, when he first went to the US !
Anyway I suggested he contact you. He has but he's not got a reply. I guess you're busy and/or don't know how to reply. I'm sure he doesn't know what he really wants. I gave him some references etc to look over and your name/email - so SORRY !!!!
I guess I'll see you just after Easter. Will you be here for the HC meeting as well as IDAG?
It will be good to see Tom in Oxford - he should liven up the IDAG discussions.
Hope all is well with you and Nick !
Cheers
Phil
PS I see Steve has replied to MM re the MBH review. This nearly got out of hand - it still could. Appalling paper in GRL in the Feb04 issue - Mike Mann's written a response. Clearly another case of the GRL editor's having no idea of the science. Who in their right mind would accept that for publication. Nowhere on the CRU site does it say that HadCRUT2v is the IPCC data. According to the HC the IPCC data is the OA version HadCRUT - no v, no 2.
The data is on the HC web site. There is a link to it from the CRU site. When getting data from the CRU site we ask people to refer to some of the papers and to use the dataset names. Soon et al didn't do either. Paper attached as I have it.
Just had a paper accepted by Reviews of Geophysics with Mike Mann on the climate of the last 2k years. Expecting flak for this, but it had 4 very positive reviews.
For some inane reason I put my name forward to do the chapter on atmospheric obs. for AR4. Hope I don't get picked.

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---

402. 1077829152.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Crap Papers
Date: Thu Feb 26 15:59:12 2004

Mike,

Just agreed to review a paper for GRL - it is absolute rubbish. It is having a go at the CRU temperature data - not the latest vesion, but the one you used in MBH98 !! We added lots of data in for the region this person says has Urban Warming ! So easy review to do.

Sent Ben the Soon et al. paper and he wonders who reviews these sorts of things. Says GRL hasn't a clue with editors or reviewers. By chance they seem to have got the right person with the one just received.

Can I ask you something in CONFIDENCE - don't email around, especially not to Keith and Tim here. Have you reviewed any papers recently for Science that say that MBH98 and MJ03 have underestimated variability in the millennial record - from models or from some low-freq proxy data. Just a yes or no will do. Tim is reviewing them - I want to make sure he takes my comments on board, but he wants to be squeaky clean with discussing them with others. So forget this email when you reply.

Cheers

Phil

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403. 1078236401.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Ben Santer <santer1@llnl.gov>
Subject: Re: [Fwd: More PCM-ERA40 comparisons]
Date: Tue Mar 2 09:06:41 2004

Ben,

Thanks for the plots and keeping me up to date. The ERA-40/CRU comparisons are quite interesting. I'm hopeful Adrian will write up a summary for publication in

addition
to an ECMWF report.
This sort of thing is important wrt IPCC and also papers such as Kalnay and Cai.
I'm also working with Russ Vose and others at NCDC to get a comparison of CRU/GHCN
and NASA datasets in GRL. NCDC have used their first difference technique with CRU
data. Differences are very, very small due to data and the technique doesn't matter much
either. All seems to boil down to how the global average is defined. Calculated as one
domain as NCDC (and until recently the HC as well) want to do it, it is biased to the NH.
If you do it the CRU way (G=0.5(NH+SH)) then it looks much more like an OA version
of HadCRUT2v that the HC have just produced. Been saying this for years as has Tom,
so no surprises. Finally got the HC to realise it, now just need to convince NCDC.
NCDC will also have a new 5 by 5 deg gridded dataset of Tx and Tn soon, right up to
the present. Need to compare this with ERA-40.
Cheers
Phil

At 18:46 01/03/2004 -0800, you wrote:

Dear Phil,
Here are the PCM/ERA-40 2m temperature comparisons that I mentioned in my email
to Adrian....
Cheers,
Ben
--

PCMDI HAS MOVED TO A NEW BUILDING. NOTE CHANGE OF MAIL CODE!
Benjamin D. Santer
Program for Climate Model Diagnosis and Intercomparison
Lawrence Livermore National Laboratory
P.O. Box 808, Mail Stop L-103
Livermore, CA 94550, U.S.A.
Tel: (925) 422-7638
FAX: (925) 422-7675
email: santer1@llnl.gov

---Return-Path:
<santer1@llnl.gov>
Received: from smtp-3.llnl.gov ([128.115.41.83] verified)
by popcorn.llnl.gov (CommuniGate Pro SMTP 4.0.6)
with ESMTP id 34392268 for santer1@popgun.llnl.gov; Thu, 26 Feb 2004 18:00:27 -0800
Received: from pierce.llnl.gov (localhost [127.0.0.1])
by smtp-3.llnl.gov (8.12.3p2-20030917/8.12.3/LLNL evision: 1.13 $) with ESMTP id
i1R20OE6003673
for <santer1@popgun.llnl.gov>; Thu, 26 Feb 2004 18:00:24 -0800 (PST)
Received: from smtp-3.llnl.gov (smtp-3.llnl.gov [128.115.41.83])
by pierce.llnl.gov (8.12.3p2-20030917/8.12.3/LLNL evision: 1.5 $) with ESMTP id
i1R20NkO028603
for <santer1@llnl.gov>; Thu, 26 Feb 2004 18:00:23 -0800 (PST)
Received: from popcorn.llnl.gov (localhost [127.0.0.1])

by smtp-3.llnl.gov (8.12.3p2-20030917/8.12.3/LLNL evision: 1.13 $) with ESMTP id
i1R208Af003594;
Thu, 26 Feb 2004 18:00:09 -0800 (PST)
Received: from [128.115.57.176] (account santer1 HELO llnl.gov)
by popcorn.llnl.gov (CommuniGate Pro SMTP 4.0.6)
with ESMTP id 34392176; Thu, 26 Feb 2004 18:00:08 -0800
Sender: bsanter@smtp-3.llnl.gov
Message-ID: <403EA554.20D01DFD@llnl.gov>
Date: Thu, 26 Feb 2004 18:03:00 -0800
From: Ben Santer <santer1@llnl.gov>
Organization: LLNL
X-Mailer: Mozilla 4.79 [en] (X11; U; Linux 2.4.18-14 i686)
X-Accept-Language: en
MIME-Version: 1.0
To: Adrian.Simmons@ecmwf.int, wmw@ucar.edu, meehl@ucar.edu, wigley@ucar.edu,
ammann@ucar.edu
Subject: More PCM-ERA40 comparisons
References: <403B1219.4060905@ecmwf.int>
Content-Type: multipart/mixed;
boundary="------------7A520C5A8CA7CE01BA097390"
X-Mozilla-Status2: 00000000

Dear Adrian,

Thanks very much for sending me your comparison of surface air temperature changes in CRU and ERA-40. I've been looking at a related issue - the correspondence between 2m temperature changes in ERA-40 and PCM.

Here's the background to this work. Increasingly, there is some interest in the problem of identifying anthropogenic climate change at regional scales. I have to give a brief talk on this subject tomorrow. In preparing for this talk, I decided that it would be useful to show how signal and noise change as a function of spatial scale. I looked at the behavior of 2m temperature in the four individual realizations of the PCM "ALL forcings" experiment (the same experiment that we analysed in our joint Nature paper). For each realization, I computed spatial averages over the globe, the Northern Hemisphere, and the western United States (30-50N, 126W-114W). These spatial averages were then expressed as anomalies relative to climatological monthly means over 1979-1999.

The orange shading in the three panels of the figure entitled "tas_tseries3.ps" is a measure of the between-realization variability in PCM. The envelope is simply the range (during any given month) between the maximum and minimum values of the four realizations. This range was then low-pass filtered. The solid red is the low-pass filtered ensemble mean.

To facilitate comparison with PCM data, I've defined 2m temperature anomalies in ERA-40 in the same way (i.e., relative to climatological monthly means over 1979-1999), and have used the same low-pass filter. One can then ask whether the 2m temperature changes in ERA-40 are consistent with those in PCM - in other words, are they encompassed by PCM's envelope of possible climate responses to combined anthropogenic and natural forcing?

They are. Surprisingly, this consistency occurs not only at the global-mean level, but also for the NH and western U.S. For the global-mean and the NH, the ERA-40 2m temperature changes are outside PCM's envelope of 2m temperature changes during the first 5-10 years of the reanalysis. After the late 1960s, however, the ERA-40 2m temperature changes are entirely consistent with those in PCM. Over the western U.S., 2m temperature changes in PCM and ERA-40 are consistent throughout the reanalysis period.

Such qualitative consistency, while interesting, is no substitute for formal, pattern-based fingerprint detection studies at global, hemispheric, and regional scales. For example, an overestimate of the regional-scale variability of 2m temperature by PCM could explain why PCM's 2m temperature changes over the

western U.S. fully encompass the ERA-40 result (see panel C). On the other hand,
there is some real similarity in the low-frequency component of the 2m temperature changes in ERA-40 and PCM (look at the similar responses to Agung, Chichon, and Pinatubo in panel B!)
The bottom line is that PCM's 2m temperature changes are reasonably consistent with those in ERA-40, even at sub-global spatial scales. This suggests that formal regional-scale detection work might be useful. If you are interested, perhaps we could collaborate on such work. A collaboration would also involve the PCM group at NCAR (to whom I'm copying this email).
The second figure that I've appended shows the global-mean changes in synthetic MSU channel 2 temperatures in PCM and ERA-40. The message is pretty much the same as for 2m temperatures: PCM's "envelope" of possible changes in tropospheric temperatures largely encompasses the ERA-40 results, except during a few large El Nino and La Nina events. Once again, there is surprising similarity in the low-frequency component of the model and reanalysis T2 changes.
It would be fun to take these simple comparisons a little further!
With best regards,
Ben
--

PCMDI HAS MOVED TO A NEW BUILDING. NOTE CHANGE OF MAIL CODE!
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404. 1079108576.txt
##

From: Kevin Trenberth <trenbert@cgd.ucar.edu>
To: tom crowley <tcrowley@duke.edu>
Subject: Re: REQUEST FOR INFORMATION ON CLIMATE CHANGE AND HUMAN ATTRIBUTIONS
Date: Fri, 12 Mar 2004 11:22:56 -0700
Cc: Chick Keller <cfk@lanl.gov>, Richard Somerville <rsomerville@ucsd.edu>, Tom Wigley <wigley@cgd.ucar.edu>, "Howard Hanson, LDRD" <hph@lanl.gov>, "James E. Hansen" <jhansen@giss.nasa.gov>, Michael Schlesinger <schlesin@atmos.uiuc.edu>, Phil Jones <p.jones@uea.ac.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Mike MacCracken <mmaccrac@comcast.net>, Ben Santer <santer1@llnl.gov>, thompson.4@osu.edu, rbradley@geo.umass.edu, mhughes@ltrr.arizona.edu, Keith Briffa <k.briffa@uea.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>

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I agree with Tom: I sent you (without copying others) a whole host of material..
Kevin

tom crowley wrote:

> For goodness sakes, I don't know where to start - let me just make one
> point with respect to solar - solar projects onto the GHG signal in
> the 20th c. so solar cannot be distinguished during that time. if one
> were to independently correlate solar and GHG with temp. since 1750,
> solar would "explain" about 75% of the variance, GHG about 70% - a
> spectacular 140% of the variance explained!
>
> the only way to evaluate solar is to look at intervals when GHG was
> not changing and solar was - the preanthropogenic interval - perhaps
> the most comprehensive evaluation of the solar effect is in the
> attached paper, where it is quite clear that solar effect is either
> negligible or just barely significant, ie., 5-10% of the decadally
> scaled variance.
>
> with respect to the MWP all you have to do is plot the data up and
> compile them - the numbers don't work out as being warmer than the
> present - at best approaching or slightly exceeding mid-20th c. the
> reason is that is was warm at different times. Soon and Baliunas of
> course never showed this - but if you actually look at the damn data
> and plot up, the same answer as I stated above keeps showing up, over
> and over.
>
> with respect to UAH, there are now two other reconstructions that show
> otherwise.
>
> enough, this is like trying to convert someone with one religion to
> another.
>
> tom
>
> Chick Keller wrote:
>
>> Richard and Friends,
>>
>> thanks for the point of view. I'll put some of this into my
>> presentation.
>>
>> However, it won't wash when facing critics head-on.
>>
>> Their latest arguments are more subtle. Their main point is that
>> their counter information hangs together into a logically coherent
>> picture.
>>
>> Models: no real finger print that distinguishes AGHG forcings from
>> others!   Models using AGHG forcings predict warming is function of
>> latitude yet the Arctic is hardly warming (north of ~^65°N), and high
>> latitude Antarctic (excepting for the peninsula) is actually cooling
>> slightly.
>>
>> Models: As you say need AGHG forcings to simulate last 30 years of
>> observed warming. But, they counter, UAH satellite reductions show
>> no such warming so don't need AGHG forcing (or at least don't need
>> effects of positive feedbacks and just increases in AGHGs don't cause
>> so much warming).
>>
>> Solar forcing--not able to generate last 30 years of observed
>> warming. Same counter as last one--"See, they say, no increased
>> solar in last 25 years is consistent with no warming!!
>>
>> Also, since no warming since 1945, MWP most likely to have been as

>> warm as now and thus sun can indeed explain (with proper lags)
>> observed warming thus far.
>>
>> Their model--climate varies depending on solar activity. all
>> observations are consistent with this.
>>
>> Models predict that any surface warming will be seen in the
>> troposphere. Since UAH satellite reduction shows no such warming--1.
>> models are wrong and/or no warming at surface just lousy observations.
>> 2. If no warming at surface in last 30 years AGHG forcing predictions
>> by models is incorrect probably due to poor cloud/water vapor
>> modeling--no positive feedbacks to speak of.
>>
>> Sooooo, you can say all you want that all the prestigious societies
>> and folks say it's AGHGs, but they've been bamboozled by a few of
>> elitist scientists. As long as satellites show no recent warming,
>> the entire AGHG hypothesis collapses, not because multi-atomic
>> molecules don't cause the atmosphere to be more opaque, but because
>> there are no positive feedbacks which the models need to get the
>> "right" answer.
>>
>> So, what I need is strong evidence that the surface record is indeed
>> correct (UHI effect is small, and marine boundary layer approximation
>> is correct).
>>
>> Now, Richard, toss in large effects of land use changes and of black
>> soot forcing changing earth's albedo, and you now have additional
>> forcings which may be causing warming but can't be countered by
>> reducing AGHGs.
>>
>> Soooo, it still ain't all that easy to convince an audience that the
>> Singer's of this world aren't on to at least part of the problem.
>>
>> AND keep in mind that increased CO2 is good for us--more agriculture,
>> etc.
>>
>> Nope it just ain't that easy. So any information--graphics, etc on
>> these issues will be greatly appreciated.
>>
>> Regards to all,
>> chick
>>
>>
>> Hi Chick and friends,
>>
>> Good to hear from you, Chick. I'm busy, like all of us, and
>> responding to Singer is not my cup of tea, so I'm glad you and others
>> are willing. I hate to be in the same room with him, frankly. He's
>> a third-rate scientist and is ethically challenged, to say the least.
>>
>> From others on your email list, I am sure you will receive tons of
>> useful information. However, I think your entire basic strategy for
>> confronting Singer might not be optimal. Sometimes the most pressing
>> issues in the research community, or the most interesting questions
>> scientifically, are not necessarily the best ways to carry on the
>> public conversation. I am thinking in particular of your statement:
>>
>> "Perhaps the most important is that satellites don't show much
>> warming since 1979 and disagree substantially with the surface
>> record, which must then be incorrect. Were we able to resolve this
>> conundrum, I think most of the other objections to human generated
>> climate change would lose their credibility."
>>

>> For what it's worth, here's my take on your approach. I
>> respectfully disagree with you that hammering away on reconciling the
>> MSU data with radiosonde and surface data is the right way to go in
>> dealing with the Fred Singers of the world. Even though much of the
>> differences may now be apparently explained, it's still a terribly
>> messy job. The satellite system wasn't designed to measure
>> tropospheric temperatures, the calibration and orbital decay and
>> retrieval algorithm and all the other technical issues are ugly, and
>> nobody knows how much the lower stratospheric cooling ought to have
>> infected the upper troposphere, among other points one might make.
>>
>> No matter what one does on trying to make the MSU data tell us a
>> clean story, there are remaining serious uncertainties.   That's
>> basically what the NAS/NRC study chaired by Mike Wallace concluded,
>> and it's still true, in my view. Plus the data record is so short.
>> In addition, as you say, you are retired, and research on these
>> things is not what you have first-person experience with, so when you
>> try to study up on the latest published results, you're at a
>> disadvantage compared with the Singers of the world, whose full-time
>> job is to cherry-pick the literature for evidence to support their
>> preconceived positions.
>>
>> One of the tactics of the skeptics is to create the impression among
>> nonscientists, especially journalists, that the entire science of
>> climate change rests on the flimsy foundation of one or two lines of
>> evidence, so that casting doubt on that foundation ought to bring
>> down the entire structure. For temperature, that approach is clearly
>> behind the attacks on the "hockey stick" curve over the last 1,000
>> years or the satellite vs. in situ differences over the last 25
>> years. Refuting the errors of the papers by Soon and Baliunas or by
>> McIntyre and Mckitrick doesn't faze these people. They just shift
>> their ground and produce another erroneous attack. Their goal is not
>> to advance the science, but to perpetuate the appearance of
>> controversy and doubt.
>>
>> I don't think the skeptics should be allowed to choose the
>> battlefield, and I certainly don't think the issue of whether
>> anthropogenic influences are a serious concern should be settled by
>> looking at any single data set. I do think the IPCC TAR was right to
>> stress that you simply can't plausibly make GCMs replicate the
>> instrumental record without including GHGs (and aerosols). I also
>> think the recent AGU and AMS public statements, which you will
>> doubtless find on their web sites, are right on target. Many of us
>> were pleasantly surprised that our leading scientific societies have
>> recently adopted such strong statements as to the reality and
>> seriousness of anthropogenic climate change. There really is a
>> scientific consensus, and it cannot be refuted or disproved by
>> attacking any single data set.
>>
>> I also think people need to come to understand that the scientific
>> uncertainties work both ways. We don't understand cloud feedbacks.
>> We don't understand air-sea interactions. We don't understand
>> aerosol indirect effects. The list is long. Singer will say that
>> uncertainties like these mean models lack veracity and can safely be
>> ignored. What seems highly unlikely to me is that each of these
>> uncertainties is going to make the climate system more robust against
>> change. It is just as likely a priori that a poorly understood bit
>> of physics might be a positive as a negative feedback. Meanwhile,
>> the climate system overall is in fact behaving in a manner consistent
>> with the GCM predictions. I have often wondered how our medical
>> colleagues manage to escape the trap of having their entire science
>> dismissed because there are uncured diseases and other remaining
>> uncertainties. Maybe we can learn from the physicians.

>>
>> People on airplanes, when they find out what I do for a living,
>> usually ask me if I "believe in" global warming. It's not religion,
>> of course. What I actually tend to believe in, if they really wanted
>> to try to understand, is quantum mechanics. CO2 and CH4 and all
>> those other interesting trace gases have more than two atoms, and
>> that fact simply has inescapable consequences. You just can't keep
>> adding those GHG molecules indefinitely without making the atmosphere
>> significantly more opaque in the IR.   The "debates" in the reputable
>> research community are all quantitative. If skeptics don't worry
>> about doubling, they ought to be pressed to tell us why they are
>> unconcerned about tripling or quadrupling or worse. That's where the
>> planet is headed. The fact that remote sensing and model building
>> are hard work, and that much remains to be done, shouldn't be allowed
>> to obscure the basic obvious facts.
>>
>> Bonne chance et bon courage,
>>
>> Richard
>
>

--
****
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P. O. Box 3000,   (303) 497 1318
Boulder, CO 80307   (303) 497 1333 (fax)

Street address: 1850 Table Mesa Drive, Boulder, CO 80303

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405. 1079384474.txt
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From: Phil Jones <p.jones@uea.ac.uk>
To: Jorge Sánchez Sesma <jsanchez@tlaloc.imta.mx>
Subject: Re: Global Temperature
Date: Mon Mar 15 16:01:14 2004

    Dear Jorge,
        Happy for you to use me in an additional attempt tp get some Mexican support
    to come to CRU next year. What exactly do you need? Send me an example of
    what you want? Life is very busy here at the moment as I'll be away for several
    meetings over the next 6 weeks and I must prepare some material for most of
them.
        GKSS is just one model and it is a model, so there is no need for it to be
correct.
    I am also aware that Ed Cook is revising the ECS curve in a paper he's
submitting
    to Quaternary Science Reviews.
        Remember that if ECS (and GKSS) are correct then the climate is more
sensitive
    to external forcing (the factors that cause past changes/variability). If the
climate is
    more sensitive then the likely changes in the future will be greater. The curves
that
    we've produced here (and also Mike Mann's) suggest a climate sensitivity of

about
2.5 deg C for a CO2 doubling. Getting volcanic forcing right in the past (along with
solar)
are crucial in any study.
Cheers
Phil
At 12:22 12/03/2004 -0600, you wrote:

Dear Dr. Jones:
I am very happy because I went to a Workshop in Kona Hawaii (with support of NASA-CRCES after to gain a contest with a review paper about global temperature reconstructions, it was a different version of the paper that you have read). There I met with Dr. Michael Mann. Mann was very kind with me, however when he did know my work he changed his attitude. I met there also Dr. Hans von Starch who presented a global temperature reconstructions with a AOCGCM with natural and anthropogenic forcings. His results agree more or less with ECS, and my results. i am in contact with the GKSS group in order to compare and share information.
However, the key point of my studies, as you have pointed out, is to justify that the background Ice Acidity (without volcanic activity) from polar caps could be considered as a proxy. I have contacted Dr. Hammer and Dr. Crowley to have information and advice.
In order continue this kind of studies I would like to propose you again (as we have tried last year) to ask support the the AMC (Mexican Academy of Sciences) to support a visit to CRU-UEA next year to continue my work, with your help and advice, about global temperature for the Holocene. I will need only an official invitation for my visit. It would be in March 2005 for 3 or 4 weeks.
Also, I am asking support to travel to Japan this year (this fall), however I would like to stop in England a week, in order to visit CRU-UEA and to continue our collaboration.
I would like to know your oppinion,
cheers,
Jorge
Jorge Sánchez-Sesma
Instituto Mexicano de Tecnología del Agua
Subcoordinación de Hidrometeorología
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pagina: [1]http://nimbus.imta.mx

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References

1. http://nimbus.imta.mx/

406. 1080257046.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Ben Santer <santer1@llnl.gov>
Subject: Re: [Fwd: More PCM-ERA40 comparisons]
Date: Thu Mar 25 18:24:06 2004

Ben,
Thanks I picked it up last Friday. See you after Easter.
Cheers
Phil
At 09:22 25/03/2004 -0800, you wrote:

Dear Phil,
Our exchange with Roger Pielke finally appeared in Science (copy appended). I'm
glad I've gotten this particular albatross off my neck. Timo et al. have
already
been circulating this stuff to all and sundry.....
See you in a few weeks' time,
Cheers,
Ben
Phil Jones wrote:
>
> Ben,
> Right decision ! She sent me an email to review a paper two weeks ago.
> Said I didn't
> have time until May. I'll continue to say that now.
> See you just after Easter. Have a good short break, as you'll have to
> miss part of it
> to come to London and IDAG.
>
> Cheers
> Phil
>
> At 19:06 22/03/2004 -0800, you wrote:
> >Dear Phil,
> >
> >I just don't have much luck with the Heikes of this world. Heike L.
> >rejected our
> >Nature paper on the analysis of changes in tropopause height and
> >equivalent MSU
> >temperatures in ERA-40. She took six weeks to make this decision, and didn't
> >even send the paper out for review! Very disappointing. I doubt whether
> >I'll be
> >submitting any papers to Nature in the next few years. We're now revising
the
> >erstwhile Nature paper for submission to Journal of Climate, and I hope to
> >have
> >it sent off before I leave for the U.K. on April 11th.
> >
> >I look forward to seeing you at the SRG meeting. Hope everything is well
with
> >you, Ruth, Hannah, and Matthew.
> >
> >Best regards,
> >
> >Ben
>
>==========
==
>
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407. 1080742144.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: have you seen this?
Date: Wed Mar 31 09:09:04 2004

Mike,
Yes, but not had a chance to read it yet. Too much else going on. Ed has a paper
reworking Esper et al. as you'll know. If you're going to Tucson, I suggest you talk to
Keith about it then - don't email him as he's too busy preparing to go and marking essays.
Jan is in one of our EU projects. Seems that Keith thinks Jan is reinventing a lot of
Keith's
work, renamed the RCS method and much more. Jan doesn't always take in what is in
the literature even though he purports to read it. He's now looking at homogenization
techniques for temperature to check the Siberian temperature data. We keep telling him the
decline is also in N. Europe, N. America (where we use all the recently homogenized
Canadian data). The decline may be slightly larger in Siberia, but it is elsewhere as
well.
Also Siberia is one of the worst places to look at homogeneity, as the stations aren't
that
close together (as they are in Fennoscandia and most of Canada) and also the temperature

varies an awful lot from year to year.
Recently rejected two papers (one for JGR and for GRL) from people saying CRU has it
wrong over Siberia. Went to town in both reviews, hopefully successfully. If either
appears
I will be very surprised, but you never know with GRL.
Cheers
Phil
Cheers
Phil
At 11:20 30/03/2004 -0500, you wrote:

Phil,
Have you seen this piece of crap by Esper?
The JGR paper, which Scott is supposed to be finalizing, demonstrates quite convincingly
that the greater amplitude of Esper et al is due to spatial and seasonal sampling,
mike

Professor Michael E. Mann
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[1]http://www.evsc.virginia.edu/faculty/people/mann.shtml

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References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml

408. 1083962092.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Scott Rutherford <srutherford@rwu.edu>
Subject: RoG Data
Date: Fri May 7 16:34:52 2004
Cc: "Michael E. Mann" <mann@virginia.edu>

Scott and Mike,

It's been a long week catching up from 3 weeks away. Getting another email from
McIntyre asking me for paleo data series I don't have (I'm not going to reply, by the way
even though he calls me Phil and other emails he sends me are to Dr Crowley and Dr.
Briffa who've also not replied) reminded me that I agreed with Mike to put together as

many of the series from the RoG paper onto a page on the CRU web site.
So, with this in mind, can you send me the data for the various plots. I checked the
paper and Fig 1 doesn't need anything, so this leave Figs 3 (on the boreholes), 5 (with
the various NH/SH/Global series) and 8 (with all the various model runs).
Figure 3 should be trivial as borehole data are only every 50 years. For the other
2 plots
I'm after the annual values of each series and the smoothed ones that get plotted. Hope
this
won't take too long to do. I'm going to send emails to a few people to check we can make
the
data available (mainly the modellers, but also Tas van Ommen).
Cheers
Phil

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409. 1083962601.txt
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From: Phil Jones <p.jones@uea.ac.uk>
To: "Tas van Ommen" <tas.van.ommen@utas.edu.au>, Caspar Ammann <ammann@ucar.edu>,
Subject: RoG paper
Date: Fri May 7 16:43:21 2004

Dear Tas and Caspar,

Attached is the proof version of the RoG paper with Mike Mann. This is about 99.99%
the final one. Mike and I sent back a few small changes to AGU a month or so ago. Keep
this to yourself for a while yet - I would expect the paper out sometime in the
July/August
period.
Many of us in the paleo field get requests from skeptics (mainly a guy called
Steve McIntyre in Canada) asking us for series. Mike and I are not sending anything,
partly because we don't have some of the series he wants, also partly as we've got the
data
through contacts like you, but mostly because he'll distort and misuse them.
Despite this, Mike and I would like to make as many of the series we've used in the
RoG
plots available from the CRU web page. Can we do this with the series we've got from
you? You don't have to do anything, except to reply yes or no !
Cheers
Phil

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410. 1084017554.txt
##########

From: f037 <M.Hulme@uea.ac.uk>
To: Aiguo Dai <adai@cgd.ucar.edu>
Subject: denial or delusion? ... Aiguo's response
Date: Sat, 8 May 2004 07:59:14 +0100
Cc: <jprospero@rsmas.miami.edu>, <m.hulme@uea.ac.uk>, <p.jones@uea.ac.uk>, <plamb@ou.edu>, <trenbert@cgd.ucar.edu>

Dear Aiguo,

You've done a great job in putting this together so quickly and clearly. I have a couple of additional comments to make on it, but can't do so until Tuesday. You (we?) might also like to think of the reply being multi-authored, including Phil, Pete, Kevin, Joe and myself.

I must say that when I first read this paper a couple of weeks ago I wrote it off as so bad (so, so bad) that it didn't even deserve a response. To pretend that the Sahel drought didn't happen (i.e., a pure artifact of wrongful use of rainfall data) is the most astounding assertion, almost on a par with holocaust denial. Try putting that proposition to the millions of inhabitants of the Sahel in the 1970s, 1980s and 1990s, many of whom died as a direct consequence and whose livelihoods were devastated. Adrian Chappell may never have visited the region, but I know Clive Agnew has (many times) - and he should know better. I did my PhD research in the region in the early 1980s and I know exactly what the rainfall conditions were like and how much oridinary people suffered as a consequence. My PhD was on rainfall variability and local water supplies in Sudan and I visited and talked to many villagers in the region.

Anyway, Phil first suggested that a corrective reply was needed and I can see the value of doing so, especially with IPCC AR4 approaching. It just seems to me such a shame that such poor science is being done by some people - in this case I don't think there is a deeper motive on the part of Chappell and Agnew than pure delusion and incompetence - and, worse, that a journal like IJC will publish it.

Thanks again for your efforts,

Mike

>===== Original Message From Aiguo Dai <adai@cgd.ucar.edu> =====
>Dear All,
>
>Soon after I sent out my last email, I quickly realized that there is
>another fundamental error in their rainfall model eq.(1): the regional
>station numbers na and nb should be replaced with regional areas. This
>can be seen clearly in the following example: suppose region a has only
>one station whose long-term mean rainfall happens to be the same as

>region a's mean, and region b has 100 stations. Then their model would
>give the completely wrong estimate of rainfall for region (a+b), while
>the area-weighted version would still work. This is an obvious error, but
>it apparently could be easily overlooked. Their model seems to be
>originated from their incorrect perception that regional rainfall has
>been traditionally derived using the simple arithmetic mean of all station
>data. After reading the leader author's response to Joe's comments, I
>could not believe that they still think previous analyses are simpler than
>theirs!
>
>I also forgot to point out in my earlier draft the fact that even if their
>modelled time series were a reasonable proxy of Sahel rainfall, their
>results would still have had little implications to previous analyses of
>Sahel rainfall. This is because their analysis maximized the effects of
>changing station networks by the design of their model and by choosing
>the boundary of the two sub-Sahel region at 6deg.W, whereas in most previous
>analyses these effects were minimized by area-weighted averaging (Jones and
>Hulme, 1996).
>
>Sorry for the overlook of these issues in my earlier email.
>
>Regards,
>
>--Aiguo Dai
>
>
>
>
>
>> Dear All,
>>
>> I was asked by Kevin to work out a rebuttal to Chappell and Agnew
>> (2004). After reading
>> it a couple of times, I found the main reason why they came to their
>> results: they devised a
>> Sahel rainfall model (eq. 1) with a necessary condition that the
>> constants a and b
>> represent the mean rainfall for the west and east part of the Sahel.
>> However, later in their
>> paper, they estimated a and b by a non-linear least-squares fitting to
>> observed rainfall
>> data, and their a (=973mm) and b (=142mm) are nowhere near the actural
>> mean rainfall
>> for these sub-Sahel regions (~645.5 mm and 471.2mm). In essense, their
>> rainfall model
>> and thus their modelled rainfall time series are no longer relevant to
>> Sahel rainfall!
>>
>> I have seen many bad papers, but this one is the worst of all, not only
>> because they
>> misled the reader with their model (intentionally or unintentionally),
>> but also because they
>> made all kinds of unfounded pure speculations about the implications of
>> their results.
>>
>> I did some quick analyses using data extracted from the update GHVN2 and
>> wrote a
>> comment paper, which is attached as Word file. Any comments will be
>> appreciated.
>>
>> Regards,
>>
>> Aiguo

>>
>> Phil Jones wrote:
>>
>>>
>>> Dear All,
>>>   Several emails today. Kevin's encouraging Aiguo Dai to write a
>>> response as well,
>>> so it might be worth some co-ordination. 2 responses might be better
>>> than one, though, so I'll
>>> leave it up to you.
>>>   They have dug themselves into a bigger hole in their response to
>>> Joe. Joe's assessment
>>> of their reasoning is exactly right. Also you can't write a paper
>>> saying an analysis is flawed and
>>> then say we don't dispute the local evidence for drought ! This is
>>> naive in the extreme and
>>> dumb. I've heard this excuse several times in the past with other
>>> contentious papers.
>>>   The one problem there might be in a response is getting a quick
>>> turnaround with IJC.
>>> With the response a strongly worded letter should go to the editor
>>> (Glenn McGregor)
>>> requesting a fast-track review. The journal does this. As Kevin says
>>> any response short
>>> be short and to the point.
>>>
>>> Cheers
>>> Phil
>>>
>>>
>>> At 18:17 06/05/2004 -0400, Joseph M. Prospero wrote:
>>>
>>>> From: "A.Chappell" <A.Chappell@salford.ac.uk>
>>>> To: "Joseph M. Prospero" <jprospero@rsmas.miami.edu>
>>>> Cc: "Clive Agnew" <clive.agnew@man.ac.uk>
>>>> Subject: Re: Sahel drought "artifact"
>>>> Date: Tue, 13 Apr 2004 12:13:48 +0100
>>>>
>>>> Dear Professor Prospero,
>>>>
>>>> Thank you for your email. I read your paper with interest. It does
>>>> indeed show a strong correlation with conventional estimates of mean
>>>> annual rainfall. However, the paper implicitly assumes that the
>>>> mean annual rainfall represents the variation in rainfall for the
>>>> entire region. Our paper shows that those statistics are flawed
>>>> because of the changing station networks and that those regional
>>>> statistics do not show a 'drought' in the Sahel. Our paper does not
>>>> dispute the local scale evidence for drought.
>>>>
>>>> It is too simplistic to average mean monthly rainfall for such a
>>>> large heterogenous region and believe that the rainfall trend is
>>>> precise. What might be interesting is to correlate your results
>>>> against the mean annual rainfall corrected for the changing station
>>>> networks.
>>>>
>>>> Regards,
>>>>
>>>> Adrian
>>>>
>>>>
>>>>
>>>>   ----- Original Message -----
>>>>   From: Joseph M. Prospero <mailto:jprospero@rsmas.miami.edu> To:

>>>> a.chappell@salford.ac.uk <mailto:a.chappell@salford.ac.uk>
>>>> Sent: Thursday, April 08, 2004 10:33 PM
>>>>   Subject: Sahel drought "artifact"
>>>>
>>> Prof. Phil Jones
>>> Climatic Research Unit   Telephone +44 (0) 1603 592090
>>> School of Environmental Sciences   Fax +44 (0) 1603 507784
>>> University of East Anglia
>>> Norwich   Email   p.jones@uea.ac.uk
>>> NR4 7TJ
>>> UK
>>>
>>>

>>>
>>>
>>>
>>>
>>
>> --
>> Aiguo Dai   email: adai@ucar.edu
>> Climate & Global Dynamics Division   phone: 303-497-1357
>> National Center for Atmospheric Research   FAX : 303-497-1333
>> P.O. Box 3000, 1850 Table Mesa Drive
>> Boulder, CO 80307
>> homepage: http://www.cgd.ucar.edu/cas/adai/
>>
>>

411. 1084625760.txt
##########

From: Tom Wigley <wigley@cgd.ucar.edu>
To: Sarah Raper <sraper@awi-bremerhaven.de>, Sarah Raper <s.raper@uea.ac.uk>
Subject: volc paper
Date: Sat, 15 May 2004 08:56:00 -0600
Cc: Ben Santer <santer1@llnl.gov>, Caspar Ammann <ammann@ucar.edu>
Attachment: volc.doc

Dear Sarah,

Ben and I have had some long discussions about this paper, and I have made quite a few changes as a consequence. Most of these are minor -- but I realized that my statement that the peak cooling depended logarithmically on the sensitivity was potentially confusing. For this to be the case one has to have a relationship like

Tmax = A + B ln(S)

which implies odd results for very low sensitivity. Instead, I have fitted a relationship of the form

Tmax = A [S**n]

which gives Tmax = 0 when S = 0.

I have fitted a similar relationship to the decay time results, and I have done the same for the LG98 results. All this information has been added to the manuscript. It helps in understanding the differences between us and

LG98.

I had hoped to send this off earlier this week, i.e., before I go to Buenos Aires (tomorrow), but I never received the copyright form from you. Then I remembered that you were at that IPCC meeting in Ireland. So I have asked Liz Rothney to send the ms off next week as soon as she gets the copyright form from you. So please fax this back (303 497 1333) as soon as possible.

Best wishes,
Tom.

412. 1086722406.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: v.shishov@uea.ac.uk
Subject: Fwd: Re: Russian daily data
Date: Tue Jun 8 15:20:06 2004

> From: Dale Patrick Kaiser <kaiserdp@ornl.gov>
> Reply-To: kaiserdp@ornl.gov
> To: Keith Briffa <k.briffa@uea.ac.uk>
> Subject: Re: Russian daily data
> Date: Mon, 7 Jun 2004 10:31:02 -0400
> User-Agent: KMail/1.5.3
> Cc: d9k@ornl.gov
> X-UEA-MailScanner-Information: Please contact the ISP for more information
> X-UEA-MailScanner: Found to be clean
> Dear Keith,
> I wish I could say that updating the Russian data is on the front burner for us right now, but I'm afraid it's not. I'm having to plan some proposals and have been pulled off part of my normal CDIAC work for about 6 months to work on a special project. And in our small group, I'm the only climate guy (and the one that has done the Russian work thus far). Thus, the first suggestion I have is to discuss the data with NCDC; perhaps the best person to start with would be Pasha Groisman. Years ago, when I did the Russian work, the data were actually transferred from Russia to NCDC and then on to us, so I wouldn't be surprised if NCDC was holding updated data or at least could get ahold of data relatively easily. Perhaps you've already corresponded directly w/Slava Razuvaev or one of his colleagues at RIHMI-WDC? I'm afraid it's been quite a while since I've spoken w/Slava.
> Wait, maybe there is another way.... I've just remembered about NCDC's Global Daily Climate Network:
> [1]http://www.ncdc.noaa.gov/oa/climate/research/gdcn/gdcn.html
> I have not learned much about these holdings, but if you check it out perhaps they've incorporated more recent data daily into this database for the FSU. I sure hope so.
> I'm sorry that I cannot be of more help at this time. With any luck CDIAC can turn its attention to updates of these data in 2005.
> Regards,
> Dale
> On Friday 04 June 2004 7:18 am, you wrote:
> > Dear Dale
> > sorry to contact you out of the blue , but Phil Jones suggested I check
> > with you about the status of daily temperature (and possibly precipitation)
> > data for Russia that I believe you and colleagues might be planning to
> > update. I work with tree-ring data in Northern Russia and we are
> > particularly interested in looking at growing season and snow lie changes
> > in recent years that may be influencing the growth rates of trees and the
> > position of the tree line . We are especially interested in data for the

> Yamal Peninsula ,Taimyr and Indigirka (though we would also like to explore
> snow lie changes over the whole of northern Siberia eventually). Is there
> any chance of getting updated data for these initial regions in the near
> term , and perhaps the wider area eventually? We would be really grateful
> for any help in this regard.
> Very best wishes and thanks for your help
> Keith
>
> --
> Professor Keith Briffa,
> Climatic Research Unit
> University of East Anglia
> Norwich, NR4 7TJ, U.K.
>
> Phone: +44-1603-593909
> Fax: +44-1603-507784
>
> [2]http://www.cru.uea.ac.uk/cru/people/briffa/

--
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Environmental Sci. Division
Oak Ridge National Laboratory
Oak Ridge, TN 37831
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(865) 574-2232 (fax)
kaiserdp@ornl.gov
[3]http://cdiac.ornl.gov

--
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Phone: +44-1603-593909
Fax: +44-1603-507784
[4]http://www.cru.uea.ac.uk/cru/people/briffa/

References

1. http://www.ncdc.noaa.gov/oa/climate/research/gdcn/gdcn.html
2. http://www.cru.uea.ac.uk/cru/people/briffa/
3. http://cdiac.ornl.gov/
4. http://www.cru.uea.ac.uk/cru/people/briffa/

413. 1086904814.txt
##########

From: Tom Wigley <wigley@cgd.ucar.edu>
To: Sarah Raper <sraper@awi-bremerhaven.de>, Sarah Raper <s.raper@uea.ac.uk>
Subject: [Fwd: IPCC announcement of opportunity]
Date: Thu, 10 Jun 2004 18:00:14 -0600
Cc: Ben Santer <santer1@llnl.gov>

This is a multi-part message in MIME format.
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boundary="------------070901080902050505090308"
--------------070901080902050505090308 Content-Type: text/plain;

charset=us-ascii;
format=flowed Content-Transfer-Encoding: 7bit Sarah, I realize that you have got a copy of
this. What I am concerned about is the use of MAGICC in AR4. It is likely that the only way
that MAGICC can be legitimately used is for it to be (again!) calibrated against the
various AOGCMs being run for AR4. The AOGCM data that will be available this time will
allow us to do this more comprehensively than your TAR analysis. I think this is something
we should do together this time. I will talk to Jerry Meehl about this tomorrow or next
week, and also discuss how best to do this statistically with Doug Nychka -- with a view to
submitting a joint proposal. I would also like to involve Ben, since he is adept at getting
appropriate data from PCMDI/CMIP data files, and he can add insights that we may otherwise
miss. So the proposal would involve you, me, Doug and Ben. Tom.
========== --------
Original Message -------- Subject: IPCC announcement of opportunity Date: Thu, 10 Jun 2004
16:22:15 -0700 From: Curtis Covey To: George Boer , Ed Schneider , Wei-Chyung Wang , Tim
Barnett , Scott Power , Jouni Raisanen , Yanli Jia , David Webb , Pierre Friedlingstein ,
Sarah Raper , Jonathan Gregory , Marc Pontaud , Greg Flato , Tom Wigley , Phil Duffy , Dave
Ritson , Valentina Pavan , Ken Caldeira , letreut , Ken Sperber , Brian Soden , Fred Singer
, David Karoly , DUFRESNE Jean-Louis , Andrei Sokolov , Olivier de Viron , kattsov , Ping
Liu , Tom Knutson , Youichi Tanimoto , Kwang-Yul Kim , "Siobhan O'Farrell" , Kristin
Kuntz-Duriseti , Steve Marcus , "Francisco E. Werner" , Mingfang Ting , Cecilia Bitz ,
"Cathrine.Myrmehl" , "Gregory M. Ostermeier" , Dave Stephenson , "Ola.Johannessen" ,
Svetlana Kuzmina , Alpert Pinhas , Hirsch Tali , Evgeny Volodin , Dan Vimont , Ken Kunkel ,
Huei-Ping Huang , Zeng-Zhen Hu , "I.-S. Kang" , "Vikram M. Mehta" , Bob Iacovazzi ,
hengliu@students.uiuc.edu, Daithi Stone , Ray Bradley , Robert Kaufmann ,
d.stainforth1@physics.ox.ac.uk, raghu@ncmrwf.gov.in, Rob Colman , jhurrell@ucar.edu, Chris
Huntingford , Peter Webster , shj@atmos.yonsei.ac.kr, ysun@al.noaa.gov, Irina Gorodetskaya
CC: Ron Stouffer , Mojib Latif , Jerry Meehl , Bryant McAvaney , Peter Gleckler Dear
colleague, Attached (in PDF) is an announcement of opportunity to participate in analyses
of global coupled model output for the Fourth Assessment Report of the Intergovernmental
Panel on Climate Change. This is an open announcement, so please feel free to forward it to
anyone who may be interested. Sincerely, The WGCM Climate Simulation Panel Gerald Meehl,
Chair IPCC_analysis@ucar.edu --------------070901080902050505090308 Content-Type:
text/html; charset=us-ascii Content-Transfer-Encoding: 7bit Sarah,
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data that will be available this time will allow us to do this more
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than your TAR analysis. I think this is something we should do together this
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appropriate data from PCMDI/CMIP data files, and he can add insights that
we may otherwise miss. So the proposal would involve you, me, Doug and Ben.
Tom.
==========
-------- Original Message --------

Subject: IPCC announcement of opportunity
Date: Thu, 10 Jun 2004 16:22:15 -0700
From: Curtis Covey [1]<covey1@llnl.gov>
To: George Boer [2]<george.boer@ec.gc.ca>, Ed Schneider
[3]<schneide@cola.iges.org>,
Wei-Chyung Wang [4]<wang@climate.cestm.albany.edu>, Tim Barnett
[5]<tbarnett@ucsd.edu>,
Scott Power [6]<s.power@bom.gov.au>, Jouni Raisanen
[7]<jouni.raisanen@smhi.se>, Yanli Jia
[8]<Yanli.Jia@soc.soton.ac.uk>, David Webb
[9]<David.J.Webb@soc.soton.ac.uk>, Pierre
Friedlingstein [10]<pierre@lsce.saclay.cea.fr>, Sarah Raper
[11]<s.raper@uea.ac.uk>,
Jonathan Gregory [12]<jonathan.gregory@metoffice.com>, Marc Pontaud
[13]<marc.pontaud@meteo.fr>, Greg Flato [14]<gflato@ec.gc.ca>, Tom Wigley
[15]<wigley@ucar.edu>, Phil Duffy [16]<pduffy@llnl.gov>, Dave Ritson
[17]<ritson@slac.stanford.edu>, Valentina Pavan [18]<pavan@cineca.it>, Ken
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Andrei Sokolov [26]<sokolov@mit.edu>, Olivier de Viron
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[30]<tk@gfdl.noaa.gov>, Youichi Tanimoto [31]<tanimoto@ees.hokudai.ac.jp>,
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[32]<kwang@cyclo.met.fsu.edu>, "Siobhan O'Farrell"
[33]<Siobhan.O'Farrell@csiro.au>,
Kristin Kuntz-Duriseti [34]<kkd@stanford.edu>, Steve Marcus
[35]<slmarcus@mail1.jpl.nasa.gov>, "Francisco E. Werner"
[36]<cisco@unc.edu>, Mingfang Ting
[37]<ting@atmos.uiuc.edu>, Cecilia Bitz [38]<bitz@apl.washington.edu>,
"Cathrine.Myrmehl"
[39]<Cathrine.Myrmehl@nersc.no>, "Gregory M. Ostermeier"
[40]<greg@atmos.washington.edu>,
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[43]<Svetlana.Kuzmina@niersc.spb.ru>,
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[54]hengliu@students.uiuc.edu,
Daithi Stone [55]<stoned@atm.ox.ac.uk>, Ray Bradley
[56]<rbradley@geo.umass.edu>, Robert
Kaufmann [57]<kaufmann@crsa.bu.edu>, [58]d.stainforth1@physics.ox.ac.uk,
[59]raghu@ncmrwf.gov.in, Rob Colman [60]<r.colman@bom.gov.au>,
[61]jhurrell@ucar.edu, Chris
Huntingford [62]<chg@ceh.ac.uk>, Peter Webster [63]<pjw@eas.gatech.edu>,
[64]shj@atmos.yonsei.ac.kr, [65]ysun@al.noaa.gov, Irina Gorodetskaya
[66]<irina@ldeo.columbia.edu>
CC: Ron Stouffer [67]<Ronald.Stouffer@noaa.gov>, Mojib Latif
[68]<mlatif@ifm.uni-kiel.de>,
Jerry Meehl [69]<meehl@ucar.edu>, Bryant McAvaney
[70]<B.McAvaney@bom.gov.au>, Peter
Gleckler [71]<gleckler1@llnl.gov>

Dear colleague,

Attached (in PDF) is an announcement of opportunity to participate in analyses of global coupled model output for the Fourth Assessment Report of the Intergovernmental Panel on Climate Change. This is an open announcement, so please feel free to forward it to anyone who may be interested.

Sincerely,

The WGCM Climate Simulation Panel
Gerald Meehl, Chair
[72]IPCC_analysis@ucar.edu

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References

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414. 1087504782.txt
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From: "Janice Darch" <J.Darch@uea.ac.uk>
To: <env.faculty@uea>, <env.researchstaff@uea>
Subject: Global change and ecosystems
Date: Thu, 17 Jun 2004 16:39:42 +0100

2. Call for proposals - Thematic call in the area of 'Global change and ecosystems'.

OJ C159 (16.06.2004) p.3
Deadline for submissions: 26.10.2004

Activity: Priority thematic area 'Sustainable Development, Global Change and Ecosystems'; Sub-priority 'Global Change and Ecosystems'.

Call identifier: FP6-2004-Global-3

Total indicative budget: EUR 205 million

Areas called and Instruments:

- Area 6.3.I: Impact and mechanisms of greenhouse gas emissions and atmospheric pollutants on climate, ozone depletion and carbon sinks (IP, STREP, CA)
- Area 6.3.II: Water cycle, including soil related aspects (IP, STREP, CA)
- Area 6.3.III: Biodiversity and ecosystems (IP, STREP, CA, NoE)
- Area 6.3.IV: Mechanisms of desertification and natural disasters (IP, STREP, CA)
- Area 6.3.V: Strategies for sustainable land management, including coastal zones, agricultural land and forests (IP, STREP, CA)
- Area 6.3.VI: Operational forecasting and modelling including global climatic change observation systems (IP)
- Area 6.3.VII: Complementary research (IP, CA)
- Area 6.3.VIII: Cross-cutting issue: Sustainable Development concepts and tools (STREP, CA)
- Area 6.3.IX: Specific Support Actions (SSA)

FURTHER INFORMATION:
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The FP6 Information Desk
Directorate General RTD
B-1049 Brussels
www.cordis.lu/

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Tel : 44 (0)1603 592994
Fax : 44 (0)1603 593035

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From: Phil Jones <p.jones@uea.ac.uk>
To: David Viner <d.viner@uea.ac.uk>
Subject: Re: Proposal for a new Tyndall-led European research initiative
Date: Fri Jun 18 16:14:57 2004
Cc: Clare Goodess <C.Goodess@uea.ac.uk>

I'll leave it up to you then.
Phil
At 16:04 18/06/2004 +0100, David Viner wrote:

Phil
Err! yes i think this would be good to get involved.
D
On 18 Jun 2004, at 15:40, Phil Jones wrote:

Dave and Clare,
I am presuming we (CRU) don't want to get involved with this.
Cheers
Phil

From: "Alex Haxeltine" <Alex.Haxeltine@uea.ac.uk>
To: "Terry Barker \(DAE\)" <Terry.Barker@econ.cam.ac.uk>,
<wj.watson@sussex.ac.uk>,
"Andrew Jordan" <a.jordan@uea.ac.uk>,
"Bob Nicholls" <'rjn@soton.ac.uk'>,
"emily boyd" <e.boyd@uea.ac.uk>,
"Emma Tompkins" <e.tompkins@uea.ac.uk>,
"Franziska Matthies" <f.matthies@uea.ac.uk>,
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<kevin.anderson@umist.ac.uk>,
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"Nick Brooks" <nick.brooks@uea.ac.uk>,
"Phil Jones" <p.jones@uea.ac.uk>,
"rachel warren" <r.warren@uea.ac.uk>,
"simon shackley" <simon.shackley@umist.ac.uk>,
"Steve Sorrell" <S.R.Sorrell@sussex.ac.uk>,
"suraje Dessai" <s.dessai@uea.ac.uk>
Subject: Proposal for a new Tyndall-led European research initiative
Date: Fri, 18 Jun 2004 15:16:20 +0100
Organization: University of East Anglia
X-Mailer: Microsoft Outlook, Build 10.0.3311
Importance: Normal
Dear Colleague,
The Tyndall Centre is intending to lead a bid for a large EU research project (ca 12-15 million Euros in the initial bid) on climate change adaptation and mitigation strategies in Europe. The call was announced this week with outline bids (ca. 20 pages) due by October (3rd call of the sixth framework programme, FP6).
Please find attached a copy of an invitation that has been sent out to a key set of European partners. This provides a little further information on the proposed scope and content of the project. We will be holding a planning meeting with European partners from the evening of Monday 19th July to end of Tuesday 20th July 2004.
You are receiving this email because we thought that you might have some interest in participating in this project. We would therefore like to hold an internal planning meeting of all interested Tyndall-linked researchers on the 19th July (starting at lunchtime; ca 3-4 hours long). Please let us know by 25th June, if you would like to take part in this internal planning meeting; and also whether you would like to make a

short presentation at the meeting, about how your work with the Tyndall Centre might contribute. If you cannot attend on the 19th but are nevertheless interested in contributing to the proposal, please also let us know.
Warm regards,
Mike Hulme
John Schellnhuber
Alex Haxeltine

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<ADAM invite to planning meeting on 19-20 July.rtf>

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[1]http://www.cru.uea.ac.uk/link (With Information Forum)
[2]http://www.e-clat.org Tourism and Climate Change (With Information Forum)
[3]http://ipcc-ddc.cru.uea.ac.uk
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References

1. http://www.cru.uea.ac.uk/link
2. http://www.e-clat.org/
3. http://ipcc-ddc.cru.uea.ac.uk/

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From: Tom Wigley <wigley@cgd.ucar.edu>
To: Sarah Raper <sraper@awi-bremerhaven.de>, Sarah Raper <sraper@awi-bremerhaven.de>, Doug Nychka <nychka@cgd.ucar.edu>, Ben Santer <santer1@llnl.gov>
Subject: AR4 proposal
Date: Mon, 21 Jun 2004 08:17:37 -0600

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Dear Sarah, Doug and Ben,

Could you please check out the attached proposal. It is
short, but actually more than is necessary according to
what Jerry Meehl has told me.

I will be back in Boulder on Wednesday and would like
to give it to Jerry then.

Thanks,
Tom.

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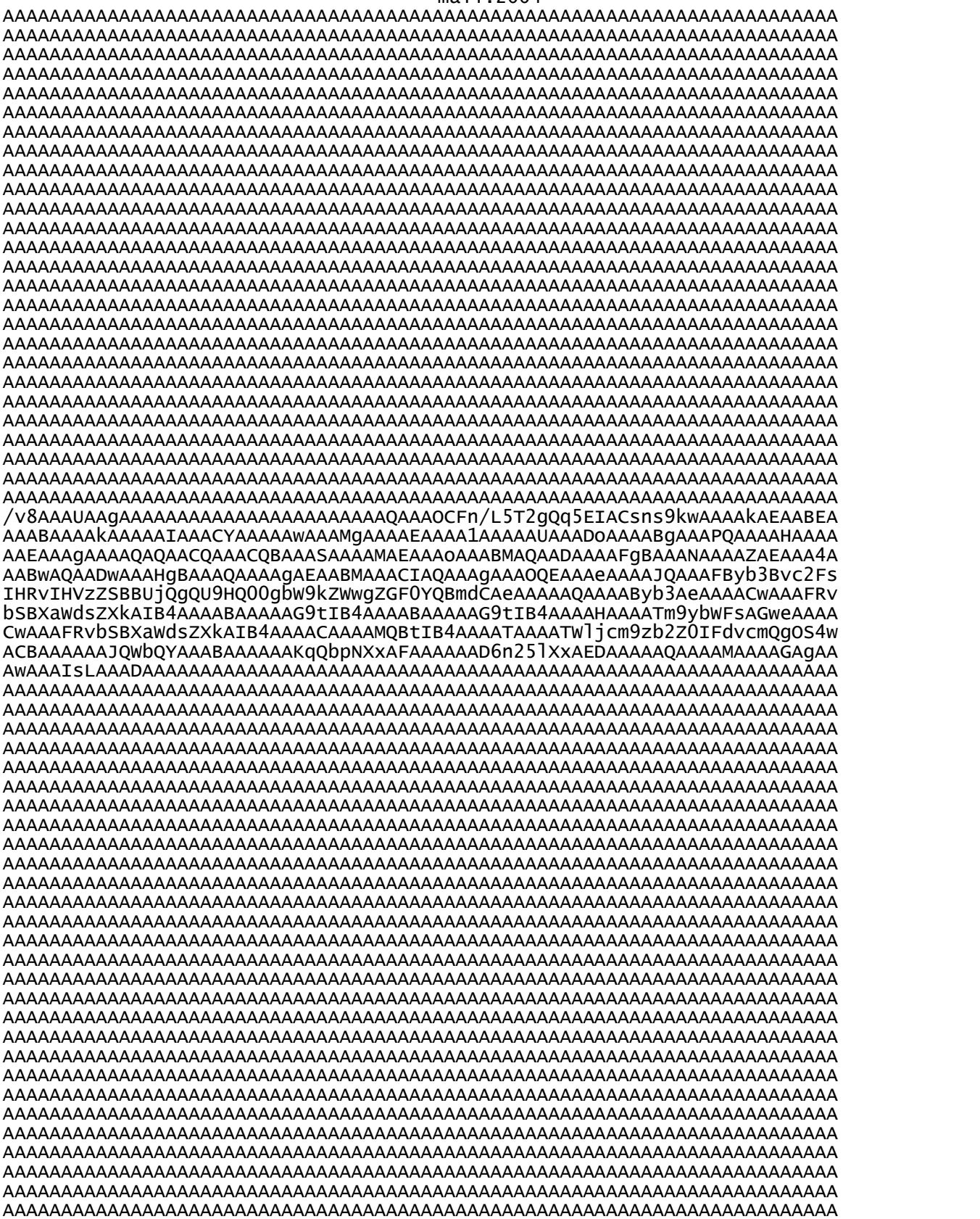
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From: Tom Wigley <wigley@cgd.ucar.edu>
To: Jerry Meehl <meehl@cgd.ucar.edu>, Sarah Raper <sraper@awi-bremerhaven.de>, Sarah Raper <s.raper@uea.ac.uk>, Ben Santer <santer1@llnl.gov>, Doug Nychka <nychka@cgd.ucar.edu>
Subject: AR4: missing attachment
Date: Wed, 30 Jun 2004 17:51:11 -0600

This is a multi-part message in MIME format.
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Content-Type: application/msword;
 name="AR4Proposal.doc"
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From: Tom Wigley <wigley@cgd.ucar.edu>
To: Sarah Raper <sraper@awi-bremerhaven.de>, Ben Santer <santer1@llnl.gov>, Doug Nychka <nychka@cgd.ucar.edu>
Subject: [Fwd: AR4 analyses]
Date: Thu, 01 Jul 2004 10:07:36 -0600

This is a multi-part message in MIME format.
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--------------020101090700030501080805 Content-Type: text/plain;
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format=flowed Content-Transfer-Encoding: 7bit -------- Original Message --------
Subject:
AR4 analyses Date: Thu, 01 Jul 2004 09:23:32 -0600 From: Jerry Meehl To: Curtis
Covey ,
wigley Thanks Tom. We have registered you, and will keep you posted. You are
correct that
the forcing data you require may not be available from all models. Hopefully
there will be
a few who will have what you need. Jerry and Curt -------- Original Message
----
Subject: AR4: missing attachment Date: Wed, 30 Jun 2004 17:51:11 -0600 From: Tom
Wigley
Organization: NCAR/CGD To: Jerry Meehl , Sarah Raper , Sarah Raper , Ben Santer ,
Doug
Nychka --------------020101090700030501080805 Content-Type: text/html;
charset=us-ascii
Content-Transfer-Encoding: 7bit
-------- Original Message --------

Subject: AR4 analyses
Date: Thu, 01 Jul 2004 09:23:32 -0600
From: Jerry Meehl [1]<meehl@ucar.edu>
To: Curtis Covey [2]<covey1@llnl.gov>, wigley [3]<wigley@ucar.edu>

Thanks Tom. We have registered you, and will keep you posted. You are correct
that the
forcing data you require may not be available from all models. Hopefully there
will be a
few who will have what you need.
Jerry and Curt
-------- Original Message --------

Subject: AR4: missing attachment
Date: Wed, 30 Jun 2004 17:51:11 -0600
From: Tom Wigley [4]<wigley@cgd.ucar.edu>
Organization: NCAR/CGD
To: Jerry Meehl [5]<meehl@cgd.ucar.edu>, Sarah Raper
[6]<sraper@awi-bremerhaven.de>, Sarah
Raper [7]<s.raper@uea.ac.uk>, Ben Santer [8]<santer1@llnl.gov>, Doug
Nychka
[9]<nychka@cgd.ucar.edu>

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References

1. mailto:meehl@ucar.edu
2. mailto:covey1@llnl.gov
3. mailto:wigley@ucar.edu
4. mailto:wigley@cgd.ucar.edu
5. mailto:meehl@cgd.ucar.edu
6. mailto:sraper@awi-bremerhaven.de
7. mailto:s.raper@uea.ac.uk
8. mailto:santer1@llnl.gov
9. mailto:nychka@cgd.ucar.edu

419. 1089318616.txt
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From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: HIGHLY CONFIDENTIAL
Date: Thu Jul 8 16:30:16 2004

Mike,
Only have it in the pdf form. FYI ONLY - don't pass on. Relevant paras are the last
2 in section 4 on p13. As I said it is worded carefully due to Adrian knowing Eugenia
for years. He knows the're wrong, but he succumbed to her almost pleading with him
to tone it down as it might affect her proposals in the future !
I didn't say any of this, so be careful how you use it - if at all. Keep quiet also
that you have the pdf.
The attachment is a very good paper - I've been pushing Adrian over the last weeks
to get it submitted to JGR or J. Climate. The main results are great for CRU and also

for ERA-40. The basic message is clear - you have to put enough surface and sonde
obs into a model to produce Reanalyses. The jumps when the data input change stand
out so clearly. NCEP does many odd things also around sea ice and over snow and ice.
The other paper by MM is just garbage - as you knew. De Freitas again. Pielke is also
losing all credibility as well by replying to the mad Finn as well - frequently as I see
it.
I can't see either of these papers being in the next IPCC report. Kevin and I will keep
them
out somehow - even if we have to redefine what the peer-review literature is !
Cheers
Phil
Mike,
For your interest, there is an ECMWF ERA-40 Report coming out soon, which
shows that Kalnay and Cai are wrong. It isn't that strongly worded as the first author
is a personal friend of Eugenia. The result is rather hidden in the middle of the report.
It isn't peer review, but a slimmed down version will go to a journal. KC are wrong
because
the difference between NCEP and real surface temps (CRU) over eastern N. America doesn't
happen with ERA-40. ERA-40 assimilates surface temps (which NCEP didn't) and doing
this makes the agreement with CRU better. Also ERA-40's trends in the lower atmosphere
are all physically consistent where NCEP's are not - over eastern US.

I can send if you want, but it won't be out as a report for a couple of months.
Cheers
Phil

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From: Phil Jones <p.jones@uea.ac.uk>
To: t.m.melvin@uea.ac.uk
Subject: Polar Urals
Date: Wed Jul 21 15:06:31 2004

Tom,
Can you send me via email the two sets of results you showed this morning of
the dating for the trw and mxd series from the Polar Urals? Just the two separate
ones - forget Yamal.

Cheers
Phil

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From: Phil Jones <p.jones@uea.ac.uk>
To: dwlarson@uoguelph.ca
Subject: Re:
Date: Fri Jul 23 15:29:11 2004

Doug,
Maybe Steve sent you the two emails I've resent. Ignore my ramblings at the end of one,
but I was getting a little fed up. The Legates email is at the end, in case you're interested.
The pdf is worth a read. Odd that he writes a press release, then starts working on a paper.
We've very occasionally written a press release, but only after the paper has come out.
I tried to explain the 'missing' rings. They aren't missing, but due to the samples not being right for density measurements. All Schweingruber's chronologies are constructed this way - traditional ring width measurements aren't made. Some of the Russian groups he's worked with have added extra ring width cores and sometime get longer series, but all the data Keith and I work with is from Fritz, so if density is missing, then RW is also.
Fritz did almost all the coring - 99% of the sites. We only help coring on a couple of occasions.
This comes from alignment tracking as you say, but Fritz also says it is partly due to the need to extract the lignin and to avoid resin. When we cored together, he was always saying we weren't doing it properly getting twisted cores. I'm not a proper dendro person, as I only got into this because of Keith - it may not be lignin, but something has to be extracted with solvents.
The Polar Urals site was collected by Fritz and Stepan Shiyatov. There are living trees back to the 1500s and then stumps at a slightly higher elevation. Stepan has been back more recently and regeneration is occurring at higher levels, but it is taking time. Tree

lines
take a while to respond to the recent warmth in some regions. Once the trees are established
and not killed by frosts/snow in winter they survive even if it gets cooler. I discussed this
in a review paper in RoG attached. The section on the issue is brief.
All the cores were collected over a couple of days. Fritz made a mistake with the labelling
for one core and that explains the 400 years of missing values. Someone at WDCP must have combined the cores with the same ids. Dendro people are always looking for the oldest trees and we kept the earliest series in. Steve seems to have a thing about these and the 10th and 11th centuries, but they are correctly dated. Fritz uses loads of plots and pointer years and doesn't make mistakes normally. There is a very distinct year at AD 1032. Fritz is also cross dating with LWW and EWW and other features and not just on RW. I say not just, he normally does with density. At the coring stage Fritz had no idea of the ages of the stumps (well just the number of years). There may have been samples off the front that couldn't be dated at all, for all I know. I suspect though they are roughly the same calendar age, as the site has distinct dates for the start of trees, which represent regeneration periods. Maybe you can try and explain the tree-line argument to Steve.
When he had to omit parts of cores, he was always able to know where the two parts sat in the sequence. We need to keep them together to do things like RCS.
Anyway, I have to go home - it's been very wet lately and the grass has grown. The lawn must be mowed when the sun shines.
Keep pushing that he should write up what he does (and Ross) in proper journals. E&E and Climate Research are not read by many now. I only look at them when I get alerted and I remain exasperated.
Cheers
Phil
Legates email
Phil Jones has made a valid point in that some of the articles cited in my critique do not 'directly' address problems with Mann and Jones (MJ) but rather, address problems with earlier works by Mann, Bradley, and Hughes (MBH) and other colleagues. Fair enough - I have changed the critique to reflect that fact. The revised version has been posted since July 19 at:
[1]http://www.ncpa.org/pub/ba/ba478/ba478.pdf
However, I still contend that most of my original arguments - namely, the problems with the shaft, blade, and sheath - apply equally to Mann and Jones as well as the other Mann et al. manifestations of the 'hockey stick'.
MJ incorporate data from a number of the same sources as those used by MBH; for example, Mann's unpublished PC1 from the western North American tree-ring data, Cook's Tasmanian tree rings, Thompson's Quelccaya and Dunde ice core oxygen isotope records (the latter embedded in Yang's Chinese composite), and Fisher's stacked Greenland ice core oxygen isotope

record. Calibration and verification of MJ includes the flawed MBH curve. Thus, any errors in MBH effectively undermine the calibration-verification results of MJ, leaving this study unsupported and any problems with the underlying common proxies identified in critiques of MBH will also result in identical problems in MJ.

My criticism regarding the blade is that 0.6 deg C warming for the last century is noted by the IPCC whereas MJ (and other M et al representations) have up to 0.95 deg C warming in their observed record. See MJ's figure 2 where for the global and NH reconstruction, their estimates for 2000 exceed +0.4 and +0.5 (nearly +0.6), respectively. MJ's NH curve is included in the attached graph. Thus, I stand by my criticism of MJ on this point, which is more egregious in MJ than other M et al representations.

>From Jones: "The trend over the 20th century in the Figure and in the instrumental data. IPCC quotes 0.6 deg C over the 1901-2000 period. Fact - but Legates is eyeballing the curve to get 0.95 deg C. A figure isn't given in Mann and Jones (2003). Take it from me the trend is about the same as the instrumental record."

Funny, but there IS a figure in MJ - see their Figure 2. As for me 'eyeballing' an apparently non-existent curve, I attach a figure from Soon et al. (2004) that contains a portion of MJ's Figure 2 to allow others to decide for themselves whether MJ suggest a twentieth century warming of 0.6 deg C or 0.95 deg C. Moreover, maybe someone can explain why every time Mann and his colleagues draft another curve, the temperature in 2000 gets warmer and warmer after the fact...

My criticisms regarding the sheath (largely from a paper on which I am working) stem from the characterization of the uncertainty by MJ that arises solely from the 'fit' statistics to the 1600-1855 period using cross-validation with, not observations, but composites of three previously compiled reconstructions, including that developed by MBH - the focus of known flaws and errors in the shaft. Note that some of the same data are used in both MBH and MJ, which doesn't allow for a truly independent cross-validation. My rather obvious point was not that fit statistics should not be included (as Jones asserts) but that MJ included no errors in either input realization (observations or proxy data) or other obvious sources of error. The claim by MBH and MJ is that only the model lack-of-fit contributes to uncertainty is inherently flawed.

Considerable errors exist in the representation of both fields - annual temperatures from both observations and proxy records - and must be incorporated. Clearly, there is a spatial bias associated with observations that are biased away from the oceans, high latitudes, and high altitudes. The spatial problem is far more pronounced when only a handful of proxies are used to represent the global temperatures at earlier time periods. Both MBH and MJ are equally guilty in this regard.

David R. Legates

Several people have asked me for the full references to the works I have cited. They are:

Chapman, D.S., M.G. Bartlett, and R.N. Harris (2004): Comment on 'Ground vs. surface air temperature trends: Implications for borehole surface temperature reconstructions' by M.E. Mann and G. Schmidt. Geophysical Research Letters, 31, L07205, doi:10.1029/2003GL019054.

Esper, J, E.R. Cook, and F.H. Schweingruber (2002): Low-frequency signals in long tree-ring chronologies for reconstructing past temperature variability, Science, 295, 2250-2253.

Esper, J, D.C. Frank, and R.J.S. Wilson (2004): Climate reconstructions: Low-frequency ambition and high-frequency ratification. EOS, Transactions of the American Geophysical Union, Vol. 85 (12):113,120.

IPCC TAR (Intergovernmental Panel on Climate Change, Third Assessment Report) (2001): Climate Change 2001: The Scientific Basis, Houghton, J.T., Ding, Y., Griggs, D.J., Noguer, M., van der Linden, P. J., Dai, X., Maskell, K., Johnson, C.A. (Eds.), Cambridge University Press.

Mann, M.E., R.S. Bradley, and M.K. Hughes (1998): Global-Scale Temperature Patterns and Climate Forcing Over the Past Six Centuries,

Nature, 392, 779-787. [see also the correction in Nature - Mann, Bradley, and Hughes, 2004]
Mann, M.E., R.S. Bradley, and M.K. Hughes (1999): Northern Hemisphere Temperatures During the Past Millennium: Inferences, Uncertainties, and Limitations. Geophysical Research Letters, 26, 759-762.
Mann, M.E., and P.D. Jones (2003): Global surface temperature over the past two millennia, Geophysical Research Letters, 30(15), 1820, doi: 10.1029/2003GL017814.
Mann, M.E., and G. Schmidt (2003): Ground vs. surface air temperature trends: Implications for borehole surface temperature reconstructions. Geophysical Research Letters, 30(12), 1607, doi:10.1029/2003GL017170.
McIntyre, S., and R. McKitrick (2003): Corrections to the Mann et al (1998) Proxy Data Based and Northern Hemispheric Average Temperature Series. Energy and Environment, 14, 751-771.
Pollack, H.N., and J.E. Smerdon (2004): Borehole climate reconstructions: Spatial structure and hemispheric averages. Journal of Geophysical Research, 109, D11106, doi:10.1029/2003JD004163.
Rutherford, S., and M.E. Mann (2004): Correction to 'Optimal surface temperature reconstructions using terrestrial borehole data'. Journal of Geophysical Research, 109, D11107, doi:10.1029/2003JD004290.
Soon, W.-H., S.L. Baliunas, C. Idso, S. Idso, and D.R. Legates (2003): Reconstructing Climatic and Environmental Changes of the Past 1000 Years: A Reappraisal. Energy and Environment, 14:233-296.
Soon, W.-H., D.R. Legates, and S.L. Baliunas (2004): Estimation and Representation of Long-Term (>40 year) trends of Northern-Hemisphere-gridded Surface Temperature: A Note of Caution. Geophysical Research Letters, 31(3).

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References

1. http://www.ncpa.org/pub/ba/ba478/ba478.pdf

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From: Phil Jones <p.jones@uea.ac.uk>
To: "Janice Lough" <j.lough@aims.gov.au>
Subject: Re: liked the paper
Date: Fri Aug 6 09:26:49 2004

Janice,
Most of the data series in most of the plots have just appeared on the CRU web site.
Go to data then to paleoclimate. Did this to stop getting hassled by the skeptics for the
data series. Mike Mann refuses to talk to these people and I can understand why. They are
just trying to find if we've done anything wrong. I sent one of them loads of series
and he barely said a thankyou. It seems they are now going for Tom Crowley, Lonnie
Thompson and Gordon Jacoby as most of their series are not on web sites.

Below is a link to an awful piece by Legates. He told me he is a writing a paper, but
wrote the press release first ! The pdf is worth getting for a couple of sentences, when
he
said that MJ restricted their use of paleo series to those that had correlations with
instrumental data ! It is a classic. 'Our uncertainty estimates are based solely on how
well
the proxy records match the observed data' !
The Legates piece must have been sent to loads of environment correspondents across
the world and a number of op-ed pieces appeared. Some were awful. Most have had responses from Ray Bradley, Caspar Amman and others.
Hope all is well with you and all the best to all. Glad you enjoyed the paper.
Cheers
Phil
PS Do you want to get involved in IPCC this time? I'm the CLA of the atmospheric obs.
chapter with Kevin Trenberth and we'll be looking for Contributing Authors to help the
Lead Authors we have. Paleo is in a different section this time led by Peck and Eystein
Janssen. Keith is a lead author as well.
Phil Jones has made a valid point in that some of the articles cited in my critique do not 'directly' address problems with Mann and Jones (MJ) but rather, address problems with earlier works by Mann, Bradley, and Hughes (MBH) and other colleagues. Fair enough - I have changed the critique to reflect that fact. The revised version has been posted since July 19 at:
[1]http://www.ncpa.org/pub/ba/ba478/ba478.pdf
However, I still contend that most of my original arguments - namely, the problems with the shaft, blade, and sheath - apply equally to Mann and Jones as well as the other Mann et al. manifestations of the 'hockey stick'.
MJ incorporate data from a number of the same sources as those used by MBH; for example, Mann's unpublished PC1 from the western North American tree-ring data, Cook's Tasmanian tree rings, Thompson's Quelccaya and Dunde ice core oxygen isotope records (the latter embedded in Yang's Chinese composite), and Fisher's stacked Greenland ice core oxygen isotope record. Calibration and verification of MJ includes the flawed MBH curve. Thus, any errors in MBH effectively undermine the calibration-verification results of MJ, leaving this study unsupported and any problems with the underlying common proxies identified in critiques of MBH will also result in identical problems in MJ.
My criticism regarding the blade is that 0.6 deg C warming for the last century is noted by the IPCC whereas MJ (and other M et al representations) have up to 0.95 deg C warming in their observed record. See MJ's figure 2 where for the global and NH reconstruction, their estimates for 2000 exceed +0.4 and +0.5 (nearly +0.6), respectively. MJ's NH curve is included in the attached graph. Thus, I stand by my criticism of MJ on this point, which is more egregious in MJ than other M et al representations.
>From Jones: "The trend over the 20th century in the Figure and in the instrumental data. IPCC quotes 0.6 deg C over the 1901-2000 period. Fact - but Legates is eyeballing the curve to get 0.95 deg C. A figure isn't given in Mann and Jones (2003). Take it from me the trend is about the same as the instrumental record."
Funny, but there IS a figure in MJ - see their Figure 2. As for me 'eyeballing' an apparently non-existent curve, I attach a figure from Soon et al. (2004) that contains a portion of MJ's Figure 2 to allow others to decide for themselves whether MJ suggest a twentieth century warming of

0.6 deg C or 0.95 deg C. Moreover, maybe someone can explain why every time Mann and his colleagues draft another curve, the temperature in 2000 gets warmer and warmer after the fact...

My criticisms regarding the sheath (largely from a paper on which I am working) stem from the characterization of the uncertainty by MJ that arises solely from the 'fit' statistics to the 1600-1855 period using cross-validation with, not observations, but composites of three previously compiled reconstructions, including that developed by MBH - the focus of known flaws and errors in the shaft. Note that some of the same data are used in both MBH and MJ, which doesn't allow for a truly independent cross-validation. My rather obvious point was not that fit statistics should not be included (as Jones asserts) but that MJ included no errors in either input realization (observations or proxy data) or other obvious sources of error. The claim by MBH and MJ is that only the model lack-of-fit contributes to uncertainty is inherently flawed.

Considerable errors exist in the representation of both fields - annual temperatures from both observations and proxy records - and must be incorporated. Clearly, there is a spatial bias associated with observations that are biased away from the oceans, high latitudes, and high altitudes. The spatial problem is far more pronounced when only a handful of proxies are used to represent the global temperatures at earlier time periods. Both MBH and MJ are equally guilty in this regard.

David R. Legates

At 15:55 06/08/2004 +1000, you wrote:

> Dear Phil
> Just finished reading your paper with Mike M in Rev of Geophysics which I very much enjoyed - will let you know when it hits the Mission Beach Chronicle!
> Hope all is well
> best wishes
> Janice
> Janice M. Lough
> Principal Research Scientist
> Australian Institute of Marine Science
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> ---
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References

1. http://www.ncpa.org/pub/ba/ba478/ba478.pdf

423. 1092167224.txt
##
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Gabi Hegerl <hegerl@duke.edu>, "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Mann and Jones (2003)
Date: Tue Aug 10 15:47:04 2004
Cc: Tom Crowley <tcrowley@duke.edu>

Gabi,
No second attempt - don't know what the first was? We'll be doing a new instrumental
data
set (surprisingly called HadCRUT3), but that's it at the moment.
Attached is a good review of corals - just out.
Cheers
Phil
At 10:36 10/08/2004 -0400, Gabi Hegerl wrote:

Hi Mike and Phil,
Thanks! Yes, factor 1.29 will get me closer to my best guess scaling (factor 1.6 to
same-size signals).
The scaling is a tough issue, and I think there are lots of possibilities to do it
depending on what one wants
to do. For comparing underlying forced signals, I think tls is best. To get a
conservative size paleo reconstruction
(like what part of instrumental do we reconstruct with paleo), the traditional scaling
is best.
I'll write up what Myles and I have been thinking and send it.
Phil, if there is a second attempt at that with the Hadley Centre, let me know, I don't
like racing anybody!
Gabi
Michael E. Mann wrote:

Dear Phil and Gabi,
I've attached a cleaned-up and commented version of the matlab code that I wrote for
doing the Mann and Jones (2003) composites. I did this knowing that Phil and I are
likely to have to respond to more crap criticisms from the idiots in the near future, so
best to clean up the code and provide to some of my close colleagues in case they want
to test it, etc. Please feel free to use this code for your own internal purposes, but
don't pass it along where it may get into the hands of the wrong people.
In the process of trying to clean it up, I realized I had something a bit odd, not
necessarily wrong, but it makes a small difference. It seems that I used the 'long' NH
instrumental series back to 1753 that we calculated in the following paper:

* Mann, M.E., Rutherford, S., Bradley, R.S., Hughes, M.K., Keimig, F.T., [1]Optimal
Surface Temperature Reconstructions using Terrestrial Borehole Data, Journal of
Geophysical Research, 108 (D7), 4203, doi: 10.1029/2002JD002532, 2003.

(based on the sparse available long instrumental records) to set the scale for the
decadal standard deviation of the proxy composite. Not sure why I used this, rather than
using the CRU NH record back to 1856 for this purpose. It looks like I had two similarly
named series floating around in the code, and used perhaps the less preferable one for
setting the scale.
Turns it, this has the net effect of decreasing the amplitude of the NH reconstruction
by a factor of 0.11/0.14 = 1.29.
This may explain part of what perplexed Gabi when she was comparing w/ the instrumental
series. I've attached the version of the reconstruction where the NH is scaled by the
CRU NH record instead, as well as the Matlab code which you're welcome to try to use
yourself and play around with. Basically, this increases the amplitude of the
reconstruction everywhere by the factor 1.29. Perhaps this is more in line w/ what Gabi
was estimating (Gabi?)
Anyway, doesn't make a major difference, but you might want to take this into account in
any further use of the Mann and Jones series...
Phil: is this worth a followup note to GRL, w/ a link to the Matlab code?
Mike
p.s. Gabi: when do you and Tom plan to publish your NH reconstruction that now goes back
about 1500 years or so? It would be nice to have more independent reconstructions
published in the near future! Maybe I missed this? Thanks...

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[3]http://www.evsc.virginia.edu/faculty/people/mann.shtml

```
% COMPOSITENH"
%
% (c) 2003, M.E. Mann
%
% THIS ROUTINE PERFORMS A RECONSTRUCTION OF NORTHERN HEMISPHERE
% MEAN ANNUAL TEMPERATURE BASED ON A WEIGHTED COMPOSITE OF LONG-TERM TEMPERATURE
% PROXY RECORDS SCALED AGAINST THE INSTRUMENTAL HEMISPHERIC MEAN TEMPERATURE
% SERIES, AS USED IN THE FOLLOWING TWO PUBLICATIONS:
%
%
% Jones, P.D., Mann, M.E., Climate Over Past Millennia, Reviews of Geophysics,
% 42, RG2002, doi:10.1029/2003RG000143, 2004
%
% Mann, M.E., Jones, P.D., Global Surface Temperatures over the Past two Millennia,
% Geophysical Research Letters,
```

```
% 30 (15), 1820, doi: 10.1029/2003GL017814, 2003
%
%
% 1. READ IN INSTRUMENTAL RECORD
%
% Read in CRU instrumental NH mean temeperature record (1856-2003)
load nh.dat;
yearinstr=nh(:,1);
% calculate both warm-season and annual means
warmseason=(nh(:,5)+nh(:,6)+nh(:,7)+nh(:,8)+nh(:,9)+nh(:,10))/6;
annualmean=nh(:,14);
% use annual mean  record in this analysis
nhmean=annualmean;
%
% 2. READ IN PREVIOUSLY PUBLISHED PROXY-RECONSTRUCTIONS OF NH ANNUAL MEAN
% RECONSTRUCTIONS AND FORM APPROPRIATELY SCALED COMPOSITE
%
% Read in Mann et al (1998), Crowley and Lowery (2000), and Jones et al (1998)
% NH temperature reconstructions
load nhem-millennium.dat;
load crowleylowery.dat;
load joneshemisrecons.dat;
nhmbh=nhem_millennium(1:981,2);
nhjones=joneshemisrecons(1:981,2);
nhcl=crowleylowery(1:981,2);
yearmillen=nhem_millennium(1:981,1);
% since some reconstructions are only decadally resolved, smooth each on
% decadal timescales through use of a lowpass filter with cutoff at
% f=0.1 cycle/year. Based on use of the filtering routine described in:
%
%    Mann, M.E., On Smoothing Potentially Non-Stationary Climate Time Series,
%    Geophysical Research Letters,  31, L07214, doi: 10.1029/2004GL019569, 2004.
%
% using 'minimum norm' constraint at both boundaries for all time series
nhsmooth=lowpass(nhmean,0.10,0,0);
nhmbhsmooth=lowpass(nhmbh,0.10,0,0);
nhjonessmooth=lowpass(nhjones,0.10,0,0);
nhclsmooth=lowpass(nhcl,0.10,0,0);
% Mann et al (1998) already calibrated in terms of hemispheric annual mean
temperature, but
% reference mean has to be adjusted to equal that of the instrumental series
% over the 1856-1980 overlap period (which uses a 1961-1990 reference period)
admbh=mean(nhsmooth(1:125))-mean(nhmbhsmooth(857:981));
newmbh=nhmbhsmooth+admbh;
% need to adjust and scale Jones et al (1998) and Crowley and Lowery (2000)
% reconstructions to match mean and trend of smoothed instrumental series
% over 1856-1980
t1=1856;
t2=1980;
x=(t1:t2)';
nhlong=nhmean(1:125);
smoothlong=lowpass(nhlong,0.10,0,0);
amean0=mean(smoothlong);
y=smoothlong;
[yc,t,trend0,detrend0,xm,ym] = lintrend(x, y);
%
y=nhclsmooth(t1-999:t2-999);
[yc,t,trendcl,detrendcl,xm,ym] = lintrend(x, y);
%
y=nhjonessmooth(t1-999:t2-999);
[yc,t,trendjones,detrendjones,xm,ym] = lintrend(x, y);
%
multjones=norm(trend0)/norm(trendjones);
```

```
adjustedjones=nhjonessmooth*multjones;
offsetjones=amean0-mean(adjustedjones(t1-999:t2-999));
newjones=adjustedjones+offsetjones;
newjones=newjones';
%
multcl=norm(trend0)/norm(trendcl);
adjustedcl=nhclsmooth*multcl;
offsetcl=amean0-mean(adjustedcl(t1-999:t2-999));
newcl=adjustedcl+offsetcl;
newcl=newcl';
%
nhlongcompose=0.3333*(newmbh+newjones'+newcl')';
%
% 3. READ IN AND PROCESS PROXY TEMPERATURE RECORDS
%
M=8;
load 'china-series1.dat'
load 'itrdb-long-fixed.dat'
load 'westgreen-o18.dat'
load 'torny.dat'
load 'chesapeake.dat'
load 'mongolia-darrigo.dat'
load 'dahl-jensen-gripbh1yrinterp.txt'
load 'dahl-jensen-dye3bh1yrinterp.txt'
% read in years
x1=china_series1(:,1);
x2=itrdb_long_fixed(:,1);
x3=westgreen_o18(:,1);
x4=torny(:,1);
x5=chesapeake(:,1);
x6=mongolia_darrigo(:,1);
x7=dahl_jensen_gripbh1yrinterp(:,1);
x8=dahl_jensen_dye3bh1yrinterp(:,1);
% read in proxy values
y1=china_series1(:,2);
y2=itrdb_long_fixed(:,2);
y3=westgreen_o18(:,2);
y4=torny(:,2);
y5=chesapeake(:,2);
y6=mongolia_darrigo(:,2);
y7=dahl_jensen_gripbh1yrinterp(:,2);
y8=dahl_jensen_dye3bh1yrinterp(:,2);
% Store decadal correlation of each proxy record with local available
% overlapping CRU gridpoint surface temperature record (see Mann and Jones, 2003)
corr(1)=0.22;
corr(2)=0.52;
corr(3)=0.75;
corr(4)=0.32;
corr(5)=0.31;
corr(6)=0.40;
corr(7)=0.53;
corr(8)=0.52;
% Estimate Area represented by each proxy record based on latitude of
% record and estimated number of temperature gridpoints represented by record
pi=3.14159;
factor=pi/180.0;
lat(1)=32.5;
dof(1)=4;
lat(2)=37.5;
dof(2)=2;
lat(3)=77;
dof(3)=0.667;
lat(4)=68;
```

```
dof(4)=3.5;
lat(5)=37.0;
dof(5)=1.0;
lat(6)=47;
dof(6)=1;
lat(7)=73;
dof(7)=0.667;
lat(8)=65;
dof(8)=0.667;
for j=1:M
    area(j)=dof(j)*cos(lat(j)*factor);
end
% determine min and max available years over all proxy records
%
minarray=[min(x1) min(x2) min(x3) min(x4) min(x5) min(x6) min(x7) min(x8)];
maxarray=[max(x1) max(x2) max(x3) max(x4) max(x5) max(x6) max(x7) max(x8)];
tbegin=max(minarray);
tend1=min(maxarray);
tend=max(maxarray);
% initialize proxy data matrix
notnumber = -9999;
for j=1:M
for i=1:minarray(j)-1
    time(i)=i;
    mat(i,j)=notnumber;
end
for i=minarray(j):tend
    time(i)=i;
end
for i=minarray(j):maxarray(j)
    if (j==1) mat(i,j)=y1(i-minarray(j)+1);
    end
    if (j==2) mat(i,j)=y2(i-minarray(j)+1);
    end
    if (j==3) mat(i,j)=y3(i-minarray(j)+1);
    end
    if (j==4) mat(i,j)=y4(i-minarray(j)+1);
    end
    if (j==5) mat(i,j)=y5(i-minarray(j)+1);
    end
    if (j==6) mat(i,j)=y6(i-minarray(j)+1);
    end
    if (j==7) mat(i,j)=y7(i-minarray(j)+1);
    end
    if (j==8) mat(i,j)=y8(i-minarray(j)+1);
    end
end
% added in Jones and Mann (2004), extend series ending between
% 1980 calibration period end and 2001 boundary by persistence of
% last available value through 2001
for i=maxarray(j)+1:tend
    if (j==1) mat(i,j)=y1(maxarray(j)-minarray(j)+1);
    end
    if (j==2) mat(i,j)=y2(maxarray(j)-minarray(j)+1);
    end
    if (j==3) mat(i,j)=y3(maxarray(j)-minarray(j)+1);
    end
    if (j==4) mat(i,j)=y4(maxarray(j)-minarray(j)+1);
    end
    if (j==5) mat(i,j)=y5(maxarray(j)-minarray(j)+1);
    end
    if (j==6) mat(i,j)=y6(maxarray(j)-minarray(j)+1);
    end
```

```
    if (j==7) mat(i,j)=y7(maxarray(j)-minarray(j)+1);
    end
    if (j==8) mat(i,j)=y8(maxarray(j)-minarray(j)+1);
    end
end
end
time=time';
data=[time mat];
% decadally lowpass of proxy series at f=0.1 cycle/year as described earlier
for j=1:M
    unfiltered=mat(minarray(j):tend,j);
    filt=lowpass(unfiltered,0.1,0,0);
    for i=1:minarray(j)-1
        filtered(i,j)=mat(i,j);
    end
    for i=minarray(j):tend
        filtered(i,j)=filt(i-minarray(j)+1);
    end
end
% standardize data
% first remove mean from each series
for j=1:M
    icount=0;
    amean(j)=0;
    for i=1:tend
        if (filtered(i,j)>notnumber)
            icount=icount+1;
            amean(j)=amean(j)+filtered(i,j);
        end
    end
    amean(j)=amean(j)/icount;
end
% now divide through by standard deviation
for j=1:M
    icount=0;
    asum=0;
    for i=1:tend
        if (filtered(i,j)>notnumber)
            asum=asum+(filtered(i,j)-amean(j))^2;
            icount=icount+1;
        end
    end
    sd(j)=sqrt(asum/icount);
    for i=1:tend
        standardized(i,j)=filtered(i,j);
        if (mat(i,j)>notnumber)
            standardized(i,j)=(filtered(i,j)-amean(j))/sd(j);
        end
    end
end
%
% 4. Calculate NH mean temperature reconstruction through weighted (and
%    unweighted) composites of the decadally-smoothed proxy indicators
%
% impose weighting scheme for NH mean composite
for j=1:M
%   weighting method 1: weight each proxy series by approximate area
%   weighting method 2: weight each proxy series by correlation between
%      predictor and local gridpoint series over available overlap period
%      during calibration interval
%   weighting method 3: weight each proxy series by correlation between
%      predictor and NH mean series over calibration interval:
%    weightlong(j)=lincor(nhlong,standardized(1856:1980,j));
```

```
%   weighting method 4: combine 1 and 3
%   weighting method 5: combine 1 amd 2 (this is the 'standard' weighting
%      scheme chosen by Mann and Jones (2003)
%   use standard weighting scheme
    weight(j)=corr(j)*area(j);
end
% perform reconstructions based on:
% (1) the 6 proxy temperature records available over interval AD 200-1980
% (2) all 8 proxy temperature records available over interval AD 553-1980
istart0=200;
istart1=200;
istart2=553;
nseries1=0;
nseries2=0;
weightsum1=0;
weightsum2=0;
for j=1:M
    if (istart1>=minarray(j))
        nseries1=nseries1+1;
        weightsum1=weightsum1+weight(j);
    end
    if (istart2>=minarray(j))
        nseries2=nseries2+1;
        weightsum2=weightsum2+weight(j);
    end
end
% calculate composites through 1995 (too few series available after that date)
% As discussed above, persistence is used to extend any series ending
% between 1980 and 1995 as described by Jones and Mann (2004).
tend=1995;
for i=istart1:tend
    unweighted1(i)=0;
    unweighted2(i)=0;
    weighted1(i)=0;
    weighted2(i)=0;
    for j=1:M
        if (istart1>=minarray(j))
            unweighted1(i)=unweighted1(i)+standardized(i,j);
            weighted1(i)=weighted1(i)+weight(j)*standardized(i,j);
        end
        if (istart2>=minarray(j))
            unweighted2(i)=unweighted2(i)+standardized(i,j);
            weighted2(i)=weighted2(i)+weight(j)*standardized(i,j);
        end
    end
end
unweighted1=unweighted1/nseries1;
unweighted2=unweighted2/nseries2;
weighted1=weighted1/weightsum1;
weighted2=weighted2/weightsum2;
unweighted1(1:istart1-1)=0;
unweighted2(1:istart2-1)=0;
weighted1(1:istart1-1)=0;
weighted2(1:istart2-1)=0;
% scale composite to have same variance as decadally-smoothed instrumental
% NH series

% Mann and Jones (2003) and Jones and Mann (2004) used for this purpose
% the extended (1753-1980) NH series used in:
%    Mann, M.E., Rutherford, S., Bradley, R.S., Hughes, M.K., Keimig, F.T.,
%    Optimal Surface Temperature Reconstructions using Terrestrial Borehole Data,
%    Journal of Geophysical Research, 108 (D7), 4203, doi: 10.1029/2002JD002532,
2003.
```

```
% That series has a decadal standard deviation sd=0.1123
% If instead, the 1856-2003 CRU instrumental NH mean record is used, with
% a decadal standard deviation of sd=0.1446, the amplitude of the reconstruction
% increases by a factor 1.29 (this scaling yields slightly lower verification
% scores)
load nhem-long.dat
nhemlong=nhem_long(:,2);
longsmooth=lowpass(nhemlong,0.10,0,0);
sd0=std(longsmooth);
% use weighted (rather than unweighted) composite in this case
series1=weighted1;
% center composites on 1856-1980 calibration period
y=series1(t1:t2)';
amean1=mean(series1(t1:t2));
compseries1=series1(t1:t2)-amean1;
mult1=sd0/std(compseries1);
% scale composite to standard deviation of instrumental series and re-center
% to have same (1961-1990) zero reference period as CRU NH instrumental
% temperature record
adjusted1=series1*mult1;
offset1=amean0-mean(adjusted1(t1:t2));
compose1=adjusted1+offset1;
compose1=compose1';
series2=weighted2;
y=series2(t1:t2)';
amean2=mean(series2(t1:t2));
compseries2=series2(t1:t2)-amean2;
mult2=sd0/std(compseries2);
adjusted2=series2*mult2;
offset2=amean0-mean(adjusted2(t1:t2));
compose2=adjusted2+offset2;
compose2=compose2';
%
% 5. UNCERTAINTY ESTIMATION, AND STATISTICAL VERIFICATION
%
% estimate uncertainty in reconstruction
% nominal (white noise) unresolved calibration period variance
calibvar=lincor(smoothlong,compose1(t1:t2))^2;
uncalib=1-calibvar;
sdunc=sd0*sqrt(uncalib);
% note: this is the *nominal* white noise uncertainty in the reconstruction
% a spectral analysis of the calibration residuals [as discussed briefly in
% Mann and Jones, 2003] indicates that a peak at the multidecadal timescale
% that exceeds the white noise average residual variance by a factor of
% approximately 6.  A conservative estimate of the standard error in the
% reconstruction thus inflates the nominal white noise estimate "sdunc" by a
% factor of sqrt(6)
sdlow = sdunc*sqrt(6)
% calculate long-term verification statistics for reconstruction
% use composite of Mann et al (1998)/Crowley and Lowery (2000)/Jones et al (1998)
% and AD 1600-1855 interval
overlapcomp=nhlongcompose(1:981);
% work with longer reconstruction (back to AD 200)
overlaprecon=compose1(1000:1980)';
%overlaprecon=compose2(1000:1980)';
%calculate verification R^2
series11=overlaprecon(601:856);
series22=overlapcomp(601:856);
verifrsq=lincor(series11,series22)^2
% calculate verification RE
var1=0.0;
var2=0.0;
var3=0.0;
```

```
var4=0.0;
var5=0.0;
am0=0.0;
% insure convention of zero mean over calibration interval
for i=857:981
   am0=am0+overlapcomp(i);
end
am0=am0/125;
for i=601:856
    var1=var1+(overlapcomp(i)-am0)^2;
    var2=var2+(overlapcomp(i)-overlaprecon(i))^2;
end
verifRE=1-var2/var1
```

--
~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~
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2. mailto:mann@virginia.edu
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml
4. mailto:hegerl@duke.edu
5. http://www.env.duke.edu/faculty/bios/hegerl.html

424. 1092418712.txt

```
##########################################################################################
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Fwd: RE: IJOC040512 review
Date: Fri Aug 13 13:38:32 2004

    Mike,
       I'd rather you didn't. I think it should be sufficient to forward the para
from Andrew
```
~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~~

Conrie's
email that says the paper has been rejected by all 3 reviewers. You can say that the
paper was an extended and updated version of that which appeared in CR.
Obviously, under no circumstances should any of this get back to Pielke.
Cheers
Phil
At 08:11 13/08/2004 -0400, you wrote:

Thanks a bunch Phil,
Along lines as my other email, would it be (?) for me to forward this to the chair of
our commitee confidentially, and for his internal purposes only, to help bolster the
case against MM??
let me know...
thanks,
mike
At 03:43 AM 8/13/2004, Phil Jones wrote:

Mike,
The paper ! Now to find my review. I did suggest to Andrew to find 3 reviewers.
Phil

From: "Andrew Comrie" <comrie@climate.geog.arizona.edu>
To: "'f028'" <P.Jones@uea.ac.uk>
Subject: RE: IJOC040512 review
Date: Mon, 24 May 2004 01:29:44 -0700
X-Mailer: Microsoft Outlook, Build 10.0.4024
Importance: Normal
X-Virus-Scanned: by amavisd-new at email.arizona.edu
X-UEA-MailScanner-Information: Please contact the ISP for more information
X-UEA-MailScanner: Found to be clean
X-UEA-MailScanner-SpamScore: ssss
<<...>>
Dear Phil,
IJOC040512 "A Socioeconomic Fingerprint on the Spatial Distribution of Surface Air
Temperature Trends"
Authors: RR McKitrick & PJ Michaels
Target review date: July 5, 2004
Following from our email, many thanks for agreeing to review the paper above that has
been submitted to the International Journal of Climatology for consideration. I have
attached the manuscript, and the information for reviewers is provided below. Please let
me know that you receieved the file.
In the interests of expediting the review process, I encourage you to email your review
as soon as is convenient. I would like to hear from you by the target date above, or as
soon after as possible.
Referee's names are kept anonymous. When composing your review, please keep your
"Comments to the Author" separate from your confidential comments to the editor. With
your comments to me, please be sure to provide one of these summary recommendations:
1. Accept without further revision.
2. Accept subject to minor revisions (changes to the text only, or simple follow-on

analyses).
3. Accept subject to major revisions (major text changes, recalculations or new analyses).
4. Reject.
In the case of minor revisions, the revised manuscript will be checked only by the editor. For major revisions, the revised manuscript may be sent to you again for a second review. It will also be useful if you will grade the contribution overall on the following scale:
A. Very good (a continuing and useful advance in an area of importance).
B. Good (satisfactory and of sufficient importance to merit publication).
C. Adequate (of marginal interest).
D. Poor (not significant enough to merit publication).
E. Very poor (trivial, or incorrect, or of no interest, or not new, etc.).
For your review, please also comment if any of the following points are not satisfactory or suitable: topic appropriate for the journal, correctness of the title, reduction in paper length, quality and quantity of illustrations, units, use of English, and key words.
Your contribution to the review process is essential and greatly valued.
Sincerely,
Andrew Comrie
Dr. Andrew C. Comrie
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Web: [1]http://geog.arizona.edu/~comrie/
Regional Editor for the Americas, International Journal of Climatology
[2]http://www.interscience.wiley.com/ijoc
-----Original Message-----
From: f028 [[3]mailto:f028@uea.ac.uk] On Behalf Of f028
Sent: Monday, May 24, 2004 1:04 AM
To: Andrew Comrie
Subject: RE: IJOC040512 review
Andrew,
    I can do this. I am in France this week but back in the UK all June.
 So send and it will be waiting my return.
 Phil
>===== Original Message From "Andrew Comrie" <comrie@climate.geog.arizona.edu>
=====
>Dear Prof. Jones,
>
>IJOC040512 "A Socioeconomic Fingerprint on the Spatial Distribution of
>Surface Air Temperature Trends"
>Authors: RR McKitrick & PJ Michaels
>Target review date: July 5, 2004
>
>I know you are very busy, but do you have the time to review the above
>manuscript for the International Journal of Climatology? If yes, can
>you complete the review within about five to six weeks, say by the
>target review date listed above? I will send the manuscript
>electronically.
>
>If no, can you recommend someone who you think might be a good choice to

>review this paper?
>
>Thanks for considering my request.
>
>Best wishes,
>
>Andrew Comrie
>
>Dr. Andrew C. Comrie
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Professor Michael E. Mann
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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[6]http://www.evsc.virginia.edu/faculty/people/mann.shtml

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References

1. http://geog.arizona.edu/~comrie/
2. http://www.interscience.wiley.com/ijoc
3. mailto:f028@uea.ac.uk
4. http://geog.arizona.edu/~comrie/
5. http://www.interscience.wiley.com/ijoc
6. http://www.evsc.virginia.edu/faculty/people/mann.shtml

425. 1092433030.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To:
John.Birks@bot.uib.no,masson@lsce.saclay.cea.fr,dirk.verschuren@UGent.be,Laurent.Labeyrie@lsce.cnrs-gif.fr,juerg.beer@eawag.ch,A.Lotter@bio.uu.nl,t.osborn@uea.ac.uk,hufischer@awi-bremerhaven.de ,dan.charman@plymouth.ac.uk,karin@natgeo.su.se
Subject: IMPRINT
Date: Fri Aug 13 17:37:10 2004
Cc: wanner@giub.unibe.ch,esper@wsl.ch,
Basil.Davis@bgc-jena.mpg.de,sigfus@gfy.ku.dk,guiot@cerege.fr,Ian.Snowball@geol.lu.se,antti.ojala@gsf.fi,atle.nesje@geol.uib.no,atte.korhola@helsinki.fi,Keith.Barber@soton.ac.uk,Sandy.Tudhope@ed.ac.uk ,eavaganov@forest.akadem.ru, Eystein Jansen <eystein.jansen@geo.uib.no>, Rick Battarbee <r.battarbee@geog.ucl.ac.uk>, Tim Osborn <t.osborn@uea.ac.uk>, , Jan Esper <esper@wsl.ch>, brazdil@sci.muni.cz, benito@ccma.csis.es

Dear Colleagues,

This note is to solicit your possible collaboration in an application to the European
Commission under Framework 6, possibly as one of the partners in IMPRINT. This is an
integrated palaeoclimate/climate modelling project concerned primarily with the Holocene,
but also incorporating specific studies on other interglacial warm periods. AT THIS STAGE
THIS IS A PROVISIONAL ENQUIRY RATHER THAN A DEFINITE REQUEST FOR YOUR INVOLVEMENT.
 The project has been some time (years) in gestation and has evolved from other proposals.
An unfinished draft is appended to this message for your information - but we would ask
that you respect its confidentiality , whether or not you are interested in working with
us. Eystein Jansen has agreed to coordinate IMPRINT. We are now refining the initial
submission. I, and Valerie Masson, are nominally fronting WorkPackage 1: concerned with
assembling, reinterpreting, amalgamating and analysing the climate data; a combination of
instrumental, documentary and other indirect, proxy climate information. This Workpackage
will also organise the aggregation of best possible climate forcing proxy evidence, as
means of exploring links with the empirical climate data, but also as input to the
significant effort in climate modelling to be undertaken in other workpackages.
WorkPackage 1 has been divided into a number of sub themes or Tasks and these, along with
the content of all Workpackages, is described in the attached document. Note that this is
very much work in progress at this stage and your comments and input to all parts will be
welcome. We will refine the wider list of collaborating institutes at a later stage.

At this stage we envisage a total budget application of about 17 million Euro with a
nominal share of 5 million for WorkPackage 1. While this is a large sum, I am sure you
will appreciate that when distributed among many partners and stretched over five years it
imposes a severe limitation on the total number of partners that can be feasibly included.

Therefore we have had to conceive of different degrees, or levels, of involvement of the
very many colleagues and institutions that are required to make this project a success.
Thus, we envisage a distinction between a number of full partners, though again with
varying resource allocation depending on specific inputs and requirements (still to be
determined), and a larger number of collaborators. Specific funding will be allocated to
facilitate the involvement of these many other groups, who we see taking part in workshops,
in return for full access to joint data and modelling results. This is the only way that
we see of overcoming the envisaged restriction imposed by the EC on total partner numbers.

We have chosen partners who we hope will be able to furnish expertise in specific research
areas and, hopefully, facilitate data assembly and exchange between members of the wider
communities.
PLEASE NOTE THAT THOSE PEOPLE LISTED IN THE "TO" LINE OF ADDRESSES ARE THOSE TENTATIVELY
EARMARKED TO BE TASK LEADERS WITHIN WORKPACKAGE 1. THOSE LISTED UNDER THE "CC" HEADING ARE
EARMARKED TO be PARTNERS - ORGANISING WORK AND DATA EXCHANGE WITHIN THEIR COMMUNITY. We
have a suggested list of many others who we would hope to involve - but not at full
partner level. Your input to the compleinon of this list will be asked for later. We would
ask that , for now, you do not circulate this provisional proposal .
We realise that many other partners could have been fully justifiably included, but the
need for pragmatism must eventually limit their formal roles. We hope that this reality
will be accepted by those colleagues not included as primary partners and they will still
be willing to collaborate to achieve the wider aims of IMPRINT.

The specific partner roles, as suggested to date, are described in the Workpackage 1
section of the appended IMPRINT document. Would you now please indicate whether or not you
are willing to join this effort, and please feel free to comment on any aspect: of
Workpackage 1 to myself and Valerie; or of the project as a whole to Eystein.

With very best wishes,

Keith

--
Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.

Phone: +44-1603-593909
Fax: +44-1603-507784

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From: Phil Jones <p.jones@uea.ac.uk>
To: "Susan Solomon" <Susan.Solomon@noaa.gov>, <trenbert@cgd.ucar.edu>, IPCC-WG1 <ipcc-wg1@al.noaa.gov>, martin.manning@noaa.gov, Susan.Solomon@noaa.gov
Subject: Re: [Fwd: Re: [Wg1-ar4-clas] WGI AR4 LA1 Programme]
Date: Sun Aug 15 10:56:37 2004
Cc: p.jones@uea.ac.uk

Susan,
Thanks for the comments.
Cheers
Phil
At 15:51 13/08/2004 -0600, Susan Solomon wrote:

Dear Phil, dear Kevin,
Thanks for your message. It's very good to hear that you are getting together and will
have time to talk about this. I will make a few points and suggestions below for your
consideration.
Safe travels,
Susan

Martin, Susan et al,
Kevin and I will be at a GCOS meeting Mon-Weds next week in Geneva, so will have some
time to discuss our chapter. I've sent Kevin some thoughts about boundaries between
chapters. If you can provide your views on a few issues, then it will help us in our
discussions.
1. We have extended outlines, which clarify some issues, but how rigid are they? I say this
wrt the overviews/visions you expect on the Monday pm of the Trieste meeting.

The extended outlines show you what the thought process was at Marrakech and Potsdam
that led to the present outlines. It's your report, and you may wish to do things
differently. Where that may involve other chapters, such work would need to be coordinated/decided jointly but most things are not like that.

2. In Chapter 3, we have a section 3.9 on synthesis/consistency amongst obs. Does this
involve obs such as glacier retreat and changes in sea ice, snow cover from chapters
4-6? Chapters 4-6 don't have similar sections.

We had some discussions on that in Potsdam in particular if I recall. Dividing up the
observations into three chapters solves some problems and raises others, and this is one
of them. My own thinking has been that issues such as the consistency of glacier
retreat with observations may be better handled in the ice chapter, which presumably
will be going into a bit more depth on processes affecting glaciers from the ice physics
point of view, providing a bit deeper basis for the assessment. The consistency of

observations between the three observations chapters could then be dealt with in the technical summary, drawing on the findings from all three. But it is probably going to be helpful if we have a discussion on this among the three chapters and come to a common view.

3. Chapter 1 has a section on new data and data rescue. I guess we should be involved in that, but also Ch 9 on attribution as it has to be worthwhile. Also the new data and rescued data could be useful for model validation. I expect Ch 3 to heavily use Reanalysis-based results.

Yes, we expected there would need to be discussion on that. It may involve a subset of people who should be urged to get together as needed.

4. Chapter 3 has SST and all the circulation indices, so here we need to liaise with Ch 5 and 6 and eventually with 9.

Yes, agreed, and Kevin and others tried to work that into the outline in Potsdam.

5. I agree with Kevin though on whether formal meetings of the whole of the chapters are needed. Might this be better done with the CLAs and you?

There will be a lot to do in Trieste and we want to make efficient use of people's time - it is probably true that not all the people need to be involved when the points you've made so far are discussed. The morning 1-hour sessions with all CLAs are also intended to be a forum where some of these kinds of issues (the broader ones) could be handled.

6. Considering all the above, I reckon we need to meet with Ch 4 and 6 (on glacier retreat, snow, sea ice and temperature), Chapters 6 and 9 on what they expect from us and similarly with Chapter 5 (although I feel this is clear in the extended outline). Finally, Chapters 1, 3 and 6 (and maybe 9) need to discuss data rescue and new techniques.

That sounds right to me. I would add your number 7 below into that mix as well. It's really up to you to decide how you want to handle it. But prompted by your message, the one from Kevin below, and some others, I think it will be helpful for us to compile a list of all such issues raised - so I am asking the TSU to do that, combining with another set that we received in the comments from governments (they actually raised a number of such comments, quite rightly).

7. The Appendices in Chapters 3-5 need some sort of co-ordination.

Bests,
Susan

At 11:31 11/08/2004 -0600, Kevin Trenberth wrote:

Martin, Susan et al:
In thinking more about Chapter 3, I believe we will have issues on who and what is
covered on
1) ENSO related stuff Chapter 3 vs Chapter 5
2) Consistency of retreat of glaciers, snow and ice vs temperatures Chapter 3 vs chapter
4.
There are probably others, but these may require some negotiation unless it is already
settled in your mind? Whether a formal meeting between chapters is needed or whether
the CLAs can meet and agree is not yet clear to me.
Kevin
IPCC-WG1 wrote:

Dear WGI CLAs and Bureau Members,
Please find attached a draft programme for the upcoming WGI AR4 First Lead Authors
Meeting, 26-29 September 2004, Trieste, Italy. Please note the section regarding
"cross-chapter breakout sessions". We have suggested four breakouts of this type, but
would appreciate any suggestions from you regarding other cross-chapter breakouts that
you feel may be needed. We kindly ask that you provide the WGI TSU
<[1]mailto:ipcc-wg1@al.noaa.gov><ipcc-wg1@al.noaa.gov> any feedback you may have by
Friday, 20 August 2004.
Best regards,
WGI TSU
--
~~~~~~
IPCC WGI TSU
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Fax: +1 303 497 5686/5628
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_____
Wg1-ar4-clas mailing list
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[4]http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas

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Please note my new email address for your records:
Susan.Solomon@noaa.gov

****

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References

1. mailto:ipcc-wg1@al.noaa.gov
2. mailto:ipcc-wg1@al.noaa.gov
3. mailto:Wg1-ar4-clas@joss.ucar.edu
4. http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-clas
5. mailto:trenbert@ucar.edu
6. http://www.cgd.ucar.edu/cas/
7. http://www.cgd.ucar.edu/cas/

427. 1093294138.txt

##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: t.m.melvin@uea.ac.uk
Subject: Fwd: Yamal treeline figures
Date: Mon Aug 23 16:48:58 2004

Date: Mon, 9 Oct 2000 18:08:04 +0500
From: Rashit Hantemirov <rashit@ipae.uran.ru>
X-Mailer: The Bat! (v1.00 Build 1311) Registered to Andy Malyshev
Reply-To: Rashit Hantemirov <rashit@ipae.uran.ru>
Organization: IPAE
Priority: Normal
X-Confirm-Reading-To: Rashit Hantemirov <rashit@ipae.uran.ru>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Yamal treeline figures
Dear Keith,
Stepan Shiyatov tell me that you need some figures concerning
Yamal chronology and tree line dynamics to show somewhere in

France.
Attached are archived files contained some figures.
File MAP - the map of region of research. Red dots - subfossil wood sites, green marks - recent northern border of larch along river valleys.
File FIGURES - in Excel format, contains several figures.
 Sheet "Values-10" - data on northernmost position of trees and number of trees dated for corresponding year (decadal step)
 Sheet "Treeline" - dynamics of treeline in Yamal during last 7000 years reconstructed using about 1000 subfossil wood remains. Recent treeline position is about 67°34.
 One year ago we supposed (C-14 data, Hantemirov, Shiyatov 1999) that significant drop of treeline (the transition from "middle" to "late" Holocene) was about 1700-1600 AD. According new data it was earlier (about 2550 BC). May be it is because of lack of data from region northward of 68°N (only 25 datings)?
 Sheet "Treeline and Nu" - treeline dynamics and number of dated trees. May be number of trees reflects the long scale climate fluctuations as well.
 Sheet "2600-all" - for last 4600 years: treeline dynamics, number of trees, 11 most cold summers for last 7000 years (according our version of reconstruction), most expressed frosts in July (reconstructed using junipers from Polar Urals, see file PATHOL, frost in 1626 BC - based on subfossil larch - you can put away it), summer temperatures reconstruction smoothed with 20- and 100-year filters (our version of reconstruction).
 Sheet "Values-2" - values for preceding figures, in 2-years step.
 Sheet "Yam-Ur-fig" - comparing of treeline data for Yamal and Polar Urals upper treeline dynamics (data by S.G.Shiyatov)
 Sheet "Yamal-Ural" - values for preceding figure, in 2-years step.
 Sheet "Treeline-std" - treeline dynamics and 50-year standard deviations of summer temperatures (our version of reconstruction). This figure shows surprising high negative correlation. However may be both of them just reflect long scale climate fluctuations?
 Sheet "Std" - 50-year standard deviations of summer temperatures (our version of reconstruction) .
File PATHOL - in Excel format, contains data and figure on pathological structures in tree rings of Siberian juniper (Juniperus sibirica Burgsd.). According our data (Hantemirov et al., 2000) the presence of frost rings provides evidence for frosts that occurred in late June or first days of July (frost rings in earlywood) and in the first half of July (frost rings in late wood). Long term and pronounced temperature drop in the middle of very warm period in the second half of July is the factor responsible for wood density fluctuations (false rings).
Please let me know when you receive this. Some time large messages get lost.
P.S. We (Eugene Vaganov, Stepan Shiyatov, Leonid Agafonov and I) will be in Birmensdorf from 23 till 29 October. Are you going to Switzerland after your meeting? We would be happy to see you there.
Best regards,
Rashit M. Hantemirov
Lab. of Dendrochronology
Institute of Plant and Animal Ecology
8 Marta St., 202
Ekaterinburg, 620144, Russia
e-mail: rashit@ipae.uran.ru

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--
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[1]http://www.cru.uea.ac.uk/cru/people/briffa/

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/

428. 1093794363.txt
##########

From: Martin Munro <mmunro@LTRR.ARIZONA.EDU>
To: ITRDBFOR@LISTSERV.ARIZONA.EDU
Subject: Calibration loose ends (was Re: [ITRDBFOR] crossdating)
Date: Sun, 29 Aug 2004 11:46:03 -0700
Reply-to: grissino@UTKUX.UTCC.UTK.EDU

This an attempt to tie up the loose ends from an earlier part of the discussion, the idea that calibration of the radiocarbon timescale be considered invalid, pending a better understanding of crossdating. Some of the previous posts seem to imply that measurements of the C-14 half-life depend on the calibration; in fact it can be determined by present-day laboratory measurements without reference to any old material, simply by observing the decay rate in a known quantity of the isotope. Physicists seem happy that beta decay isn't affected by mundane external influences, so the half life should be constant. If the amount of C-14 in a sample depends only on its age and the (constant) half life, a calibration curve from a collection of samples of known true age would be a diagonal straight line; but this would imply that each sample started with the same concentration of C-14. There are many effects that could change this concentration through time: variations in cosmic ray sources, changing solar activity, changes in the upper atmosphere, atmospheric circulation, uptake and release of carbon from large sinks and sources... etc. Given enough correctly dated samples, you can recover the sum of these variations from the form of the calibration curve. In practice, the most important variation appear to be on multi-millennial scales, with smaller fluctuations (wiggles) on century/multi-decadal scales superimposed on this.

Wood from crossdated tree rings provided the known-age reference material used in the calibration curves, and there were two main phases of work, the first of which roughed out the general form of the curve and hinted at the short-period structure, the second of which reconstructed the century-scale variations in detail using higher precision measurements. Contamination of old samples with C-14 of more recent origin is a widely recognized problem, addressed by physical and chemical pre-treatment protocols for the material. A couple of complicating effects that are of more interest from a tree-physiological point of view. Isotopic fractionation occurs along the entire chain of processes between carbon in the environment and its incorporation in the specific components of the wood that end up in the calibration samples. A ring forming in a particular year might

continue to accumulate C-14 in subsequent years. But people who work with C-14 are well aware of various corrections for isotopic fractionation, and the migration of carbon across ring boundaries has been the subject of several empirical investigations, notably using the stepwise change in C-14 concentrations following atmospheric nuclear tests in the 1950s and 60s as a tracer. The more recent phase of calibration work was substantially complete around 15 years ago, and was covered in an extensive series of journal articles and symposia.

Let's suppose we have been provided with a demonstration that crossdating is invalid: what would be the consequences for C-14 calibration? One of the most alarming would be that we would have to come up with a convincing explanation of how independent tree ring chronologies could be in error in precisely the same way---the known-age reference samples are not just from bristlecone pines, and crossdating within the network of oak chronologies is completely independent of the bristlecones. Both are completely self-supporting chains of inferences anchored in living trees and extending back into sub-fossil wood. There are published comparisons of paired calibration curves, with the absolute dates and C-14 concentrations based on oaks in one case, and on bristlecones in the other. My understanding of tree physiology is rudimentary at best, but surely when two such vastly different wood anatomies are involved there must be differences in the physiological constraints on wood formation. If potentially unidentified missing rings are supposed to be the most serious problem with the bristlecone chronologies, the oak chronologies should not be affected in any case, since they almost never include missing rings in this sense (although that's not to say they have no anatomical ambiguities that can confound crossdating). The crossdating error could not be merely a shared systematic bias; not only does the long term trend in the calibration curves derived from the two chronologies share a common non-linear trend, but the short-term fluctuations in C-14 concentration (wiggles) match between the two curves. There are small differences between calibrations derived from different geographical regions, but these have themselves formed the basis for further research and geophysical modeling.

The strengths of the two sets of chronologies are complimentary. Oaks may have almost no missing rings (sensu stricto) and provide larger volumes of wood for C-14 analysis, but the individual samples are only a few hundred years long, showing significant variations in growth with increasing pith age, and (particularly in the case of the sub-fossil wood) there will be uncertainties about the environment in which the tree was growing. Bristlecone pines give a much better chance of finding wood that has grown over periods of many centuries with no marked age-related trends, and there's a compelling continuity between the living trees and the remnant wood lying on the ground nearby.

An account of wood formation from a physiological perspective would undoubtedly be a beautiful thing in its own right, even if it had little to contribute to dendrochronology. Moreover one of my pet peeves is seeing people manipulate data as mere collections of numbers divorced from any underlying model---and in the case of dendrochronolgy the model has to be biological. But I'd number myself amongst those who can't see why our use of crossdating must await a reasonably complete physiological model of wood formation. By analogy, if the doctors in some traditional society are using a human physiology based on the balance or imbalance of the four humours, but they have a treatment for a particular disease that results in an 80% survival rate, as opposed to a %40 survival rate if it goes untreated, you're obviously better off slurping down their bitter potion first

and working out the explanation in current Western physiological terms
afterwards (if that's the only treatment option).

So even if at present our understanding of crossdating is largely
limited to statistical phenomenology, that may be good enough to live
with until something better comes along. That's not to imply that we
should be credulous, and automatically accept current practices simply
because great authorities have taken the same route: astronomers were
at one time expected to work as astrological consultants, casting
horoscopes for rulers and interpreting signs in the sky in terms of
current political affairs. There's no necessary reason to follow
Douglass' crossdating methods any more than we should follow Kepler's
example of casting horoscopes---unless they work. Although the seeming
effectiveness of crossdating could in principle be invalid, it
has been applied so widely that we would need presented with a very
strong critique before abandoning it.

I'm not really qualified to discuss crossdating and C-14 calibration
from a point of view of someone active in current research, but was
fortunate to be sitting on the sidelines of the oak calibration work
in the 80s, and just the other day Tom Harlan dropped by with the
oldest known absolutely dated bristlecone sample, so will offer
this as a kind of correction by proxy until any of the people
who've done the real work care to comment
 ---Martin.

429. 1094483447.txt
##
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@cgd.ucar.edu>
Subject: Re: question
Date: Mon Sep 6 11:10:47 2004
Cc: Professor David Taplin <coliemore@hotmail.com>, Ben Santer <santer1@llnl.gov>

    Tom,
        Ben should have seen the ERA-40 Report # 18. You can forward the JGR paper.
       WRT 1, it is difficult to say as it depends who's produced the values. For
HadCRUT2v, I
    think I've convinced the HC that the globe is (NH+SH)/2. If Peter Thorne did the
   calculations
    then this will be the case.
      There is another issue. Sometimes the trends over Jan79-Dec03 are calculated
from the
    300 months rather than the 25 years. Christy does this, I think.
       NCDC's Globe is probably the one domain. I've been doing some work with Russ
Vose at
    NCDC, which he's still to write up. Most of the differences were due to how the
globe
    was calculated. It is more informative to also include NH and SH as well as
globe in such
    tables. I'll forward a plot Tom Peterson produced a week or two ago.
    ERA-40 (2)comparisons are discussed in the ERA-40 report # 18 and the JGR
submitted
   paper.
    This also has comparisons by continent, which again are more informative. There
is a plot
    in that work from the full globe vs the CRU coverage. I wouldn't believe their
tropics.
   Also
    Antarctica is way off as well - at least where the surface data are located, so

I wouldn't have much faith in their values for the unmonitored parts.

On (3) I did some comparisons ages ago with Jim Angell's surface data from sondes. Jim's data was just noisier and I suspect LKS would be also. I've not done anything like this for ages. The closest would be the ERA-40 comparisons, which is much more extensive than the LKS network.

I might have a chance to do an LKS comparison if Dian sends me the co-ordinates.

Comparisons over 1958-2003 will be much more realistic, but the ERA-40/NCEP degrade prior to the 1960s. LKS would be better here. All sonde data look odd in the late 1950s to the early 1960s. The jump around 1976/77 has always intrigued me. It is bigger in some regions than others - I think it gets more credence because it is large over western North America. Kevin had a paper on this in BAMS in the late 1980s.

Cheers

Phil

At 15:57 04/09/2004, Tom Wigley wrote:

> Phil,
> On Sept. 13-17 I will be at a meeting at the Met Office to do with a report we are writing on trends in vert temp profiles as part of the US Climate Change Science Program (CCSP). It involves all the usual suspects. Seven chapters, the last of which is equivalent to a summary for policy-makers -- for which I am the lead author. Various people are updating data sets and doing calculations of trends, etc. Some of the surface numbers I found to be a bit disturbing -- so I am asking for your opinion. These are trends per decade for Jan. 1979 thru Dec. 2003 .....
>
> | SOURCE | GLOBE | 30S-30N |
> |---|---|---|
> | HadCRUT2v | 0.169 | 0.127 |
> | NCDC | 0.151 | 0.146 |
> | ERA40 | 0.113 | 0.032 |
> | LKS | 0.074 | 0.056 |
>
> (1) CRU and NCDC are consistent within the noise, but I have one question -- how do both calculate GLOBE?
> (2) ERA40 is marginally OK (relative to CRU) in GLOBE, but the tropics is alarmingly different. (The diff here accounts for the GLOBE difference.) Why is this? Which is better? Is this discussed in your paper with Adrian?
> (3) LKS is the surface data from the corrected LKS radiosonde data set. The difference here must be partly due to coverage issues. But I recall that years ago we saw a difference between surface sonde and CRU data. Have you done a like with like comparison (i.e., selecting the LKS sonde sites and extracting the corresp CRU (and NCDC, and ERA40 -- and (if possible) NCEP) data? This seems to be a pretty basic sanity check on the sonde data -- so, if you have not done this already, could you do it for me please?
> I think there is a nice little GRL paper here. For the CCSP we are also giving trends, etc. over 1958-2003. So the real need is for a full time series comparison over this period -- i.e., not just trends. In other words, what I would like you to produce is the monthly time series for the various data sets for the LKS coverage. If you don't know the LKS site locations, I can get these for you.
> Re going back to 1958, the sonde trop data have a well known (but not well explained) problem over roughly 1958 to 1964/5. I am curious

as to whether this shows up in the LKS surface record. I am also
curious about the apparent 1976 jump -- some people have made a
lot of noise about this, but I don't see it as a major item in the global
surface data. So the Q here is, is is apparent in the restricted coverage
of the sonde data?
I hope you can help. I am leaving here on Sept 7 to spend a few days
with a friend of mine in Plymouth -- you could contact me thru him (I
am copying this to him so you can see his email).
Thanx,
Tom.

Prof. Phil Jones
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School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK
---

430. 1094495798.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: wigley@cgd.ucar.edu
Subject: Sahel IJC paper
Date: Mon Sep 6 14:36:38 2004
Cc: santer1@llnl.gov

Tom,
You've probably seen this response to a truly awful paper in IJC. Aiguo did a really good job. Apparently, these two jerks have submitted a response to the comment. Wonder what they will say ? Adrian Chappell still thinks his analysis is correct !
Cheers
Phil

Prof. Phil Jones
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School of Environmental Sciences Fax +44 (0) 1603 507784
University of East Anglia
Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK
---

431. 1094752345.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: wigley@cgd.ucar.edu
Subject: Re: question
Date: Thu Sep 9 13:52:25 2004
Cc: santer1@llnl.gov

Tom,
Program and the input LKS file. Program is adapted from one I had. Ended up a

little
convoluted. Should work with any of the 4 CRU temp data files (CRUTEM2(v), HadCRUT2(v)).
For the Russian, grid point, changing 4 59 to 4 57 will give a box with data in from
1929.
3rd file is my unix run file - for files to channels.
Cheers
Phil
At 12:20 09/09/2004, D M R Taplin wrote:

Phil,
Thanx. Looks very interesting. I will look more when I get back to Boulder. It would
help if you sent the program (just to Boulder). Also what are the numbers listed at the
end of the LKS file?
Will you be reading email while away?
Tom.
====================
Professor David Taplin DSc
Coliemore House
Down Thomas Plymouth PL90BQ UK

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@cgd.ucar.edu>
CC: Professor David Taplin <coliemore@hotmail.com>, Ben Santer <santer1@llnl.gov>
Subject: Re: question
Date: Wed, 08 Sep 2004 13:44:44 +0100
Tom,
Here are some files to look at and think about. John Lanzante has sent me the
locations of
the 87 stations in the LKS dataset. I associated these with CRU 5 deg grid boxes and
calculated NH (based on 54 sites), SH (32) and Global (as one domain), so to get the
globe
the CRU way you need to average the NH and SH series (all to 3 deg places). The second
line in all the results files is the count of stations. I can do this as % area if you
want.
The CRU data I used is the file hadcrut2v, so this includes SST anoms over the
ocean.
I can repeat this with the land only file. Used the variance corrected version.
There are 4 files
1. The LKS stations. This is what John sent with the lat/long identifiers for the grid
boxes on
the front.
2-4 NH, SH and Globe as one domain results.
The first file has a fix in it. This is to pick up the 5 deg square (85-90S, 5W-0)
that has
the South Pole data. This square is where I've always put this data.
For the NH there were 54 sites and for the SH 32. Site 9 (WMO ID 21504) is always
missing,
even with hadcrut2v. The site is located on an island in the Laptev Sea.

There isn't a surface site anywhere near it. I could move the location and pick up the nearest CRU box, but it will be over 5 deg of lat and 10 deg of long away. It's somewhat unusual for sonde sites not to have a surface site near them. I guess it just doesn't report its surface data.

I'm here until Sept 15 then away for much of the time until end of October. I could send you the program, which should run with crutem2v or the non-variance adjusted versions, which you could pick up from the CRU web site.

Cheers

Phil

At 15:57 04/09/2004, Tom Wigley wrote:

Phil,

On Sept. 13-17 I will be at a meeting at the Met Office to do with a report we are writing on trends in vert temp profiles as part of the US Climate Change Science Program (CCSP). It involves all the usual suspects. Seven chapters, the last of which is equivalent to a summary for policy-makers -- for which I am the lead author. Various people are updating data sets and doing calculations of trends, etc. Some of the surface numbers I found to be a bit disturbing -- so I am asking for your opinion. These are trends per decade for Jan. 1979 thru Dec. 2003 .....

SOURCE	GLOBE	30S-30N
HadCRUT2v	0.169	0.127
NCDC	0.151	0.146
ERA40	0.113	0.032
LKS	0.074	0.056

(1) CRU and NCDC are consistent within the noise, but I have one question -- how do both calculate GLOBE?

(2) ERA40 is marginally OK (relative to CRU) in GLOBE, but the tropics is alarmingly different. (The diff here accounts for the GLOBE difference.) Why is this? Which is better? Is this discussed in your paper with Adrian?

(3) LKS is the surface data from the corrected LKS radiosonde data set. The difference here must be partly due to coverage issues. But I recall that years ago we saw a difference between surface sonde and CRU data. Have you done a like with like comparison (i.e., selecting the LKS sonde sites and extracting the corresp CRU (and NCDC, and ERA40 -- and (if possible) NCEP) data? This seems to be a pretty basic sanity check on the sonde data -- so, if you have not done this already, could you do it for me please?

I think there is a nice little GRL paper here. For the CCSP we are also giving trends, etc. over 1958-2003. So the real need is for a full time series comparison over this period -- i.e., not just trends. In other words, what I would like you to produce is the monthly time series for the various data sets for the LKS coverage. If you don't know the LKS site locations, I can get these for you.

Re going back to 1958, the sonde trop data have a well known (but not well explained) problem over roughly 1958 to 1964/5. I am curious as to whether this shows up in the LKS surface record. I am also curious about the apparent 1976 jump -- some people have made a lot of noise about this, but I don't see it as a major item in the global surface data. So the Q here is, is is apparent in the restricted coverage of the sonde data?

I hope you can help. I am leaving here on Sept 7 to spend a few days

with a friend of mine in Plymouth -- you could contact me thru him (I am copying this to him so you can see his email).
Thanx,
Tom.

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<< lksdata.out >>
<< lksnh7003v.dat >>
<< lkssh7003v.dat >>
<< lksgl7003v.dat >>

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432. 1096382684.txt
##########

From: Andy Revkin <anrevk@nytimes.com>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: mann's thoughts
Date: Tue, 28 Sep 2004 10:44:44 -0400

<x-flowed>
that is a useful way to look at it.

again, takeaway msg is that mann method can only work if past variability same as variability during period used to calibrate your method.

so it could be correct, but could be very wrong as well.
by the way, von storch doesn't concur with osborn/briffa on the idea that higher past variability would mean there'd likley be high future variability as well (bigger response to ghg forcing).
he simply says it's time to toss hockeystick and start again, doesn't take it further than that.

is that right?

At 09:40 AM 9/28/2004, you wrote:
>Dear Andy,
>
>our schematic figure is attached.
>
>Tim
>
>
>
>Dr Timothy J Osborn
>Climatic Research Unit

>School of Environmental Sciences, University of East Anglia
>Norwich NR4 7TJ, UK
>
>e-mail: t.osborn@uea.ac.uk
>phone: +44 1603 592089
>fax: +44 1603 507784
>web: http://www.cru.uea.ac.uk/~timo/
>sunclock: http://www.cru.uea.ac.uk/~timo/sunclock.htm

Andrew C. Revkin, Environment Reporter, The New York Times
229 West 43d St. NY, NY   10036
Tel: 212-556-7326, Fax: 509-357-0965 (via www.efax.com, received as email)

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433. 1096645745.txt
##########

From: Stefan Rahmstorf <regentage@gmx.de>
To: Eystein Jansen <eystein.jansen@geo.uib.no>
Subject: [Wg1-ar4-ch06] Ch6-Climate Sensitivity
Date: Fri, 01 Oct 2004 11:49:05 +0200
Reply-to: stefan@pik-potsdam.de
Cc: wg1-ar4-ch06@joss.ucar.edu

Hi co-authors,
here are some thoughts on what to say on climate sensitivity in our chapter - this is an
attempt to focus on the main, simple messages for policy makers. (I think we should try
retaining those important messages and not lose sight of them amidst all the details,
complexity and caveats.)
The main policy-relevant question could be phrased as follows: Does the past climate
history tell us how sensitive the climate system is to CO2?
I submit that the answers to this we get from different time periods are the following.
Deep Time:
Reconstructions are too uncertain (and boundary conditions too different, e.g. continents
in different places, different ocean circulation) to draw quantitative conclusions about
sensitivity to CO2, but there is clear evidence that times of high CO2 in Earth history
tend to be ice free (Royer et al. 2004). A second piece of evidence is the Late Paleocene
Thermal Maximum, which shows that the climate has responded by warming to a large carbon
release into the atmosphere. Just how large this carbon release was is not known, since
several origins of the carbon are possible, which have different isotope signature and
would thus imply different amounts. But the temperature response was large (6K), and if
anything this response would point to a high sensitivity.
Glacial-Interglacial Changes:
We have by now sufficiently good quantitative reconstructions of CO2 and other forcings as

well as temperatures in order to derive useful quantitative estimates of climate sensitivity. LGM was the most recent time in history in which CO2 concentration differed greatly from pre-industrial values, by as much as it does now. It is the closest test case for response to CO2 changes that we have.
There are two basic methods to derive climate sensitivity:
(i) Based on data analysis - e.g. Lorius et al. 1991 (concluding sensitivity is 3-4 K).
This method has the caveat that this sensitivity applies to colder climate, which may differ somewhat from that which applies in present climate as the strength of feedbacks is expected to depend on the mean climate (e.g., stronger snow-albedo feedback in colder conditions).
(ii) Based on combining data and models - e.g. Schneider von Deimling et al. 2004. Does not have the above caveat, but depends on models.
Lag of CO2 behind temperature does not imply a lack of CO2 effect on climate, since the lag is small (centuries, not millennia).
Holocene, last millennium
??
Overall conclusions
Qualitatively, climate history is at least consistent with the accepted CO2 sensitivity.
There is no evidence for much lower or much higher CO2 sensitivity (note that CO2 is not the only forcing). The more recent climate history (as far back as ice core data go) does allow quantitative inferences. The results of these estimates all lie within the IPCC range and provide strong support for this. Paleodata may even allow to reduce this range, since at least one study argues that values above 4K are very likely inconsistent with the reconstructed LGM climate: for high CO2 sensitivity, tropical cooling in the glacial should have been larger.
Cheers,
Stefan
_____ Wg1-ar4-ch06 mailing list Wg1-ar4-ch06@joss.ucar.edu http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06

434. 1097078296.txt
##########

From: Tom Wigley <wigley@cgd.ucar.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: past 1000 yr
Date: Wed, 06 Oct 2004 11:58:16 -0600

<x-flowed>
SEE CAPS

Tim Osborn wrote:

> Hi Tom - I'd be happy to contribute if I have something worth
> contributing! I'm a bit rushed today and away tomorrow, but can
> respond to further emails later in the week.

>
> At 14:31 03/10/2004, Tom Wigley wrote:
>
>> Caspar Ammann and I plan to publish some MAGICC
>> results for the past 100 years.
>
>
> Presume you mean 1000 years, hence relevance of ECHO-H/von Storch.

OOPS! YES.

>
>
>> Part of the reason is the new
>> solar forcing, as in my Science note with Peter Foukal.
>
>
> Yes I saw that. With a brief scan I didn't realise that you were
> presenting a new forcing history, just discussing reasons why
> long-term changes may be lower than previously estimated. But
> presumably you can use such reasoning to develop a new forcing history
> - or, better, a range or even a PDF of such histories. And then
> extend it using 14-C or 10-Be, or a combination?

WE SAY *NO* LOW FREQ FORCING. C-14/Be-10 ARE PROXIES FOR MAGNETIC FIELD
CHANGES. THERE
IS NO ADEQUATE THEORY RELATING THESE TO LUMINOSITY CHANGES -- IN FACT
THEORY SUGGESTS
THEY ARE *NOT* RELATED. SO WE ARE SUGGESTING A DIFFERENT FORCING
HISTORY, WITH
IMPLICATIONS AS IN THE FIGURE. NO SOLAR-INDUCED LIA, IN ACCORD WITH THE
PROXY CLIMATE
RECONSTRUXIONS. FURTHER, THERE IS SOME RECENT WORK SUGGESTING THAT PART
OF THE
C-14/Be-10 CHANGESW ARE DUE TOCHZNGES IN THE *EARTH'S* MAGNETIC FIELD.

>
>
>> So we
>> address both forcing and senstivity uncertainties. In
>> addition, the drift due to incorrect initialization is an issue.
>
>
> Surely not so in MAGICC? But yes, it is in GCMs and particularly so
> in ECHO-G.

OF COURSE WHAT I MEAN IS TO USE MAGICC TO QUANTIFY THE INITIALIZATION
'DRIFT'.

>
>
>> I have not yet read the Storch paper or your comment -- but
>> did you mention this problem?
>
>
> We said that ECHO-G had a redder spectrum than other model simulations
> (there was no room to say that it showed greater fluctuations, but we
> cited the Jones/Mann paper which has an intercomparison figure in
> it). We didn't talk about the reasons for this (drift early on,
> strong solar forcing throughout and no tropospheric aerosols to

> mitigate recent warming) because we'd already said that the simulation
> didn't necessarily represent real climate history.
>
>
>> Also, can you remind me just what was done with the ECHO
>> run?
>
>
> Main problem in terms of introducing "drift" (or "adjustment") was
> that they used a control run with present day CO2 as initial
> conditions. Although they allowed a 70-year spin-up (prior to AD
> 1000) to adjust back to pre-industrial CO2, this doesn't look long
> enough and the adjustment probably goes on for the first 400 years of
> the run - i.e. there is gradually disappearing cooling trend over this
> period. All based on MAGICC runs, but still fairly convincing
> (including non-zero heat flux out of the ocean in ECHO-G itself).

SEE THE STOUFFER PAPER IN CLIM DYN 23, 327 (2004).

>
>
>> If you have something to add on this, you can join as a co-author.
>
>
> I'm not quite sure what you plan, nor the input you need, but
> hopefully I can help.

WHAT I WOULD LIKE IS YOUR BEST ESTIMATE OF THE MAGNITUDE OF THE SPURIOUS
INITIALIZATION EFFECT IN
TERMS OF FORCING.

>
>
> Cheers
>
> Tim
>
>
> Dr Timothy J Osborn
> Climatic Research Unit
> School of Environmental Sciences, University of East Anglia
> Norwich NR4 7TJ, UK
>
> e-mail: t.osborn@uea.ac.uk
> phone: +44 1603 592089
> fax: +44 1603 507784
> web: http://www.cru.uea.ac.uk/~timo/
> sunclock: http://www.cru.uea.ac.uk/~timo/sunclock.htm
>
>
>

</x-flowed>

435. 1097159316.txt
##
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Ben Santer <santer1@llnl.gov>

Subject: Re: More vertical profile plots
Date: Thu Oct 7 10:28:36 2004

Ben,
Thanks for the plots. I gather from Karl that you'll be in Seattle and not at the HC review.
I'll be in Seattle also and am missing the HC review, so we can catch up on things.
Last week was the first LA meeting of AR4. You have likely been contacted by Kevin and also maybe by Brian Soden about writing something on tropopause heights.
It would perhaps be useful to send them these figures and maybe also to David Parker.
For our chapter Kevin is co-ordinating the U/A and circulation sections. I'm doing the surface T/P and extremes and the final summary. I've been too busy to think about anything yet ! We have a mix of abilities in the LAs, but Brian, David P, Dave Easterling and Albert Klein Tank of KNMI are solid. The Iranian, Argentinian, Romanian, Kenyan don't seem up to too much, but this is life in the IPCC - remember Ebby !
The fact that HadCRUT2v is close to PCM may be fortuitous, but good nonetheless. If you subsample PCM with CRU coverage, you say the PCM trend will reduce. The paper and report with Adrian shows that if you look at the full ERA-40 surface T data, then the reverse happens. Not a large increase though. Most comes from the SH, so there are issues of what ERA-40 is doing over the Southern Oceans, Antarctica and Australia are key. I'll be talking about this work in Seattle.
I don't have any IDAG work to give you - not done a lot. Plan to look at the 1740 event in Europe, when time permits. If you want any of my ppt for your IDAG talk, you can look through in Seattle.
Good to catch up in a weeks time. Hope you and Nick are well. Away next week in Delhi at a GCOS workshop.
Cheers
Phil
At 01:50 07/10/2004, you wrote:

Dear Jerry, Ram, and Jim,
Here are the profiles of zonally-averaged atmospheric temperature change that you requested. As I mentioned in yesterday's email, I've prepared a couple of different versions of these plots. First, there are two different analysis periods: January 1979 through to December 1999, and January 1958 through to December 1999. Second, temperature changes are expressed in two different ways: in terms of linear trends per decade, and in terms of the total linear changes over the two analysis period. So there are four different vertical profile plots:
-rw-r--r-- 1 bsanter climate 194436 Oct 6 16:27 ccsp_vp_lt_1979-1999.ps
-rw-r--r-- 1 bsanter climate 142312 Oct 6 16:27 ccsp_vp_lt_1958-1999.ps
-rw-r--r-- 1 bsanter climate 201997 Oct 6 16:43

ccsp_vp_tlc_1958-1999.ps
-rw-r--r-- 1 bsanter climate 198109 Oct 6 17:04
ccsp_vp_tlc_1979-1999.ps

All the relevant information is encoded in the file name: "lt" denotes linear trend, and "tlc" denotes total linear change. Personally, I have a preference for the total linear change plots. If you compare panel f (the PCM ALL forcing case) of the "tlc" plots for 1979-1999 and 1958-1999, the much larger total changes over the longer analysis period are visually obvious. This is not the case if changes are expressed in degrees C/decade.

I note that (as requested by Roger Pielke in Exeter), the plots are appropriately area weighted.

All profiles of zonally-averaged atmospheric temperature change are ensemble means. Each ensemble mean was calculated from four individual realizations. There is no subtraction of control run drift, which probably is not a significant factor at this point in the perturbation experiments.

I've also updated the two plots that I sent you yesterday, which show global-mean and tropical-mean profiles of atmospheric temperature change. These plots now include observed near-surface temperature trends, estimated from HadCRUT2 and HadCRUTv (the latter is the variance corrected version of HadCRUT2). PCM ALL and HadCRUT near-surface temperature changes are in good agreement, both for global- and tropical averages. I'm pretty sure that in the global-mean case, subsampling PCM ALL results with HadCRUT coverage would yield a slightly warmer PCM ALL 2m temperature trend (in view of the muted warming of 2m temperatures at high southern latitudes in ALL; these areas are not well sampled in HadCRUT).

It would be nice to show these plots of global- and tropical-average changes in Chapter 5. I think they make some useful points.

Hope all of this is helpful,

With best regards,

Ben

(P.S.: I'd like to acknowledge the assistance of Charles Doutriaux and Mike Wehner in producing these plots. Considerable data processing was involved in generating these six figures).

--

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436. 1097540855.txt

##########

From: Eystein Jansen <eystein.jansen@geo.uib.no>
To: Keith Briffa <k.briffa@uea.ac.uk>, wg1-ar4-ch06@joss.ucar.edu
Subject: Re: [Wg1-ar4-ch06] IPCC last 2000 years data
Date: Mon, 11 Oct 2004 20:27:35 +0200

<x-flowed>
Hi Keith,
I can take a stab at the THC bit (not strong
evidence so far for linkages to
multidecadal/century scale changes, but cannot be
ruled out) the marine evidence from the North
Atlantic (14C chronological control), and some
aspects of tropical/high latitude linkages.
Eystein

At 17:00 +0100 11-10-04, Keith Briffa wrote:
>Friends and authors (especially Ricardo, Olga,
>Fortunat, David, Ramesh, Zhang, Dan, Eystein and
>Valerie)
>Now back from travels (until Wednesday when off to Austria for a few days)
>I thought it best to suggest a break down for
>the writing of the data section for the last
>2000 years of the IPCC palaeoclimate chapter.
>Please see the outline produced at the meeting.
>We have 4 IPCC pages . I will write a short
>intro linking to the instrumental data with
>links to Chapters 3-5. I will coach this in a
>general introduction to this section that
>addresses the points listed in the initial notes
>(namely how we use the various high , and few
>low, resolution data to construct regional and
>large-scale temperature variability , and where
>possible, gain insight into hydrologic
>variability. I will say we use models to get
>insight into methodology and to explore regional
>coverage and seasonality issues and we use
>control and forced model runs to look at
>sensitivity and detection issues , but also use
>date to test model variability and sensitivity .
>I can first go at the NH (SH) Spaghetti diagram
>discussion and hopefully you will pick up the
>regional aspects of the temperature and
>precipitation (moisture) variability .
>Rather than me say - I would like you to come
>back with the major areas you will cover , but
>these may best be done in terms of
>climatologically meaningful regions - ie
>relating to the ENSO, NAM, PDO , AAO, monsoon
>areas - then we could fill in the remaining
>regions if significant non overlap in areas is
>apparent (Eurasia, non-monsoon china etc) . We
>do not want a list of every paper ever written ,
>but a selection of (the better) work that you
>feel has regional relevance (and some length
>presumably). THe other alternative is just to
>divide up the world to our own regions and then
>discuss the climate indices separately. This
>would likely be easier to do . Let me know what
>you think. Either way , we also should have a
>specific discussion of forcings at high
>resolution , and Fortunat, Valerie could cover
>solar and volcanic , perhaps Eystein discussing
>what evidence there is for THC change . The
>knotty issue of THC versus NAO and the link to

>model theories/models could go here - or
>perhaps later in the section 6.4.3.2 ? Davis
>what say you about this? The same is true of
>ENSO links to terrestrial precipitation patterns
>and temperature?
>I don't like the idea of dealing wit quasi
>periodicities separately , but rather wit the
>regional discussions eg North American drought.
>The question of LIA , MWP will come up in the
>large scale average discussion but you can also
>address it in the regional discussions , but in
>a critical and quantitative way. I would like to
>see the evidence for extremmes/abrupt change
>from the regional syntheses and then see if we
>have enough to define and discuss the issue
>separately. Olga could you pick up on the
>glacial variations (perhaps with links to models
>also?)
>
>So come back to me asap to let me know
>impressions and regional/variable focus you all
>wish to pick up. Ricardo will obviously do North
>South linkages as per the PEP1 transect , but
>what about along PEP2 and 3/ WE may have to pick
>this up in the light of the regional data. Can
>you also let me know if/who you might be asking
>to help with writing . Peck , I would still
>rather have Mike Mann in , so what is the story
>here - can I ask him? Suggestions for summary
>Figures still welcome - I would like to have a
>High lat , mid lat , low lat transect type
>figure for temperature , possibly along each PEP
>transect - with longest instrumental data . A
>forcing diagram is also a must - but could
>combine Holocene and "blow up " last 2000 years.
>
>Best wishes
>Keith
>
>--
>Professor Keith Briffa,
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>Phone: +44-1603-593909
>Fax: +44-1603-507784
>
>http://www.cru.uea.ac.uk/cru/people/briffa/
>_____
>Wg1-ar4-ch06 mailing list
>Wg1-ar4-ch06@joss.ucar.edu
>http://www.joss.ucar.edu/mailman/listinfo/wg1-ar4-ch06

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_____
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The Bjerknes Training site offers 3-12 months fellowships to PhD students
More info at: www.bjerknes.uib.no/mcts

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437. 1097785771.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@virginia.edu
Subject: Re: comment Von Storch?
Date: Thu Oct 14 16:29:31 2004

Mike,
FYI.
I met this guy in Utrecht last week at Albert Klein Tank's PhD ceremony. It appears from
many media reports that people really believe that their run is an ALTERNATE to yours -
based
on no proxy data. Even Hans has sent an email around to this effect, but he obviously
isn't
making it as clear as I've just done to this Dutch journalist. I think he might be being
clear with
fellow scientists and economical with the truth with journalists, i.e. not directing them
down the
correct path when he sees them going down the wrong one.
I should see Ray next week in Seattle at a DoE meeting.
Cheers
Phil
Dear Karel,
I have only got back from a meeting this morning. I see you have also had a long reply
from
Mike Mann about the von Storch paper.
Basically the von Storch et al paper is a discussion of the methodology used in the
Mann,
Bradley Hughes papers from 1998, 1999. It doesn't contain any new nor any observed proxy
data. It is entirely a model study. Therefore, it cannot produce a record for the last
millennium,
it cannot claim that the Medieval Warm Period was warmer than today, nor that the Little
Ice
Age may have been colder than MBH says.
It is really alarming that many media people (including yourself) have been taken in.
What the
von Storch et al paper is about is a climate model run - just one simulation. All it uses
is

an estimate of past variations in solar forcing and volcanic eruptions and more recently
anthropogenic changes in greenhouse gases and sulphate aerosols.
As I said the paper in a methodological critique of MBH, nothing more than that. It IS
NOT
an alternative to MBH. It also not based on ANY paleoclimatic data. If you believe it, you
are putting everything on the model being correct and that their best guess at the past
history
of forcing as being correct.
Regards
Phil

At 15:28 13/10/2004, you wrote:

Dear professor Jones,
(We met ten days ago in Utrecht, when Albert Klein Tank got his PhD).
I am a science journalist of the Dutch daily newspaper NRC Handelsblad in Rotterdam
([1]www.nrc.nl).
I try to write an article about climate (surface temperature) reconstruction as far back
as the year 1000 - the well know Mann, Bradley, Hughes (1998 and 1999) research.
The reason is, of course, the publication of the article of Von Storch, Zorita, c.s. in
Science-online (30 september). Von Storch claims that the statistical approach of Mann
c.s. produced a serious underestimation of the low frequency (long term) oscillations
in global temperature. The conclusion could be that the Medieval Warm Period was in fact
warmer than today. And the recent warming is - after all - not so special.
Can you in a few words - and for a general public - give a comment on the paper? Does it
make sense? It seems pretty convincing to me.
Can you help me?
Waiting for your reply,
sincerely yours,
Karel Knip
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References

1. http://www.nrc.nl/

##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: John.Birks@bot.uib.no,masson@lsce.saclay.cea.fr,
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oyvind.lie@bjerknes.uib.no , joos@climate.unibe.ch , juerg@giub.unibe.ch , Elsa
Cortijo <Elsa.Cortijo@lsce.cnrs-gif.fr>, j.holmes@ucl.ac.uk,
harrye@ldeo.columbia.edu, jgoqam@iiqab.csic.es, mschulz@geo.palmod.uni-bremen.de
Subject: IMPRINT Budget (Work package 1)
Date: Wed Oct 20 13:49:34 2004

   Dear Partners in Workpackage 1 of IMPRINT,
   today is the deadline by which Eystein requested input as regards the
   reworked (and necessarily much shortened), proposal document. We have also been
making some
   effort to consolidate the indicative budgets that most of you have sent to us.
   We now need to transfer these figures to Eystein , even though a few partners
have not
   supplied numbers to us , though they may have sent them to Eystein directly.

   It is clear that we are now close to 30 partners in Workpackage 1 alone, and have
   indicative budget requests totaling well over the nominal 5 million Euro
originally
   allocated. In fact , the likely total with all partner requests included is
likely to be
   nearer to 10 million!
   We have been given a (very unofficial) hint from Brussels that an "appropriate"
total
   project request of about 17 million for IMPRINT might be sensible , with a final
figure ,
   if the project ever gets accepted, of 15 million being possibly awarded (subject
of course
   to referees' comments and subsequent reorganisation of priorities).
   The simple message is that Eystein will now have to make an executive decision as
to the
   total amount requested .
   If we ever get that far, reorganised budgets will have to be decided on the basis
of very
   specific
   work plans that will need to formalised for a second submission - especially as
they relate
   to the justification for field work and new data analyses. We also need to budget
for the
   involvement of non-partners , possibly using a mixture of workshop and minor
funding awards
   to facilitate data collection etc.
   It has been made clear that new practical work campaigns would not be sanctioned
across all
   Tasks
   in Workpackage 1 . Rather, the bulk of work would involve
re-dating/interpretation of

mostly existing data and reconstructions of forcings and climate . Specific cases will have
to be made to justify sampling and processing of new data.
Thanks to all of you for your help and thanks to Eystein for taking on the enormous task of
organising this proposal .
Keith and Tim

--
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[1]http://www.cru.uea.ac.uk/cru/people/briffa/

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/

439. 1098388401.txt
##########

From: "Rob Wilson" <rjwilson_dendro@blueyonder.co.uk>
To: <K.briffa@uea.ac.uk>
Subject: data - Quaternary Science Reviews 19 (2000) 87-105
Date: Thu, 21 Oct 2004 15:53:21 +0100
Reply-to: "Rob Wilson" <rjwilson_dendro@blueyonder.co.uk>

Hi Keith,

When would be a good time tomorrow (or next week) to phone you about the data you have
available at your website from your QSR 2000 paper.

I am particularly interesting in using the long chronologies from the Polar Urals (Yamal)
and Tornetrask.

This is for Gordon's and Rosanne's NH temp recon update, so I thought I should have a chat
with you before using the data.

all the best

Rob

440. 1098472400.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>

To: Tom Wigley <wigley@cgd.ucar.edu>
Subject: Re: MBH
Date: Fri Oct 22 15:13:20 2004
Cc: santer1@llnl.gov

Tom,
Just got the Science attachments for the von Storch et al. paper for Tim and Keith, so
I thought you might like to see them. I've just sent a reply to von Storch as he claims
his model is a better representation of reality than MBH. How a model that is only given
past forcing histories can be better than some proxy data is beyond me, but Hans seems
to believe this. The ERA-40 report and JGR paper are relevant here. ERA-40 is not of
climate quality. There are differences and trends with CRU data before the late 1970s
and again around the mid-1960s that should include other variables that are calculated.
It is so bad in the Antarctic that ERA-40 rejects most of the surface obs (because they
get little weight) and they don't begin to get accepted until the late 1970s. Conclusion
is that
you can't consider ERA-40 for climate purposes. Maybe the next generation, with a
considerable
efforts in getting all the missing back data in and changes to weights given to surface
data might
mean the 3rd generation is better.
I shouldn't rabbit on about this as I have to go home to drive with Ruth to Gatwick
for
our week in Florence. A lot of people criticise MBH and other papers Mike has been
involved in, but how many people read them fully - or just read bits like the attached.
The attached is a complete distortion of the facts. M&M are completely wrong in virtually
everything they say or do. I have sent them countless data series that were used in the
Jones/Mann Reviews of Geophysics papers. I got scant thanks from them for doing this -
only an email saying I had some of the data series wrong, associated with the wrong
year/decade.
I wasted a few hours checking what I'd done and got no thanks for pointing their mistake
out
to them.
If you think M&M are correct and believable then go to this web site
[1]http://cgi.cse.unsw.edu.au/~lambert/cgi-bin/blog/
It will take a while to get around these web pages and you've got to be a bit of nerd and
know
the jargon, but it lists all the mistakes McKittrick has made in various papers. I bet
there isn't
a link to this on his web site. The final attachment is a comment on a truly awful paper
by

McKittirck and Michaels. I can't find the original, but it's reference is in this. The
paper didn't
consider spatial autocorrelation at all. Fortunately a longer version of the paper did get
rejected by IJC - it seems a few papers are rejected !
Point I'm trying to make is you cannot trust anything that M&M write. MBH is as good a
way of putting all the data together as others. We get similar results in the work in the
Holocene in 1998 (Jones et al) and so does Tom Crowley in a paper in 1999. Keith's
reconstruction is strikingly similar in his paper from JGR in 2001. Mike's may have
slightly less variability on decadal scales than the others (especially cf Esper et al),
but
he is using a lot more data than the others. I reckon they are all biased a little to the
summer
and none are truly annual - I say all this in the Reviews of Geophysics paper !
Bottom line - their is no way the MWP (whenever it was) was as warm globally as the
last 20 years. There is also no way a whole decade in the LIA period was more than 1 deg C
on a global basis cooler than the 1961-90 mean. This is all gut feeling, no science, but
years of experience of dealing with global scales and varaibility.
Must got to Florence now. Back in Nov 1.
Cheers
Phil

At 20:46 21/10/2004, you wrote:

Phil,
I have just read the M&M stuff critcizing MBH. A lot of it seems valid to me.
At the very least MBH is a very sloppy piece of work -- an opinion I have held for some time.
Presumably what you have done with Keith is better? -- or is it?
I get asked about this a lot. Can you give me a brief heads up? Mike is too deep into this to be helpful.
Tom.

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References

1. http://cgi.cse.unsw.edu.au/~lambert/cgi-bin/blog/

441. 1101133749.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Adrian.Simmons@ecmwf.int, santer1@llnl.gov

Subject: Fwd: Re: K&C (fwd)
Date: Mon Nov 22 09:29:09 2004
Cc: wigley@ucar.edu

Adrian and Ben,
Roger Pielke did send this to me over the weekend, so he's being honest in one respect. I still think he's reading far too much into NCEP1. The bottom panel of their Fig1 shows both CRU and GHCN (-ERA40) having no difference over the period from the late 1960s. If the obs assimilated before 1967 (even in the US) were improved, the apparent drop before might disappear.
Cheers
Phil

Date: Fri, 19 Nov 2004 18:35:58 -0700 (MST)
From: Roger Pielke <pielke@atmos.colostate.edu>
To: p.jones@uea.ac.uk
cc: wigley@cgd.ucar.edu
Subject: Re: K&C (fwd)
X-UEA-MailScanner-Information: Please contact the ISP for more information
X-UEA-MailScanner: Found to be clean
Phil-
FYI; thank you for sharing your paper. I have circulated the attached to our CCSP Committee with the permission of Eugenia and Ming, and want to also share with you.
The conclusion from my own work with the NCEP reanalysis is that it is appropriate for trend assessments if integrated metrics are used (thickness for example), and for regions where the regional trend signal is quite large. We have published on both of this issues. One value-added of reanalyses is that since the winds are monitored independently of the temperatures, they provide information on the horizontal layer averaged temperatures in the mid- and high-latitudes, which helps adjust, to some extent, biases in the temperatures.
Also, as we have shown with regional data (e.g. Florida) and others have shown elsewhere (e.g. Andy Pitman for Australia) there is a clear land use change signal on surface temperature. This provides independent evidence that the Kalnay and Cai results should be expected.
Roger
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Roger A. Pielke, Sr., Professor and State Climatologist
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Phone: 970-491-8293/Fax: 970-491-3314, Email: pielke@atmos.colostate.edu
VISIT OUR WEBSITES AT: [1]http://blue.atmos.colostate.edu/
and [2]http://climate.atmos.colostate.edu
---------- Forwarded message ----------
Date: Fri, 19 Nov 2004 11:04:42 -0700 (MST)
From: Roger Pielke <pielke@atmos.colostate.edu>
To: _NESDIS NCDC CCSP Temp Trends Lead Authors
<CCSPTempTrendAuthors.NCDC@noaa.gov>, chris.folland@metoffice.gov.uk,
peter.thorne@metoffice.gov.uk
Cc: Eugenia Kalnay <ekalnay@atmos.umd.edu>, Ming Cai <cai@huey.met.fsu.edu>
Subject: Re: K&C (fwd)
Resent-Date: Fri, 19 Nov 2004 11:05:15 -0700
Resent-From: CCSPTempTrendAuthors.NCDC@noaa.gov
Hi All
I requested to Ming Cai and Eugenia Kalnay that they respond to the comments regarding their work. The response is forwarded to you in this e-mail.
This debate, of course, should really take place in the literature. There

has been, however, in my view an unfortunate change over time where reviewers who disagree with already published work recommend rejection of subsequent work rather than letting the community view and assess the different perspectives on a science issue. Our report has to make sure it is inclusive, in order to avoid this pitfall.
An unbiased discussion of the K&C results, and ways to resolve the disagreement through hypothesis testing, should be included in the appropriate chapters.
Roger
--
++
Roger A. Pielke, Sr., Professor and State Climatologist
1371 Campus Delivery, Department Atmospheric Science,
Colorado State University, Fort Collins, CO 80523-1371,
Phone: 970-491-8293/Fax: 970-491-3314, Email: pielke@atmos.colostate.edu
VISIT OUR WEBSITES AT: [3]http://blue.atmos.colostate.edu/
and [4]http://climate.atmos.colostate.edu
---------- Forwarded message ----------
Date: Fri, 19 Nov 2004 12:16:27 -0500
From: cai <cai@met.fsu.edu>
To: Roger Pielke <pielke@atmos.colostate.edu>
Cc: Ming Cai <cai@met.fsu.edu>, Y. K. Lim <yklim@met.fsu.edu>,
Eugenia Kalnay <ekalnay@atmos.umd.edu>
Subject: Re: K&C
Dear Roger,
Attached is the preliminary summary report on our recent work on the estimate of land-use-change climate impact using the reanalysis. Very fortunately, we had secured a one-year funding from NSF starting last August. Despite a short time period, we have already produced sufficient results to confirm the robustness of our original work using different datasets that have the state-of-art quality.
Here I just want to add one more comment about Simmons et al. paper. Basically, they claimed that the difference between the ERA40 and CRU is very small and therefore, our method is not applicable if the reanalysis is as good as the ERA40. There are two things that are incorrect in their claims. First of all, if the reanalysis were made to be exactly the same as the observations, by definition, there would be no difference between reanalysis and the surface observations. Since the ERA40 was obtained by directly assimilating the CRU surface observations whereas the NNR didn't use any surface temp. observation, it is natural to expect that the difference between the surface observation and ERA40 is small. Second, Simmons et al. manually reduces the difference between the ERA40 and CRU by setting the mean difference between the ERA40 and CRU from 1987 to 2001 be ZERO. As a result, the difference "LOOKs" very small in recent years. However, the difference from 1961 to 1985 has to be larger (otherwise, they would make an error in their plot). In other words, by doing so, the gap between the ERA40 and CRU appears decreasing in time rather increasing in time as shown in KC and in the new figure 1 in the attached file (which is the same as Simmons et al. paper except we reset the 1960-70 to be zero in order to see how the POSITIVE gap increases in time). If we closely examine their figures, we will see by applying their treatment, the gap between CRU and reanalysis is a NEGATIVE one (e.g., CRU is below ERA40 from 1960 to 1980) and such a NEGATIVE gap decrease in time is equivalent to that the POSITIVE gap increases in time as found in KC from the NNR data (e.g., the CRU becomes more above the ERA40). So Simmons et al's results actually CONFIRM our findings rather discredit our finding. We actually reproduced Simmons et al calculations and confirm that their results are correct (see the second attached figure, which is identical to Fig.1 in our preliminary report except the NEGATIVE gap is used and 1-year running mean was applied as in Simmons et al). But their interpretations are incorrect.
I appreciate if you could also forward the email to the CCSP authors.
Let me know if you want to me to reply to Tom and CCSP co-authors

directly.
Regards.
Ming
The report:
The replica of one of the key figures in Simmons et al.
On Nov 18, 2004, at 4:53 PM, Roger Pielke wrote:

Tom-
 Since we have not seen the paper, we cannot make any judgements on the robustness of that paper in showing that the Kalnay and Cai work is "flawed". I expect to have a summary by Eugenia and Ming tomorrow, however, which will address the published concerns on their work, and will
forward to the Committee. Please forward us a copy of the Simmons et al paper.
 I also would like a response to my MWR Florida paper where we specifically show the dominant role of documented land use change in peninsular Florida in the 20th century on July-August surface air temperature change. Or Andy Pitman's work who shows a major effect on temperature trends in south-western Australia due to land use change. This work, and others like it, support the conclusions of Kalnay and Cai
on a major role of land surface processes on surface temperature trends.
 How do you reconcile those independent conclusions with the paper you list above?
Roger
--
++ ++++++
Roger A. Pielke, Sr., Professor and State Climatologist
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VISIT OUR WEBSITES AT: [5]http://blue.atmos.colostate.edu/
and [6]http://climate.atmos.colostate.edu
On Thu, 18 Nov 2004, Tom Wigley wrote:

Date: Thu, 18 Nov 2004 14:28:16 -0700
From: Tom Wigley <wigley@cgd.ucar.edu>
To: CCSP Authors <CCSPTempTrendAuthors.NCDC@noaa.gov>
Subject: K&C
Resent-Date: Thu, 18 Nov 2004 14:28:17 -0700
Resent-From: CCSPTempTrendAuthors.NCDC@noaa.gov
Folks,
Roger makes the point that there is no comprehensive assessment of this
paper.
There is ... It is in a paper that has, I believe, been accepted by JGR
atmospheres.
A.J. Simmons, P.D.Jones, et al. "Comparison of trends and low-frequency
variability in CRU,
ERA-40 and NCEP/NCAR".
I think the conclusion is that the K&C paper *is* flawed.
Tom.

Ming Cai
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References

1. http://blue.atmos.colostate.edu/
2. http://climate.atmos.colostate.edu/
3. http://blue.atmos.colostate.edu/
4. http://climate.atmos.colostate.edu/
5. http://blue.atmos.colostate.edu/
6. http://climate.atmos.colostate.edu/

442. 1101243716.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: v.jones@geog.ucl.ac.uk
Subject: first go
Date: Tue Nov 23 16:01:56 2004
Cc: v.shishov@uea.ac.uk

Viv
attached is the text you sent with some suggestions and comments (track changes must be
on).
I am also sending a small piece of text that could be expanded if needed (this to be
inserted where you describe the treering input) - but at this stage I think you need to
have a look at comments and consider the specifics of the lake and tree sampling (the
latter if any).
I thought it best to send these comments rather that plough on doing stuff you don't want.
I think the "hook" needs to be the important opportunity to assess recent changes in lake
and tree productivity and see if any evidence for response to climate , as well as
searching for unprecedented evidence of climate change. I realise this is predominantly a
lake project with a link to trees and models , but the links must be more than token . I
can provide more background as to where we are with tree-ring work in Euro-Siberia if
needed . I think the model stuff also needs specific justification . Is Simon going to
contribute here?
Don't get hung up on the "decline or changing sensitivity issue" in trees . This is NOT a
great problem in Scandinavia, Ural/Yamal and is anyway a divergence in trend and quite
subtle and evident in wood density mostly. We are also of the opinion that it could be

partly a statistical processing artifact - we are exploring this now.
If you plough through my comments and suggestions and then return the text with specific
requests of what you wish to do I will then try to oblige thursday
cheers
Keith

--
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[1]http://www.cru.uea.ac.uk/cru/people/briffa/

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/

443. 1101850440.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Martin Todd <mtodd@geog.ucl.ac.uk>
Subject: Re: NERC application
Date: Tue Nov 30 16:34:00 2004

Martin
in response to Nadia's message and our talk - consider the following as regards title and
objectives
Title
The precedence of Ecological Responses to 20th Century Climate changes in Arctic Lakes and
Trees
Suggested Objectives
We will quantify how the changes in 20th century Arctic climate (including mean and
variability) are reflected in recent and past lake sediment records. We will determine the
response of lake ecosystem parameters and the relationships with specific climatic
controls.
We will define the character of variability in different natural archives contained in
dated sediments reaching back over 2000 years. We will generate well-calibrated ,
high-resolution (decadal to centennial time scales) estimates of past summer climate
variability over this time in western Arctic Siberia.
We will compare the lake sediment data with evidence of tree-growth and associated summer
climate changes , based on selected updating of an extensive, existing network of
chronologies, including long sub-fossil series extending back more than 4000 years in Yamal
and Taimyr. These data (with perfect inter-annual dating accuracy) will be reprocessed to
provide summer temperatures specifically representative of annual, decadal and centennial
timescales.

We will determine (for the first time) the extent to which the independent proxy-based
summer climate histories concur or disagree and explore the extent to which they
demonstrate the precedence of recent (20th century) climate trends in a multi-millennial
context. By comparing this evidence with the output of state-of-the-art GCM experiments ,
simulating climate changes in the Arctic over the last 500 to 1000 years, we will explore
the degree to which recent changes in Arctic lakes (and tree-growth rates) are attributable
to anthropogenic as opposed to natural climate changes.
At 13:55 30/11/2004, you wrote:

Hi keith,
The submission deadline for the NERC grant with Viv Jones is imminent.
She's getting in a bit of a panic. I wonder whether you have some text
already prepared to describe the details of the ECHO-G experiments. I
could get the information but will have to dig in the lierature. I was
hpoing you would have a summary paragraph from the SO&P
documantaton similar to the one we have written about the HADCM3 exp
Thnaks
Martin
****
Martin Todd University Lecturer Department of Geography
UCL (University College London)
26 Bedford Way
London WC1 8HR
email m.todd@geog.ucl.ac.uk
****

--
Professor Keith Briffa,
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[1]http://www.cru.uea.ac.uk/cru/people/briffa/

References

1. http://www.cru.uea.ac.uk/cru/people/briffa/

444. 1101999700.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Tom Wigley <wigley@cgd.ucar.edu>
Subject: Re: New version of Chapter 4
Date: Thu Dec 2 10:01:40 2004
Cc: "Folland, Chris" <chris.folland@metoffice.gov.uk>, Thomas R Karl <Thomas.R.Karl@noaa.gov>, Ben Santer <santer1@llnl.gov>

Dear Toms, Chris and Ben,
If large-scale is important (as said by Tom W), I can't see how microclimatic
issues that Roger goes on about can be that important. Maybe when you all
meet at the delightful Chicago Airport Hilton, you can remind him of spatial
degrees of freedom.

Is the NOAA Tsurf used the new Smith and Reynolds (2005) spatially infilled surface dataset? If this is the case maybe Ben could do a plot of NOAA minus HadCRUT2v?

I have a plot that David Parker produced of Smith and Reynolds (2005) over land and Jones and Moberg (2003) land (as smoothed global averages) from 1880. Prior to about 1960 the SR dataset is always about 0.15 warmer than JM. This looks likely due to infilling with 61-90 averages (i.e zeroes) over the Antarctic and some continental interiors of S. America, Africa, western China and Australia (where there are no obs pre early 1950s, 1956 for the Antarctic). SR should be OK for 1979-99 and be very similar to HadCRUT2v.

Cheers

Phil

At 23:31 01/12/2004, Roger Pielke wrote:

Tom-

One issue to sort out with respect to "VTT" remains whether there are unrecognized biases in the surface data. This issue is very much relevant if, as seems the case from Phil Jones's e-mail, the "raw data" that has been used has such large overlap among the different surface analyses. If this is the case, there are not three independent assessments of surface temperature trends. Moreover, unlike the MSU data, there are inhomogeneities associated with the diverse locations of each surface monitoring site (which have microclimate changes over time).

This issue is also very much a tropical issue as this is where large land use/land cover change has occurred in the satellite era (photographs rather than written documentation would really help in this assessment, as we have proposed).

Roger

--

++

Roger A. Pielke, Sr., Professor and State Climatologist
1371 Campus Delivery, Department Atmospheric Science,
Colorado State University, Fort Collins, CO 80523-1371,
Phone: 970-491-8293/Fax: 970-491-3314, Email: pielke@atmos.colostate.edu
VISIT OUR WEBSITES AT: [1]http://blue.atmos.colostate.edu/
and [2]http://climate.atmos.colostate.edu

On Wed, 1 Dec 2004, Tom Wigley wrote:

> Date: Wed, 01 Dec 2004 16:15:01 -0700
> From: Tom Wigley <wigley@cgd.ucar.edu>
> To: "Folland, Chris" <chris.folland@metoffice.gov.uk>
> Cc: Thomas R Karl <Thomas.R.Karl@noaa.gov>,
> Roger Pielke <pielke@atmos.colostate.edu>,
> Phil Jones <p.jones@uea.ac.uk>, carl mears <mears@remss.com>,
> CCSPTempTrendAuthors.NCDC@noaa.gov
> Subject: Re: New version of Chapter 4
>
> Chris et al.,
>
> I do not see this as high priority. We are supposed to be looking at
> *VTT*. Uncerts/diffs in individual data sets are relevant, of course, but
> what is currently missing is a map (maps) of sfc vs trop trend diffs.
> We are meant to be addressing a problem that we have made
> clear at the global and tropix scale -- but just *where* are the problem
> areas? (I think Carl showed us such a map previously -- we need this,
> or similar, or more, in the report since it really is the crux of the
> problem.)
>
> Ideally we need sfc minus MSU LoTrop (A), sfc minus MidTrop
> (UAH (B) and RSS(C)) to at least look at, and decide which is/are best to

> show. I imagine this will have some bearing on Roger Pielke's concerns
> re LULC. If the biggest differences are over the oceans (and from memory
> this is the case, worst in the SH), then sorting this out would arguably
> be more important than sorting out LULC effects. It would be hard to
> argue (albeit not impossible) that teleconnections from LULC in (e.g.)
> North America, or even the Amazon Basin, are responsible for trend diffs
> over the South Pacific
>
> In Ch. 1 there is a correlation map -- this is pretty useless in my
> view, altho
> it would be interesting to compare the correl map with an equiv trend
> diff map.
>
> Ch. 3 has maps of the trends at sfc, mid trop, lo strat -- so we are close
> to trend diff map. But even those who might be brilliant enough to produce
> the trend diff map in their heads will be thwarted, becoz the mid trop map
> in Ch. 3 uses the average of UAH and RSS. Good grief! This really is
> carrying political correctness too far. Please, please John L et al.,
> replace
> the mid trop panel in 3.6.2.3 by separate panels for RSS and UAH.
>
> The next in my list of related wishes is a map of the RSS minus UAH trend
> diffs (D). Eyeballing A, B, C and D together could be interesting.
>
> I would put these things right at the top of my wish list for Chicago.
>
> Tom.
> ========================
>
> Folland, Chris wrote:
>
> >Tom
> >
> >Can you get Russ Vose to look at the issues of data overlap and local
> >and regional similarity. My original suggestion was to compare trends
> >over 1958-2003 and 1979-2003 at each grid point in the two data sets and
> >also over larger (regional) areas. This would go to the heart of any
> >differences in the context of this report, is easy to do, and can be
> >plotted on a pair of maps with a third "difference in trend" map for
> >each period. Where differences are large, a more detailed look at the
> >data can be done. It might even show up errors! Even the first analysis
> >on its own should give enough information to sharpen up well the current
> >speculative text and can be done perhaps in parallel with NRC review.
> >
> >Chris
> >
> >Professor Chris Folland
> >
> >Head of Climate Variability Research
> >
> >Global climate data sets are available from [3]http://www.hadobs.org
> >
> >Met Office, Hadley Centre, Fitzroy Rd, Exeter, Devon EX1 3PB United
> >Kingdom
> >Email: chris.folland@metoffice.gov.uk
> >Tel: +44 (0)1392 886646
> >Fax: (in UK) 0870 900 5050
> > (International) +44 (0)113 336 1072)<[4]http://www.metoffice.gov.uk>
>
> >Also: Hon. Professor of School of Environmental Sciences, University of
> >East Anglia
> >
> >

> >
> >-----Original Message-----
> >From: Thomas R Karl [[5]mailto:Thomas.R.Karl@noaa.gov]
> >Sent: 01 December 2004 18:23
> >To: Roger Pielke
> >Cc: Phil Jones; Folland Chris; carl mears;
> >CCSPTempTrendAuthors.NCDC@noaa.gov
> >Subject: Re: New version of Chapter 4
> >
> >
> >Phil,
> >
> >I think we need to be careful -- the method of combining the data can
> >matter very much. It is just that despite our different methodologies
> >the results are similar on large scales. I know we could use other
> >methods and the differences are more significant, e.g, first
> >differences, homogenization of ships, etc.
> >
> >Tom
> >
> >Roger Pielke wrote:
> >
> >
> >
> >>Hi Phil
> >>
> >>Thanks for the quick feedback. This helps a lot!
> >>
> >>With Best Regards
> >>
> >>Roger
> >>
> >>
> >>
> >>
> >>
> >
> >
> >
> >
> >
> >

Prof. Phil Jones
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References

1. http://blue.atmos.colostate.edu/
2. http://climate.atmos.colostate.edu/
3. http://www.hadobs.org/
4. http://www.metoffice.gov.uk/
5. mailto:Thomas.R.Karl@noaa.gov

445. 1102524151.txt

##########

From: Phil Jones <p.jones@uea.ac.uk>
To: dkaroly@ou.edu, Kevin Trenberth <trenbert@cgd.ucar.edu>
Subject: Re: Communication with AR4 WGI Chapter 3
Date: Wed Dec 8 11:42:31 2004
Cc: Susan Solomon <solomon@al.noaa.gov>, Martin Manning <Martin.Manning@noaa.gov>, Jean Palutikof <jean.palutikof@metoffice.gov.uk>, Cynthia Rosenzweig <crosenzweig@giss.nasa.gov>

Resending. Apologies! I changed Jean's email incorrectly. This one is now correct.
Phil
David,
I will send you this once we post the ZOD on the WG1 web site in mid-Jan05. Our diagrams
are in a state of flux. Most of the temperature and precipitation trend maps are being
done
in Asheville and I should be getting them later this week or early next. We will be
showing maps
for the whole 20th century, but others will focus on the period since 1979. You might like
to
consider avoiding duplication by using these - eventually they will be 1979-2005 (poss
2006).
Trends of indices in extremes will likely be similar, but with +/- signs on maps. Nothing
has
been decided yet, though, and I expect a significant part of our time at LA2 will be taken
up
by discussing/improving diagrams in our ZOD.
You can help us by sending comments to WG1 on the relevant parts - which are likely
to be almost all.
Cheers
Phil
Cheers
Phil
At 16:47 07/12/2004, David Karoly wrote:

Hi,
As you may be aware, I am an LA for chapter 1 "Assessment of observed changes and
responses in natural and managed systems" in the AR4 WGII and I have been identified as
one of the points-of-contact for interactions between WGI and WGII. The chapter in which
I am involved will depend heavily on inputs from a number of chapters in the WGI report.
Hence, I contacting the CLAs of the relevant chapters, including chapters 2, 3, 4, 5, 6,
7, and 9, by email to discuss ways to ensure effective communication between our
chapters and to avoid undue overlap between respective chapters in WGI and our chapter
in WGII.
Your chapter on "Observations: Surface and atmospheric climate change" is a key chapter
in WGI and it is important that what we say in our chapter in WGII follows from

and
agrees with your chapter. I would be very happy to discuss ways to ensure effective
communication between our two chapters.
Specific aspects from your chapter of relevance to our chapter include observed changes
in regional temperature and precipitation, both means and extremes. We plan to use a
figure in our chapter showing a global map of observed temperature trends over the last
30 years (?) overlaid with locations of significant observed changes in natural and
managed systems. We want to make sure that this is based on the same dataset(s) that you
will be using to show the observed temperature trends.
In practice, almost everything in your chapter will be relevant to our chapter. I would
be grateful if you could send me a copy of your ZOD after it is completed, so that I can
make sure that our chapter is consistent with yours. I am happy to send you a copy of
our ZOD, if you would like to read it.
I will not be coming to the WGI LA meetings until LA3, when I will be involved as a
review editor. It will be important that we have already established effective
communication before then.
I look forward to working with you over the next two years to ensure that the IPCC AR4
is the best possible assessment.
Best wishes, David
--
~~~~~~
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Williams Chair and Professor of Meteorology
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100 E. Boyd St., fax: +1-405-325-7689
Norman, OK 73019 email: dkaroly@ou.edu
USA [1]http://weather.ou.edu/~dkaroly/Personal.htm
~~~~~~

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References

1. http://weather.ou.edu/~dkaroly/Personal.htm

446. 1102687002.txt
##########

From: Gavin Schmidt <gschmidt@giss.nasa.gov>
To: mprather@uci.edu, robert.berner@yale.edu, p.jones@uea.ac.uk, rjs@gfdl.noaa.gov, jhansen@giss.nasa.gov, dshindell@giss.nasa.gov, rmiller@giss.nasa.gov, drind@giss.nasa.govjames.risbey, td@gfdl.gov, aclement@rsmas.miami.edu,

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atimmermann@ifm.uni-kiel.de, ajb@gfdl.gov, penner@umich.edu, solomon@al.noaa.gov,
jmahlman@ucar.edu, rbierbau@umich.edu
Subject: RealClimate.org
Date: 10 Dec 2004 08:56:42 -0500
Cc: Mike Mann <mann@virginia.edu>, Eric Steig <steig@ess.washington.edu>,
ammann@ncar.edu, rbradley@geo.umass.edu, aclement@rsmas.miami.edu,
rasmus.benestad@met.no, rahmstorf@pik-potsdam.de

Colleagues,

No doubt some of you share our frustration with the current state of media reporting on the climate change issue. Far too often we see agenda-driven "commentary" on the Internet and in the opinion columns of newspapers crowding out careful analysis. Many of us work hard on educating the public and journalists through lectures, interviews and letters to the editor, but this is often a thankless task.

In order to be a little bit more pro-active, a group of us (see below) have recently got together to build a new 'climate blog' website: RealClimate.org which will be launched over the next few days at:

http://www.realclimate.org

The idea is that we working climate scientists should have a place where we can mount a rapid response to supposedly 'bombshell' papers that are doing the rounds and give more context to climate related stories or events.

Some examples that we have already posted relate to combatting dis-information regarding certain proxy reconstructions and supposed 'refutations' of the science used in Arctic Climate Impact Assessment. We have also posted more educational pieces relating to the interpretation of the ice core GHG records or the reason why the stratosphere is cooling. We are keeping the content strictly scientific, though at an accessible level.

The blog format allows us to update postings frequently and clearly as new studies come along as well as maintaining a library of useful information (tutorials, FAQs, a glossary etc.) and past discussions. The site will be moderated to maintain a high signal-to-noise ratio.

We hope that you will find this a useful resource for your own outreach efforts. For those more inclined to join the fray, we extend an open invitation to participate, for instance, as an occasional guest contributor of commentaries in your specific domain, as a more regular contributor of more general pieces, or simply as a critical reader. Every time you explain a basic point of your science to a journalist

covering a breaking story, think about sharing your explanation with wider community. RealClimate will hopefully make that easier. You can contact us personally or at contrib@realclimate.org for more information.

This is a strictly volunteer/spare time/personal capacity project and obviously nothing we say there reflects any kind of 'official' position. We welcome any comments, criticisms or suggestions you may have, even if it is just to tell us to stop wasting our time! (hopefully not though).

Thanks,

Gavin Schmidt

on behalf of the RealClimate.org team:
- Gavin Schmidt
- Mike Mann
- Eric Steig
- William Connolley
- Stefan Rahmstorf
- Ray Bradley
- Amy Clement
- Rasmus Benestad
- William Connolley
- Caspar Ammann

447. 1102948164.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@cgd.ucar.edu>
Subject: Some weekend thoughts
Date: Mon Dec 13 09:29:24 2004

Kevin,
I've read everything over the weekend, and here are a few comments. Glad I did this yesterday, as not thinking too well at the moment as daughter-in-law in labour for the last 4 hours. No news yet - just waiting !
I haven't made any alterations yet. Here are my thoughts.
3.1 I'll make a few cosmetic changes - mainly to refer to the Appendices a couple of times re significance.

Box 3.3 Reads better, will replace with this one when merge is done.
3.4 3.4.1.5 needs some work. Doesn't seem to read or flow that well.
3.4.2.1 Maybe need to expand on homogeneity tests.
3.4.2.2 4th para seems a little at odds with previous one?
3.4.2.3, 3.4.2.4 OK
3.4.3 Clouds. Needs some more work to develop a clearer message. You're aware of this.
3.4.4 Radiation. Similar comments to the cloud section. I have some specific notes for both. Despite this, probably OK for the ZOD. Maybe all we need to do is to highlight this to the reviewers.
3.5 Section seems overlong. I know you've reduced it a lot ! Contains a number of sentences where English could be improved.

3.5.1. OK
3.5.2 Significance levels for Fig 3.5.1 need some discussion. We'll need to work some on this Figure.
3.5.3 and 3.5.4 OK for the ZOD with a few better sentences.
3.5.5 and 3.5.6 Both sections seem overlong. Again know you've reduced this a lot, but if we need reductions here is a good place.
3.5.7 OK
Box 3.5 OK
3.6 Generally good.
3.6.1 OK
3.6.2 Probably remove the impact para - leave for the moment, though.
3.6.3 OK
3.6.4 I can improve this a little. It isn't all Scandinavian glaciers that are advancing, just those in SW Norway. Those in the north of Sweden are retreating.
3.6.5 OK
3.6.6/ 3.6.7 Basically OK. May need more re ACW and SAM link if we can say anything.
3.7 This is probably too long, so would be another area for some reduction. Agree on your suggestions for deletions as repetitive.
3.7.1.1-3.7.1.3 OK though all a little long.
3.7.1.4 This is the one where there is some repetition. Not much on monsoon. A lot here is already in 3.8 on extremes and the Dai et al (2004) paper is now referred to in 3.3, here and in 3.8. Suggest it should just be in 3.3 and again in 3.9 (it isn't there yet).
Your figures seem in better shape than those in my section. We will likely need to work on the one Dennis is doing. Will need some colour. You're aware of which need more work from your comments. We can leave these in for reviewer and LA thoughts.
Dave has sent me a first go at the figures. Made loads of suggestions. Dave was aware colour choices poor and will be doing more on them today.
Is Chris Landsea the only person you've removed from the CA list so far? It seems so.
I should have time tomorrow onwards to do merging and send out the 3 files to all our LAs. Are you happy with me merging in your refs list? I'll keep the discard ones at end in a separate list. Still hopeful of doing all this by close of play here on Thursday. All day in London on Friday and CRU party today week from 11am onwards. Going for Dec 16 means I will only be able to get some of the Figures in 3.2 and 3.3 properly into the text.
Will send Dave's next Figure versions if they are much better. No point with current one.
Still no news !
Cheers
Phil

At 21:16 10/12/2004, you wrote:

Phil
Attached are the three sections. Please use these for any suggested edits. Of the text, 3.7 is losest and needs careful comparison with 3.3 to check for inconsistencies.
There is model stuff in there that is not quite right or incomplete: I removed some.
There is reduncdant ENSO-related stuff. A lot of the monsoon variability is linked to

ENSO and we could say that succinctly but it would decimate what the CAs and Panmao have
done. I think we will need to do this in Beijing, but I left it for now. Note the refs
has a list of discards at the end.
Suggest we keep this, perhaps in a different file, and if stuff gets deleted with
references, then the refs get moved there.
Some of the figures are not quite in order in 3.6 and their is the extra figure that
Dennis generated, not currently referred to. Key question is whether to follow up on
this and how to make the multiple figs in 3.6 more compatible. I know you have
suggestions on long time series and I urge you to keep in mind the purpose here: to show
the past variability and place recent trends in that context. A lot could be done on
indices and assoc plots, and patterns. I think we have license to do some of this as
long as the figs are in literature. But we may not be able to reproduce the results???
I have hedged a lot on clouds and radiation, and maybe clarification will come? See if
you think it is OK for now.
Note these 3 versions are dated 1210: 10 Dec. They replace entirely the 1204 versions
which you can discard.
Kevin
--
****

Kevin E. Trenberth e-mail: trenbert@ucar.edu
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Norwich Email p.jones@uea.ac.uk
NR4 7TJ
UK

References

1. http://www.cgd.ucar.edu/cas/

448. 1102953345.txt
##########

From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: need to chat - important
Date: Mon, 13 Dec 2004 10:55:45 -0500

Hi Keith,
I have to head out around 11:30 AM (40 minutes from now). You can try reaching me at my
cell phone after that (434-227-6969)...

Thanks,
Mike
At 08:03 AM 12/13/2004, Michael E. Mann wrote:

HI Keith,
I'll be working at home this morning. You can call me at: 434-977-7688
Mike
At 07:25 AM 12/13/2004, Keith Briffa wrote:

Mike
could you confirm a telephone number to call you on in 3 hours say
thanks
Keith
--
Professor Keith Briffa,
Climatic Research Unit
University of East Anglia
Norwich, NR4 7TJ, U.K.
Phone: +44-1603-593909
Fax: +44-1603-507784
[1]http://www.cru.uea.ac.uk/cru/people/briffa/

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University of Virginia
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[2]http://www.evsc.virginia.edu/faculty/people/mann.shtml

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References

1. http://www.cru.uea.ac.uk/cru/people/briffa/
2. http://www.evsc.virginia.edu/faculty/people/mann.shtml
3. http://www.evsc.virginia.edu/faculty/people/mann.shtml

449. 1102956436.txt
##########

From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: email #1: some background info first...
Date: Mon, 13 Dec 2004 11:47:16 -0500

HI Keith,
Thanks again for your phone call, and the (informal) opportunity to help out where I can.
I'm perfectly happy in that role (as an informal contributor and a formal reviewer, for
example), if you and Peck, for example, are both comfortable with that.
First, "RealClimate" should be helpful. It deals w/ the skeptic claims, etc. but

using the
legitimate
peer-reviewed research as a basis for the discussion.
The "hockey stick" overview should be helpful:
[1]http://www.realclimate.org/index.php?p=7
as well as itemized esponses to the various contrarian propaganda/myths:
[2]http://www.realclimate.org/index.php?p=11
and the specific discrediting of the claims of McIntyre and McKitrick, based both on our
response to their rejected Nature comment:
[3]http://www.realclimate.org/index.php?p=8
and the discussion of the analysis in the Rutherford et al (2004) paper in press in Journal
of Climate, that independently discredits them:
[4]http://www.realclimate.org/index.php?p=10
In the following emails, I'll attach some other materials (submitted papers) that deal w/
the McIntyre and Mckitrick matter, and the von Storch matter,
Please let me know if there is anything we discussed that I forget to provide you. Will
also draft an email to the small group (you, me, Scott, Caspar, Gene) about the prospective
additional RegEM/Mann et al method model analyses,
cheers,
Mike

Professor Michael E. Mann
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[5]http://www.evsc.virginia.edu/faculty/people/mann.shtml

References

1. http://www.realclimate.org/index.php?p=7
2. http://www.realclimate.org/index.php?p=11
3. http://www.realclimate.org/index.php?p=8
4. http://www.realclimate.org/index.php?p=10
5. http://www.evsc.virginia.edu/faculty/people/mann.shtml

450. 1102956446.txt
##########

From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: email #2: paper in review in J. Climate (as a letter), discrediting McIntyre and McKitrick
Date: Mon, 13 Dec 2004 11:47:26 -0500

Keith,
This paper is in review, and can be referred to (just clear w/ Caspar or Gene first) for
IPCC draft purposes. They basically show that the McIntyre and McKitrick paper is total
crap, and they provide an online version of the Mann et al method (and the proxy data), so
individuals can confirm for themselves...
Mike

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

e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[1]http://www.evsc.virginia.edu/faculty/people/mann.shtml
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References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml

451. 1102956796.txt
##########

From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: email #3: Stendel et al paper (submitted)
Date: Mon, 13 Dec 2004 11:53:16 -0500

Keith,
Attached is the Stendel et al paper (submitted to "Climate Dynamics" last month) and a
corrected version of their Figure 3 (using the correct Mann and Jones NH series).
The importance of this paper is that they use the same model as von Storch (higher
resolution in fact), and get a temperature history that looks much like the
reconstructions/other models. Also, they appear to get the negative NAO pattern in the
Maunder Minimum, which von Storch et al do not...
Again, this should be referenceable in the zero order draft, but would be good to contact
Martin Stendel first about this...
Mike

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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[1]http://www.evsc.virginia.edu/faculty/people/mann.shtml
Attachment Converted: "c:\eudora\attach\stendel_et_al_ClimDyn.pdf" Attachment Converted:
"c:\eudora\attach\nh-extend.pdf"

References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml

452. 1102957001.txt
##########

From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>

Subject: email #4: comment (in press in Science) on von Storch et al paper
Date: Mon, 13 Dec 2004 11:56:41 -0500

Keith,
I think the attached comment (in press in "Science") is pretty self-explanatory. It raises
the main objections to the von Storch et al paper (some of which you and Tim already had
raised, really)...
Mike

Professor Michael E. Mann
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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[1]http://www.evsc.virginia.edu/faculty/people/mann.shtml
Attachment Converted: "c:\eudora\attach\VonStorchReply04-submitrevised.pdf"

References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml

453. 1102957016.txt
##########

From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: email #5: paper in review in J. Climate letters using NCAR forced simulation and RegEM
Date: Mon, 13 Dec 2004 11:56:56 -0500

HI Keith,
here (w/ the supplementary info also attached) is the paper summarizing the results I
showed in Victoria of the RegEM analysis of pseudoproxies in the forced CSM simulation.
This is in review as a "letter" in Journal of Climate, and can be referred to as
"submitted" in the zero-order draft.
As we discussed, parallel experiments are being done using the MBH98 method, but regardless
of those results, this suggests, at least, that the RegEM-based NH reconstructions (e.g. in
the Rutherford et al paper you're co-author on) are unlikely to be impacted by the bias
discussed by von Storch et al...
Mike

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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[1]http://www.evsc.virginia.edu/faculty/people/mann.shtml
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"c:\eudora\attach\supplementary1.pdf"

References

1. http://www.evsc.virginia.edu/faculty/people/mann.shtml

454. 1103236623.txt
##

From: Jonathan Overpeck <jto@u.arizona.edu>
To: "Ricardo Villalba" <ricardo@lab.cricyt.edu.ar>
Subject: Re: [Wg1-ar4-ch06] Fw: Section on Modes of Variability
Date: Thu, 16 Dec 2004 17:37:03 -0700
Cc: k.briffa@uea.ac.uk, peltier@atmosp.physics.utoronto.ca, Eystein Jansen <eystein.jansen@geo.uib.no>

Hi Ricardo - good to hear from you. Thanks too for the interesting figure. I have some comments on this section (6.5.4) and also for the others' you're helping to lead.

Regarding 6.5.4 - I hope Dick and Keith will have jump in to help you lead, and I can too. I think the hardest, yet most important part, is to boil the section down to 0.5 pages. In looking over your good outline, sent back on Oct. 17 (my delay is due to fatherdom just after this time), you cover ALOT. The trick may be to decide on the main message and use that to guid what's included and what is left out. For the IPCC, we need to know what is relevant and useful for assessing recent and future climate change. Moreover, we have to have solid data - not inconclusive information. My take:

ENSO - coral records sensitive to ENSO (e.g., Urban et al. and Cobb et al - attached) suggest ENSO has changed in response to past forcing change (Cobb et al - updated interp by mann et al - see recent email attachment) and recent climate change (Urban et al). Ditto for Indian Ocean - not sure if can connect to dipole - I could ask Julie Cole? NAO - lots of papers and what's the consensus? I'm not sure, but I think it is that we can't say for sure what has happend to the NAO - or AO for sure (Keith might no more - recent Ed Cook paper might be the key? - I'm not an expert here). Same thing for PDO (not an expert, but aren't their recons that don't agree - see cole et al for one- attached). In both these cases, the recons don't always agree. Or do they say the NAO variability has stayed pretty constant?

Tropical Atlantic - Black et al 1999 (attached to prev email) also says 12year mode (no consensus if diapole is the correct name for what Chang first described - see ref in Black attached) has been constant for 800 years.

Annual modes - does paleo have anything definitive to say yet? I'm a coauthor on a soon to

be submitted AO recon paper, but I'm not sure reviewers will go for it - nor does it match
D'Arrigo's recent AO recon paper (can't find).

So, the trick is for you to lead us (Dick, Keith, me - maybe Julie - ENSO expert) to
produce 0.5 pages of HIGHLY focused and relevant stuff. Can you take another crack at your
outline and then tell us what you need? Thanks!

Regarding 6.5.9 - can you help Dan, Ramesh and others to make quick headway on this one -
it's totally missing. Thanks!

Regarding 6.3.2.1 - Keith will need help, no doubt - particularly with a good S. Hemisphere
perspective (he can override me on this, but since I'm contacting you...) thanks! What do
we have for the southern hem? Southern S. America, New Zealand, Tasmania, ice core?

Regarding 6.3.2.2 - what's your opinion of where this section stands?

Thanks - hope you are enjoying summer - although Tucson never gets that cold!

Best, Peck

----- Original Message -----

From: [1]Ricardo Villalba

To:

Sent: Thursday, December 16, 2004 2:55 PM

Subject: Fw: Section on Modes of Variability

Dear IPCC colleagues

Please, find attached a preliminary draft of the proposed figure for the section: Modes
of variability. The caption follows. Best regards,

Modes of variability

Figure caption. Coherent modes of climate variability across the Pacific Ocean during
the past four centuries. The upper part of this figure compare temperature-sensitive
tree-ring records (red triangles) from high-latitude, Western North and South America
with a geochemical coral record (yellow triangle) from Raratonga, tropical South
Pacific. The series shown from top to bottom are: Spring/Summer Gulf of Alaska
temperature reconstruction (1600-1994; Wiles et al., 1998), Sr/Ca coral record from
Rarotonga (1726-1996; Linsley et al. 2004) and annual Northern Patagonia temperature
reconstruction (1641-1989; Villalba et al., 2003). Correlation coefficients between
records are indicated. To facilitate the comparison, the Sr/Ca coral record is shown

reversed.

Interdecadal to centennial variability in each time series was isolated by using singular spectrum analysis (SSA; lower part of the figure). For each record, all SSA reconstructed components with mean frequencies longer than 20 years where summed. Correlation coefficients between these long-term modes of variability are also shown. Thin and thick arrows indicate coincidences in oscillations between the Raratonga and one or two high-latitude records, respectively.

Linsley, B., G. Wellington, D. Schrag, L. Ren, M. Salinger and A. Tudhope, 2004: Geochemical evidence from corals for changes in the amplitude and spatial pattern of South Pacific interdecadal climate variability over the last 300 years. Climate Dynamics, 22, 1-11.

Villalba, R., Lara, A., Boninsegna, J.A., Masiokas, M., Delgado, S., Aravena, J.C., Roig, F.A., Schmelter, A., Wolodarsky, A., Ripalta, A. 2003. Large-scale temperature changes across the southern Andes: 20th-century variations in the context of the past 400 years. Climatic Change, 59: 177-232.

Wiles, G. C., D'Arrigo, R.D. and Jacoby, G.C., 1998. Gulf of Alaska atmosphere-ocean variability over recent centuries inferred from coastal tree-ring records. Climatic Change, 38, 289-306.

Ricardo

Ricardo Villalba
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PAGES SSC: [3]http://www.pages.unibe.ch/

Attachment converted: Macintosh HD:modes of variation.jpg (JPEG/prvw) (000C0BD1)

--

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http://www.geo.arizona.edu/
http://www.ispe.arizona.edu/

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References

1. mailto:ricardo@lab.cricyt.edu.ar
2. mailto:ricardo@lab.cricyt.edu.ar
3. http://www.pages.unibe.ch/

455. 1103583356.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@cgd.ucar.edu>, Kevin Trenberth <trenbert@cgd.ucar.edu>, Peter Ambenje <omash01@yahoo.com>, Roxana Bojariu <bojariu@b.astral.ro>, David Easterling <david.Easterling@noaa.gov>, David Parker <david.parker@metoffice.gov.uk>, Fatemeh Rahimzadeh <rahim_f@irimet.net>, Jim Renwick <j.renwick@niwa.co.nz>, Matilde Rusticucci <mati@at.fcen.uba.ar>, Brian Soden <bsoden@rsmas.miami.edu>, Panmao Zhai <pmzhai@cma.gov.cn>, Albert Klein Tank <Albert.Klein.Tank@knmi.nl>
Subject: Re: [Fwd: Re: [Fwd: Re: "Model Mean Climate" for AR4]]
Date: Mon Dec 20 17:55:56 2004
Cc: richard.wood@metoffice.gov.uk

Kevin,
I will be around tomorrow (so Dec 21) until Dec 23 inclusive. Then again from Jan 3.
I will be checking email during the break from Dec 28 onwards.
Are you in control of the glossary additions and modifications?
As to change of base period - this seems like a decision for the whole of WGI. To redo
the global temperature average, I can just move the series up/down, but this isn't
the correct way to do it. I should talk out a new base period from all the individual
stations and recalculate anomalies for the oceans. For the oceans this isn't a
problem, but the land it is a serious problem. Many stations have good (i.e. near
complete base periods for 1961-90) but I'll lose hundreds, maybe over a thousand,
stations if I went to 1981-2000.
For both surface temperature and precipitation we don't have spatially complete datasets

(like models) so it will be quite difficult.
For the circulation indices (like SOI and NAO) based on station pairs there is a variance term (SD). Some of the character of the series will change. We could easily adjust all these series by simple offsetting but it isn't doing it properly.
I'm in the throws of a project with the HC checking all the 61-90 normals we have for series that are incomplete, to ensure we don't have any biases. This has taken quite a time and I don't want to waste the effort.
The arguments of Albert and Dave make a lot of sense - continuity with the TAR etc.
These sort of things can be explained, but then the FOD will not be compatible with all the papers we are referring to. This will lead to lots of confusion. I would like to stick with 1961-90. I don't want to change this until 1981-2010 is complete, for 3 reasons : 1) We need 30 years and 81-10 will get all the MSU in nicely, and 2) I will be near retirement !! 3) is one of perception. As climatologists we are often changing base periods and have done for years. I remember getting a number of comments when I changed from 1951-80 to 1961-90. If we go to a more recent one the anomalies will seem less warm - I know this makes no sense scientifically, but it gives the skeptics something to go on about ! If we do the simple way, they will say we aren't doing it properly.
Best idea might be to show some maps of 1981-2000 minus 1961-90 to show spatially where it makes a difference for temp and precip. Showing it is quite small and likely within the intermodel differences for years which are only nominally 1981-2000. This might keep both sides happy.
We also probably need to consider WGII. Also the paleo chapter will find 1981-2000 impossible. 1961-90 is difficult for them but not insurmountable.
Cheers
Phil
PS Fatima has received all the emails - her email only came to me. Not heard from some of our LAs.

At 15:44 20/12/2004, Kevin Trenberth wrote:

Hi all
I have received comments on this from Albert, David, Dave, and Jim. Some below.
As I commented to Jim, the choice of a base period affects the zero line. In some of our plots, namely the ones that have series of bars from the zero line to the anomaly value, thereby infilling between the anomaly and the zero, the zero base value is greatly emphasized. This is in contrast to a simple time series with points joined, especially if the zero line is not also drawn. In the latter case, it is simple to move the axis up or down to fit with the new base period. But it makes a bigger difference

to the bar plots. Now maybe that is a comment on the use and utility of bar plots,
because the relative values do not change.
The choice also affects any anomaly plots for any subperiod. But this is where the
comparison with models is most likely to occur. In this case there is a spatial pattern
to the offset, namely the difference between means for 1961-90 and 1981-2000. We could
also derive that difference for certain fields and provide it to modelers to enable
comparisons with our plots. For trends over certain subperiod, this makes no
difference.
It seems that whatever we do, we will need an extra appendix explaining some of this and
perhaps even giving plots of these differences.
In the meantime, let me suggest to those of you making computations, that you consider
doing it both ways, rather than having to go back and do it over later.
Regards
Kevin
I agree with Albert, this would make comparisons with the TAR figures difficult.
Dave
Klein Tank, Albert wrote:
Hi Kevin,

My immediate response is that the choice for another base period will probably not
affect our assessment of results, but it will change all figures w.r.t the TAR. This
will be difficult to communicate and will take much more space to explain.

Albert.
-------- Original Message --------
Subject: Re: [Fwd: Re: "Model Mean Climate" for AR4]
Date: Mon, 20 Dec 2004 13:06:44 +0000
From: Parker, David (Met Office) [1]<david.parker@metoffice.gov.uk>
To: Kevin Trenberth [2]<trenbert@cgd.ucar.edu>
References: [3]<41C34CDA.3060304@cgd.ucar.edu>
Kevin

It is obviously possible to use 1980-2000 though it would require some
data-processing work. The main objection is that anomalies (of
temperature) would appear to be reduced relative to previous
publications and readers/policymakers could become confused. A minor
objection is that 1980-2000 is a bit short. Satellite data are of course
in its favour. In due course, 1981-2010 will be ideal!

Regards

David

On Fri, 2004-12-17 at 21:17, Kevin Trenberth wrote:
> All
> Please note the discussion below. Note the proposed base period of
> 1980-2000. Can we get your reactions? If it is decided to use this,
> what difficulties would it create? Other comments?
> Kevin
>
> -------- Original Message --------
> Subject:

> Re: "Model Mean Climate" for AR4
> Date:
> Fri, 17 Dec 2004 14:14:58 -0700
> From:
> Kevin Trenberth
> [4]<trenbert@cgd.ucar.edu>
> To:
> Wood, Richard
> [5]<richard.wood@metoffice.gov.uk>
> CC:
>
> References:
> [6]<FCE86FAA6B302A42AF7F9C6255745E3703C5F4@exxmail2.desktop.frd.metoffice.com>
>
> Richard
>
> The current base period being used in Chapter 3 is anomalies
> determined with respect to the 1961-1990 base period. In
> observations there is a strong emphsis on using 30 year periods and
> the more recent one, 1971-2000 is not yet available. We would need to
> discuss whether to try to switch to that. It certainly won't be in
> any ZOD. Otherwise, though, we are placing a lot of emphasis on
> trends from 1979 on. The grounds for this are 1) The 1976-77 shift
> seems to be about when anthropogenic climate change took off: prior to
> then we are under the realm of natural variability (basically a TAR
> result); and 2) 1979 is when a whole bunch of satellite data and
> other analyses (like global reanalyses) become much more reliable and
> global. So 1979 is the closest proxy to 1976/77.
>
> If 1981-2000 is to be used, it will, of course, include some climate
> perceptible climate change that may influence peceptions of
> anomalies. But I agree there is a lot to be said for consistency.
> Moreover, it is manageable for observational data bases. Because of
> the satellite effects on obs it is important to start on or after 1979
> and stop while we still have obs. So for round numbers 1981-2000 makes
> most sense. I think that was the conclusion we came to in Trieste,
> but it is not reflected in any material I have seen yet in our
> chapter.
>
> Phil is not available till after New Year, I believe.
>
> Regards
> Kevin
>
> Wood, Richard wrote:
> > Dear Jerry and other CLAs,
> >
> > Jerry: would you be willing to do this please, once some text is agreed?
> > All: any comments on the proposed text? (esp from observational chapters
> > re meaning periods). An early response would be appreciated as if we
> > send this to PIs it needs to be done as soon as possible.
> >
> >
> > We've just had a meeting of Chapter 8 LAs in San Francisco. One issue
> > that came up was what period of what run to use for the analysis of the
> > 'mean climate' in the AR4 models, for Chapter 8. Clearly we hope there
> > will be a number of diagnostic projects looking at the models over the
> > next few months, and the more uniformly that analysis can be done the
> > better.
> >
> > To cut a long story short, we felt that given the choice it would be
> > most appropriate to define models' 'mean climate' by looking at the
> > 1981-2000 mean from the all forcings 20th Century runs (or the ensemble

> > mean if there is an ensemble). That would be consistent with the base
> > period Chapter 10 is using for the projections. We recognise that there
> > could be all sorts of reasons why that is not appropriate in particular
> > cases, both scientific and practical (e.g. the observational dataset
> > covers another period, or a longer time mean is needed because of
> > particular modes of variability, or there is a problem with model drift
> > or trends). So we wouldn't want to be prescriptive, but all other things
> > being equal we would suggest that as the analysis period. If there are
> > no show-stoppers for this, we were thinking it would be good to send out
> > a brief email to the PIs of the diagnostic projects to request that they
> > bear this in mind in their analysis. Jerry, there were a few other
> > topics that might be raised in such an email and Karl Taylor will
> > contacting you about those.
> >
> > To be definite, I suggest below some straw-man text that could be sent
> > out.
> >
> >   Thanks and best wishes,
> >   Richard
> >
> > "Defining model 'mean climate':
> > In defining the 'mean climate state' of a model for comparison against
> > observations there are number of choices that could be made, e.g. use
> > model 'control runs' (which may have either preindustrial or present day
> > trace gases), or use the '20th Century all forcings' runs (many of which
> > are available as ensembles started from varying initial conditions). For
> > the 20th Century integrations there is also a choice of meaning period.
> > It is recognised that the optimal choice for a given problem may depend
> > on a number of factors including the period over which obervations are
> > available, and the need for a non-drifting or non-trending model
> > solution. We also recognise that some projects have already begun their
> > analysis based on a particular choice. We therefore do not wish to
> > prescribe a solution to this problem and leave it to the judgement of
> > individual projects. However, in cases where there is a choice, we wish
> > to encourage as much uniformity in the analysis as possible, and
> > therefore propose that other things being equal, model mean climate is
> > defined based on the 1981-2000 period of the 'all forcings 20th
> > Centrury' runs (or the ensemble mean where appropriate)."
> >
> >
> > --------------
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References

1. mailto:david.parker@metoffice.gov.uk
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From: Phil Jones <p.jones@uea.ac.uk>
To: Kevin Trenberth <trenbert@cgd.ucar.edu>
Subject: A quick question
Date: Tue Dec 21 11:39:09 2004

Kevin,
No idea how Chris Folland got this. Presumably David Parker forwarded it !
Anyway, it doesn't matter. The questions are:
When will you be sending me your signed-off draft?
Will this be the complete doc file of text?
Will you be modifying any of the figures?
On the latter just want to know if I'm keeping track of figs as well as Refs. I've got
the two you sent last night.
I'll be off from 5pm on Dec 23. I'll begin reading the draft from Dec 29. Will likely
be in at least once on Dec 29-31, but will be checking email from Dec 29.
Cheers
Phil

All

As someone who dealt with these matters in the past, a decision about the climate
normals period was regarded as so important that all of WG1 debated it and agreed the
outcome. So that should be the route again, I believe, if a change is wanted. From a
personal perspective, I tend to agree with Phil that this time we should stick (in
general) to 1961-90 normals, and that IPCC 2013 should perhaps change to 1981-2010.

Having said that, we may produce 1981-2000 normals in the next year for SST if we can
solve adequately remaining problems (for climate change monitoring) with satellite SSTs.
A key goal is monitoring changes in the Southern Ocean. Solutions are likely to include

use of some corrected (to bulk SST data) ATSR data. This depends on work elsewhere in
the Met Office. However, some less well corrected AVHRR data is needed as well to extend
normals adequately back to 1981 in much of the Southern Ocean.This may give a new
perspectives on the southern ocean SST changes; are likely to be significantly different
in the southern half of the southern ocean from the global average. This is suggested by
the lack of reduction of Antarctic sea ice, in contrast to the Arctic, which still
persists. Such work may or may not get into IPCC FAR but if it did, it could be a
special case. But it would need careful handling for conversion to advice to policy
makers.

Chris

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From: "Michael E. Mann" <mann@virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: Fwd: Re: [Wg1-ar4-ch06] IPCC last 2000 years data
Date: Thu, 23 Dec 2004 14:04:44 -0500

Hey Keith,
I hope your visit w/ your family went well...
I went ahead and tried to make some constructive comments on what you sent (figured it
would be nice to get this out of the way before the holidays come round)..
Let me say I think it's shaping up very nicely--looks like it should be a significant
improvement on the '01 report. You've handled the various controversies and points of
dispute delicately and adeptly, while still driving home in the end the key point (that the
evidence appears to point to anomalous late 20th century behavior).
I made a dozen or so minor comments--please make use of them as you see fit.
Lets reconvene on this after the holidays. Thanks again for including me in and giving me
an opportunity to comment.
I hope the rest of your holidays go well,
mike
At 01:31 PM 12/22/2004, you wrote:

Mike
don't know what the status of the whole chapter is - but I thought I would send this
very first and rough

draft to you anyway - I have to wait and see the whole thing and hear from Peck before
doing more.
Just heard my dad is now pretty much bedridden and officially declared blind (diabetes
etc) and have to fit in a visit to him and mum (who I have not seen for ages) and spend
at least a few days with the kids so there is no way I can work more on this till later
- as I said - really appreciate your input , have a great Christmas and for f..ks sake
keep the right priorities to the fore as the years progress
cheers
Keith

Date: Wed, 22 Dec 2004 18:23:02 +0000
To: Jonathan Overpeck <jto@u.arizona.edu>
From: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: [Wg1-ar4-ch06] IPCC last 2000 years data
Cc: Eystein Jansen <eystein.jansen@geo.uib.no>
Bcc: t.m.melvin@uea.ac.uk,Tim Osborn <t.osborn@uea.ac.uk>
Peck and Eystein
I have to break off now for the christmas period
This is unavoidable. I am sending what I have now
even though I am not at all happy with it.
It is obviously only part way there. Getting the data
to produce Figures and work out how to design them
is going to be time very consuming
and I will rely entirely on Tim here to do them
- and the regional input
stuff if wanted will need input from a number of people
that I have not been able to contact (see later)
The borehole discussion (contributed to by Henry Pollack) will need
batting around and Henry (and Mike , who contributed
a section on regional forced changes) will need to be kept
on board. There will be loads to say on the simulated
temperature histories and Tim will help here also
- but much is unpublished or
even unanalysed (hence Simon and Eduardo will need
to contribute eventually). The glacier bit at the end is what
Olga sent and I have not had time to work through it.
You two need to give some direction as to how
much you wish to have explicitly looking at the mass of
NAO?AO reconstructions , ditto ENSO or PDO and all the
simulations of these - but at this stage not sure where in overall
plan all this going. Do we really want a discussion on MWP
and LIA per se ? The regional descriptions , including Southern Hemisphere
could be infinite length and I suppose we should only discuss longest or
pre assimilated information - but will need specific input here from colleagues
if we are to do these regional (including precipitation) sections .
I know Julie and Ed , and presumably Eystein , will be the best people to ask.
I am attaching the current text and placeholder ideas for Figures .
Not feasible to work more on these until know wider priorities re space.
Have had bad experience with ENDNOTE - and Tom Melvin here will forward
the biblio file later.
I wanted to do more , but that is all I can manage til after Xmas
Here is wishing you (and your loved ones) all the best
Keith
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e-mail: mann@virginia.edu Phone: (434) 924-7770 FAX: (434) 982-2137
[2]http://www.evsc.virginia.edu/faculty/people/mann.shtml
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References

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