CRU CORRESPONDENCE
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88. 0917644194.txt
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From: Janice Darch <J.Darch@uea.ac.uk>
To: env.faculty@uea.ac.uk, env.researchstaff@uea.ac.uk
Subject: EN99:04 UKRO - European News (29 January 1999) (fwd)
Date: Fri, 29 Jan 1999 16:09:54 GMT

Dear All, The most pertinent document is item one on copyright. Some ENv policy documents are also included as
item5.
#Janice
Forwarded Message:
From: Helen Self <H.Self@uea.ac.uk>
Date: Fri, 29 Jan 1999 14:32:36 GMT
Subject: EN99:04 UKRO - European News (29 January 1999) (fwd)
To: d.chadd@uea.ac.uk, dean.wam@uea.ac.uk, Dora.K@uea, e.banakas@uea,
     e.doy@uea, f.littlewood@uea, g.turner@uea.ac.uk, h.brownlee@uea,
     j.casey@uea.ac.uk, j.darch@uea, j.johnson@uea.ac.uk, j.schostak@uea,
     j.steward@uea, j.watson@uea.ac.uk, m.silbert@uea, m.stallworthy@uea,
     mrs@sys.uea.ac.uk, odg.gen@uea, r.mcbride@uea, r.mclarty@uea.ac.uk,
     r.sales@uea.ac.uk, r.sassatelli@uea.ac.uk, t.prime@uea.ac.uk,
     v.koutrakou@uea

Forwarded Message:
From: ukro.ukro <ukro.ukro@BBSRC.ac.uk>
Date: Fri, 29 Jan 1999 12:45:25 +0000
Subject: EN99:04 UKRO - European News (29 January 1999)
To: g.l.a.jones@reading.ac.uk, geoff.g.wood@vla.maff.gov.uk,
     costas.kaldis@britcoun.gr, david.elliott@britcoun.org.il,
     shabtay.dover@skynet.be, elosuniv@BBSRC.ac.uk, eoscmemb@BBSRC.ac.uk,
     elosresc@BBSRC.ac.uk

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EN99:04 UKRO - European News (29 January 1999)
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News on non-Framework Programme 5, programmes & policy
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GENERAL:
1. ESF on Copyright Law
2. GENERAL - Policy documents

LIFE SCIENCES:
3. DG V - Newsletter on Alzheimer's Disease
4. Microbiology - Industrial Platform

ENVIRONMENT:
5. ENVIRONMENT - Policy documents

ENERGY:
6. Synergy - International Cooperation in Energy

INFORMATION TECHNOLOGIES:
7. Public-Sector Information

INDUSTRY & TECHNOLOGIES:

8. Results - Pilot Projects on Benchmarking
9. Communication on Industrial Policy

EDUCATION:
10. Leonardo Database on Cordis

REGIONAL FUNDS:
11. Mid-term Review for Structural Funds

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1. ESF on Copyright Law

The European Science Foundation is warning that current plans for new EU copyright laws, if left unchanged, could harm the international competitiveness of European research. The Commission's draft Directive harmonising aspects of copyright will shortly be debated by the Council of Ministers. The ESF is calling for changes to be made to the wording of one of the Directive's key articles which deals with 'exceptions' to the proposed laws to ensure that it doesn't cause legal and financial headaches for Europe's researchers.

The Foundation supports the Commission's objectives of improving the protection of intellectual property as technological developments make it ever easier for pirates to duplicate and distribute copyright material. But it warns that this should not be at the expense of Europe's ability to carry out research. Reflecting widespread concern in its Member Organisations, the Foundation argues that the draft Article 5, which deals with 'exceptions' to the proposed laws, "could result in research being treated differently in different countries across Europe". As presently written, the Article sets out an exhaustive list of permissible exceptions to the directive, but it leaves to Member States the interpretation and implementation of these 'exceptions'. The effect of this could be that some researchers might find themselves in a worse position than at present regarding their access to and use of published material. Given the differences in national legislation between Member States, the ESF recognises it may be difficult to draft and agree prescriptive legislation for 'exceptions'.

The Foundation is recommending, therefore, that a clause be added to the Directive allowing for the inclusion of all current 'exceptions' set out in national legislation. Other suggested revisions include the need to ensure that 'scientific research' is interpreted in a broad sense, with research in the humanities and arts being explicitly included. In addition, the ESF suggests that the current reference to 'non-commercial' research could cause confusion, as it would be very difficult to differentiate between commercial and non-commercial research in most academic settings. To avoid this, it recommends the introduction of a 'public good' definition of research, which could form an 'exception' to the Directive. The Foundation's statement also points out that the Directive's current reference to the possibility of Member States exempting the use of work "provided that such use exclusively serves the purpose of illustration for teaching or scientific research" is ambiguous. It could be interpreted that there is such a thing as 'illustration for research' and that any 'exception' did not apply to research in general. A simple rewording of the sentence to read "sole purpose of scientific research or for illustration for teaching" would clarify the 'exception'.

The European Commission's draft "Directive on the Harmonisation of Certain Aspects of Copyright and Related Rights in the Information Society" is available on-line at http://europa.eu.int/comm/dg15/en/index.htm

FURTHER INFORMATION: Johanne Martinez, Information Officer, European

Science Foundation, tel 0033 3 8876 7114, fax 0033 3 8837 0532, email: jmartinez@esf.org, URL: http://www.esf.org

2. GENERAL - Policy documents

Recent policy documents issued by the European institutions. Full titles and details appear on the UKRO web site under the subject listings:
* Community action programme in the field of Civil Protection
* Action programme for customs in the Community
* Further actions in the fight against trafficking in women
* Further actions in the fight against trafficking in women
* Better lawmaking 1998: a shared responsibility Commission report to the European Council
* Determination of the person liable for payment of value added tax
* Legal aspects of electronic commerce in the internal market
* General framework for Community activities in favour of consumers
* Action programme for customs in the Community

3. DG V - Newsletter on Alzheimer's Disease

The first edition of the Alzheimer Europe quarterly newsletter has been published by DG V (Public Health). The newsletter is intended to draw attention to the aims and activities of Alzheimer Europe, a grouping of national organisations dealing with Alzheimer's disease. The newsletter includes news of research, events and conferences relevant to the field. It will focus on important developments in the European institutions which affect people with dementia and is also intended to be a platform for the exchange of ideas between organisations and institutes active in the field of Alzheimer's disease. Each issue will include reports on EC-funded transnational projects, beginning in the first edition with London's Institute of Psychiatry EUROCARE project. The next edition of the newsletter will be published towards the end of March 1999.

FURTHER INFORMATION: Alzheimer Europe, tel 00352 297 970, fax 00352 297 972,email: info@alzheimer-europe.org, URL: http://www.alzheimer-europe.org

4. Microbiology - Industrial Platform

The Industrial Platform for Microbiology, a ginger group of EU-funded companies and researchers, has decided to change the focus of its activities. It will now aim to provide a forum for EU industrial microbiologists to discuss research and development strategies, scientific aspects of regulatory developments in applied life sciences, and professional issues such as education and training in the field. The Industrial Platform for Microbiology was originally established to organise information exchange between EU-funded companies interested in using the results of EU funded projects and academics working on microbiology research and development projects. Its members will meet again in Brussels in February 1999 to discuss a draft "code of conduct" for companies involved in bioprospecting activities.

FURTHER INFORMATION: Anne-Marie Prieels, Tech-Know Consultants, tel 0032 58 513 953, email: anne.marie.prieels@skynet.be, URL: http://www.tech-know.be

5. ENVIRONMENT - Policy documents

Recent policy documents issued by the European institutions. Full titles and details appear on the UKRO web site under the subject listings:
* Present situation and prospects for radioactive waste management

* Minimum criteria for environmental inspections in the Member States
* Cooperation in the field of accidental marine pollution
* Limitation of emissions of volatile organic compounds due to the use of organic solvents
* Review clause Environmental and health standards four years after the accession of Austria, Finland and Sweden to the E. U.
* Application of aerial-survey and remote-sensing techniques to the agricultural statistics for 1999-2003
* Financial instrument for the environment
* Forestry strategy for the E. U.
* Control of transboundary movements of hazardous wastes and their disposal
* Voluntary participation by organisations in a Community eco-management and audit scheme
* Remote sensing applied to agricultural statistics during the period 1994-1998

6. Synergy - International Cooperation in Energy

The Council has announced a Decision (1999/23/EC) adopting a multiannual programme to promote international cooperation in the energy sector (1998-2002). According to the Decision, within the European Union's Energy Framework Programme (see EN39:98, item 11), a specific programme for reinforcement of international cooperation in the energy field will be implemented from 1998 to 2002 ('Synergy programme').

The objectives of this programme are to provide assistance to third countries with the definition, formulation and implementation of energy policy, and to promote industrial cooperation between the Community and third countries in the energy sector. The main tasks of the Synergy programme are to help achieve the Community's energy objectives: competitiveness, security of supply, and protection of the environment.

The financial reference for the Synergy programme will be ECU 15 million. Of this, ECU 6m will be for the period 1998 to 1999. The finances for the period between 2000 and 2002 will be reviewed if the amount ECU 9m is not consistent with the financial perspective for that period.

Supported activities are:
* Energy policy advice and training;
* Energy analyses and forecasting;
* Energy dialogue and exchanges of information on energy policy, notably by means of organisation of conferences and seminars;
* Support to regional transboundary coooperation;
* Improvement of the Framework for industrial cooperation on energy.

According to this Decision, NO FUNDING MAY BE GRANTED TO RESEARCH, development, or demonstration projects.

FURTHER INFORMATION: OJ L 7 of 13 January 1999, p.23.

7. Public-Sector Information

The European Commission has decided to publish a Green Paper on how the information gathered by government departments and other public bodies can be used to provide the greatest benefit for citizens and businesses in Europe. A lot of information gathered by public bodies for carrying out their duties could be used by the multimedia industry for developing new products and services. Citizens could make better use of their rights if, for example, information was readily available on the conditions for

working, studying or living as a pensioner in other Member States. Many people would like to have full information on the tax regulations for cross-border purchases. The competitiveness of businesses could be increased if they had a quick and easy means of finding out what the regulations and procedures are for exporting to other countries. All this information exists, but the technical and legal procedures and terms under which the Member States make it available are uncoordinated and therefore not very transparent for citizens and business. The Green Paper calls for these matters to be discussed and asks questions about how the situation can be improved.

FURTHER INFORMATION: http://ww.echo.lu/legal/en/access/access.html

8. Results - Pilot Projects on Benchmarking

Results from four pilot projects on benchmarking framework conditions - in the fields of professional qualification, logistics, the impact of new information technologies (NITs) on company organisation, and the financing of innovation - are reported in the January issue of the newsletter of the European Association of Development Agencies (EURADA).

The lessons drawn included:
* Companies located in peripheral regions suffer from the poor quality of infrastructures, expensive logistical services and weaknesses in the field of transnational cooperation;
* The authorities should support the effective use of NIT and the enhancement of NIT-related structures;
* SMEs lack NIT qualifications and skills;
* Business Angels play a lesser role in innovation in Europe than in the US, probably due to tax- and revenue-related problems;
* Even though it remains below the number of such companies operating in the US, the number of venture capital companies operating in Member States of the EU is rising (750 versus 1800);
* Generally speaking and in comparison with the US and Israel, Europe suffers from a deficit in terms of the ability to evaluate technological risks and from a lack of initiatives to support faster interaction between universities and companies;
* Education policies should be more practical and in-company training should be fully integrated in the programmes of higher education institutions;
* Closer links should be promoted between industry and the educational system;
* The skills which new workers lack most upon entry to the labour market are (a) knowledge of English, (b) computer literacy, (c) knowledge about the industrial world, and (d) adaptability.

FURTHER INFORMATION: EURADA, Avenue des Arts 12/7, B-1210 Brussels, tel 0032 2 218 4313; fax 0032 2 218 4583, email: info@eurada.org, URL: http://www.benchmarking-in-europe.com

9. Communication on Industrial Policy

The Commission has adopted a communication launching an open debate with the EU's different political, economic and social players on the orientation of a new industrial policy with a view to addressing the challenges of globalisation and accelerated technological changes. The communication diagnoses European industry's weaknesses and proposes a series of measures to promote industrial competitiveness.

The communication diagnoses the weaknesses of European industry:
* Europe does not have a strong presence in the services sector;

* European enterprises resort to insufficient externalisation;
* Specialisation remains underdeveloped in sectors with high growth, highly differentiated products and requiring a strong marketing strategy;
* The European audiovisual sector is in an unfavourable competitive position;
* European enterprises form relatively few alliances in advanced technology areas;
* The amounts invested by risk capital funds are insufficiently oriented towards new and high- technology industries;
* European enterprises can access financial markets only with difficulty;
* The level of R&D spending in terms of EU GDP is still below that of its principal global economic partners;
* The exploitation of research results is not efficient enough;
* The EU suffers from high costs and the complexity of procedures for achieving intellectual property protection in Europe;
* European enterprises put very few joint research projects in place.

To counteract this situation and stimulate European competitiveness, the communication emphasises the following proposals, among others:
* Reinforce intangible investment, by adapting the systems of accrediting competencies and by improving the level of and return from research resources, especially through a better system of intellectual property protection;
* Develop human resources by acting on the educational system, by encouraging the spirit of enterprise and various forms of social innovation and social cohesion;
* Promote the access of European enterprises to the world market, by accelerating the exploitation of the competitive advantages of the Single Market;
* Promote fair rules of the game at a world level in view of the new round of WTO negotiations (that is by developing an observation system for public support to research in industrialised countries);
* Develop the dialogue between industry and public authorities and forms of self-regulation (protection of consumers and users);
* Improve financing by eliminating institutional and regulatory barriers to the development of venture capital and improving the tax regime applied to venture capital;
* Adaptation of the rules to the context of the information society and electronic commerce (agreements such as the "International Charter").

FURTHER INFORMATION: Press release IP/99/33.

10. Leonardo Database on Cordis

DG XXII and Cordis have approved plans to include the products database of the Leonardo da Vinci Programme on the Cordis service. The publication of the Leonardo Da Vinci products database on Cordis should allow its continual update. This is hoped to improve interaction between the owners of products and their users. The schedule for the launch of this database on Cordis has yet to be confirmed.

FURTHER INFORMATION: DG XXII, fax 0032 2 295 5699, URL: http://europa.eu.int/en/comm/dg22/leonardo.html

11. Mid-term Review for Structural Funds

The European Commission approved a report on the mid-term review for the poorest (Objective 1) and sparsely populated regions (Objective 6) in the European Union (EU) for the present programming period (1994 1999). It gives an overview of the mid-term evaluations carried out for the Structural Funds programmes during the current programming period. The

report shows important achievements, e.g. when it comes to reducing disparities in basic infrastructure, energy diversification or environmental improvements.

URL: http://www.inforegio.org/wbdoc/docoffic/official/repor_en.htm

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Commission press releases (reference 'IP/year 2 digits/number') can be obtained from 'RAPID' at http://europa.eu.int/en/comm/spp/rapid.html
Log in as 'guest' with password 'guest'.

European documents (ISBNs) and Official Commission documents (reference 'Com (year 2 digits) number') are available from your local European Documentation Centre at: http://www.cec.org.uk/relays/relhome.htm or from the Stationery Office, Tel 0171 873 8372, fax 0171 873 8463.

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UKRO
Rue de la Loi 83
1040 Brussels, Belgium
Tel: 0032 2 230 5275 / 1535
Fax: 0032 2 230 4803
Email: ukro@bbsrc.ac.uk
URL: http://www.ukro.ac.uk

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Dr J P Darch
j.darch@uea.ac.uk
Research Administrator, School of Environmental Sciences,
University of East Anglia, Norwich, NR4 7TJ, UK
Tel: +44 (0)1603 592994   Fax: +44 (0) 1603 507784/507719

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From: Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>
To: Mike Hulme <m.hulme@uea.ac.uk>
Subject: Re[2]: IPCC Chapter 13 - invitation to contribute
Date: Tue, 2 Feb 1999 20:21:47 +0100
Reply-to: Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>

Dear Mike,

thanks for your message. I am sure we can work with these files as soon as we know how the grid is organized. Is it line by line from the North to the South, starting at the dateline? Or something different?

Yes indeed, it would be the best to work with *your* 61-90 baseline for this. Does the baseline also contain cloudiness? If not, then I intend to generate that from our own files, and we will make the assumption that, on the level of monthly means, this does not change as much as to significantly affect the sensitivity of vegetation to the other forcings.

As for a minor point, please remember to use my pik-address whenever possible. The other two (csi and t-online) are both used for sending mail while I am on the road (csi) or at home (t-online), and particularly t-online has the drawback that I can ONLY access it from home (presently) and not from the lab. Unfortunately, I cannot convince my mail sending software to always pretend the mail comes from PIK...

Yes, I will come to the ACACIA meeting, at least until the second day in the afternoon - after that I have to juggle two other meetings in Holland and Germany. With some luck, I should be able to present some results there.

Best wishes!

Wolfgang

PS: I saw your correspondence with Kinne and am interested to follow up - but not today.

On Dienstag, 2. Februar 1999, you wrote:

> Wolfgang,

> Martin is dragging his feet, but you have convinced me we should distribute
> them anyway. I have got someone onto it today and with luck may have the
> minimum (8 realisations for 4 scenarios and for 3 timeslices and for Tmean,
> Precip and DTR on the HadCM2 grid for the entire world) completed and on an
> ftp site by Friday. I will also let Nigel know about this. Presumably you
> will use the 1961-90 0.5deg baseline data? Our files will present
> *changes* from 1961-90 on a mean monthly basis on the 2.5 by 3.75 grid.

> Let's keep in touch on this since it opens up a number of other
> issues/applications. Will you be coming to the ACACIA meeting in early March?

> Mike

> p.s. the files will be in the same format as the attached file to this
> email - just so that you can start thinking about what you need to do.

mailto:Wolfgang.Cramer@pik-potsdam.de

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From: "Jonathan T. Overpeck" <jto@ngdc.noaa.gov>
To: Frank Oldfield <frank.oldfield@pages.unibe.ch>
Subject: Re: Finances and futures
Date: Thu, 4 Feb 1999 11:43:09 -0700
Cc: messerli@giub.unibe.ch, domraynaud@glaciog.ujf-grenoble.fr,
pedersen@eos.ubc.ca, k.briffa@uea.ac.uk

Hi Frank and friends - I'm happy to see the budget looking sound and feel
Franks suggestions are good ones in terms of money to spend this year.
Building on the Swiss paleoclimate course is a good idea, and, of course,
we should decide on future REDIE investments at future SSC's. My gut
feeling is that REDIE will have to continue to be a lower priority in the
future, BUT that we should stay committed to getting scientists (including
youngsters) from developing countries to our science mtgs - makes more
sense than training probably, given tight budgets. Thanks,Peck

>Dear colleagues,
>
>I now share with you some ideas about our financial situation in PAGES. I
>think the information should be treated confidentially at this stage and
>certainly with some discretion.
>
>During the course of last year, it was very difficult to keep track of our
>financial position from month to month, partly because it took our
>financial contacts in the University of Bern an inordinately long time to
>sort out the financial implications of the OSM, partly because, in the
>course of doing this, they made some understandable but very significant
>and confusing errors. Niklaus has now managed to sort these out and we also
>have our confirmed budget for 1999 - which means that we can begin to do
>some real planning.
>
>The first significant point is that we are carrying over into 1999 a
>surplus some US$15k greater than we began with in 1998. In fact we have
>been building up our 'carry-over' steadily since the beginning of 1996 and
>it is now around $67k - between 13% and 14% or our annual budget and a much
>higher proprtion of that part of our budget that is uncommitted each year.
>Whilst I believe it would be unwise to eliminate it entirely, I do think we
>should aim to reduce it significantly provided there is a good rationale
>for the means we choose.
>
>I have attached a summary of how I see things for 1999. You will see that
>even if we spend all the funds committed to workshops at our Pallanza
>meeting, we still have a very healthy surplus. On past experience, I do not
>think this sum will be exceeded during 1999 - even if we have one or two
>more urgent requests, they are more than likely to be offset by delayed
>workshops, so I think this is actually likely to be an over-estimate.
>Moreover, I have assumed that ALL the money allocated by IGBP for Synthesis
>will be spent in 1999. We are under some pressure to do this, but the pace
>of the exercise makes me suspect that we may have difficulty.
>
>At the end of the Table, I list 3 additional commitments I would like to
>propose for prioiritizing and I discuss each briefly below:
>
>1. REDIE (which you may remember stands for Regional Educational and
>Infrastructure Efforts (about which we have, so far, said very little and

>done even less).
>
> In this area, one of the ideas gently simmering on the back burner has
>been the notion of winning support from START to run something like a
>Summer School for selected young scientists from developing countries. This
>emerged from an informal discussion between ouselves in the Office, Bruno
>and Roland Fuchs, the Director of START, when he was over here on a visit.
>At the time, he seemed quite keen on the idea, but has since been silent.
>No matter, I still feel it is an idea worth working towards at least up to
>the pre-commitment stage and I have been exploring informally the
>possibility of basing such a course in London.
>
>This coming summer, I think we may have a chance to do a kind of partial
>trial run. Thomas Stocker and Andy Lotter (a first class paleolimnologist
>here in Bern) plan to run a Summer School nearby this year. Thomas
>approached me some time ago to see if PAGES could support participation by
>any overseas students and my reply was a very cautious one to the effect
>that we would normally expect to be approached and have an input at the
>planning stage and that we would only really consider such a possibility in
>the context of training for scientists from developing countries. Having
>discussed the whole thing more fully with him, I begin to wonder whether
>it may offer quite an interesting possibility. My plan would be to seek
>nomination of/applications from say 3 to 5 young scientists from different
>parts of the developing/former eastern bloc world (represnting each of the
>PEP Transects) and bring them to Bern both for the course and for a short
>period linked into the PAGES Office. The ideas behind the latter part of
>the suggestion would be to
> - support their participation if need be,
> - give them some sense of PAGES and its role in nternational global
>change science/IGBP etc and
> - solicit feedback and advice about what the shape of an ideal course for
>developing country scientists interested in PAGES activities might be.
>
>I believe that even if we did not have something like REDIE in our
>Implementation Plan it should be an important commitment; since we do, it
>is an absolute obligation which we ignore at the risk of serious
>allegations of bad faith.
>
>2. I feel there will be a need to follow up my PEP II visit to Australia
>with something positive there. John Dodson is responding well to
>suggestions about more co-ordination and bringing in more colleagues to
>share the responsibility, but I think that if whatever we agree in Perth is
>actually to work, there will be a need to fund a WORKshop (as distinct from
>a mini-symposium) of thematic and/or regional co-ordinators to get their
>act together. We should offer money for this.
>
>3. The difference it has made having Cathy Stickley (based at UCL) working
>for PEP III is fantastic, but we risk losing her input unless something can
>be done. I'm negotiating with ESF, but it will be over a year before their
>finely grinding mills deliver anything. Rick and Francoise are also going
>to apply to EC for Framework 5 funding, but that will be no quicker. I am
>seriously considering asking Zimmie to help bridge the gap since he did not
>quite close the door when I last talked this through with him, but I feel
>that if I do this, PAGES might need to put up a bit more colateral, the
>more so since we are in credit.
>
>Both 2 and 3 reflect my view that the PEP's remain an absolutely vital part
>of the PAGES structure and need to be supported if that is the only way
>they can achieve their objectives.
>
>All three of the above suggestions require some endorsement in principle
>before I take them any further. If we were to spend all the funds envisaged
>before the end of 1999, our budget credit would be very much reduced -

>probably by too much, but I believe the PEP funding would probably be paced
>over a longer period and that the other items in our budget are more likely
>to be marginally under- than over-spent, so I do not feel we are proposing
>any unreasonable risk.
>
>I look forward to any reactions members of EXCOMM may have to these
>suggestions.
>
>Withh all good wishes,
>
>Frank
>
>
>Attachment converted: Macintosh HD:Budget for 1999 (RTF /MSWD) (0000B314)
>_____
>Frank Oldfield
>
>Executive Director
>PAGES IPO
>Barenplatz 2
>CH-3011 Bern, Switzerland
>
>e-mail: frank.oldfield@pages.unibe.ch
>
>Phone: +41 31 312 3133; Fax: +41 31 312 3168
>http://www.pages.unibe.ch/pages.html

Dr. Jonathan T. Overpeck
Head, NOAA Paleoclimatology Program
National Geophysical Data Center
325 Broadway E/GC
Boulder, CO 80303

tel: 303-497-6172
fax: 303-497-6513
jto@ngdc.noaa.gov

For OVERNIGHT (e.g., Fedex) deliveries,
PLEASE USE:

Dr. Jonathan Overpeck
NOAA National Geophysical Data Center
3100 Marine Street, RL3, Rm A136
Boulder, CO 80303
tel: 303-497-6160

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##########

From: Simon Tett <sfbtett@meto.gov.uk>
To: Peter Stott <pastott@meadow>, Gareth Jones <gsjones@meadow>, Myles Allen
<allen@wobble.ag.rl.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, Keith Briffa
<k.briffa@uea.ac.uk>
Subject: Tuesday Meeting
Date: Wed, 17 Feb 1999 23:01:45 +0000

Dear all,
thank you for the meeting on tuesday. I think it went well. Here as promised and slightly late is a summary of what we discussed. Myles can you forward the message to Michael. Can you let me know if you are all happy with this and once I have made any corrections you want I'd like to send it to John, Geoff and Tim Barnett -- anyone else you think should get it?

Proxy Climate forcing.

Solar -- Beer has a Be based proxy reconstruction of Solar ACTIVITY which can be converted to irradiance changes. [Is it different from LBB or H&S ?] Has the LBB dataset been updated? Has Svensmark got a better handle on his proposed physical mechanisms to amplify solar irradiance changes? [Someone to check at RMS meeting which I won't be able to attend] Want forcing back to 1600?? though HC would find it hard to justify doing runs that early -- me to see if John/Geoff think useful or not.
Me to check with William the source of the rumour about problems with the H&S dataset.

Volcanoes. Volcanoes are an important climate forcings [Issue for IPCC??] Do volcanic erruptions cluster? Myles to "persuade" a student to look at Phil/Keith's dataset and see if there is evidence for this? Are there other indices of volcanic activity? Is climate response to volcanoes sensitive to mean state?? i.e. in cooler climate get bigger response. [Gareth could see from our model if Krakota response significantly different from Pinatubo]

Proxy Climate data + comparision with obs and models.

Keith/Phil have 400 sites of high quality tree ring density data which there are willing to let HC (Mat) use to do a crude model/data comparision. Mat and Tim to liase on what they are doing. Note that funny things are happening in the density data post 1950. Also available may be some borehole data [Phil to talk to Pollock/Wang about possibility] which could use to compare with model -- should consider using lower soil temperature rather than 1. m temp. There are a few sites with data from 0A to 2000 as well as many sites with data for 1700 to 2000 -- should consider both. There may be some other tree ring data which tells us something about SW USA precip and thus ENSO.

Tim wants to compare patterns of temperature var from the proxy data and compare that with the models i.e compare "observed" and modelled covariance structure rather than just the variability. Also Tim wants to try and unpick Mann's stuff.... HC to provide solar forced run from 1700 -- Me to check if it goes from 1700!

Our approach will be to compare model data "directly" with Proxy data rather than do Interpolation a la GISST or Mann et al.

EU proposal

Not clear if in this years framework 5 call there will be room for Detection/Attribution proposals (which is how we'd like to frame a model/proxy comparision). Other issue is that QUARCC 2 and model/proxy comparision could involve similar institutions which could cause problems. Phil to check if room this year for proposal. Keith pointed out that we can't just recycle the NERC thematic proposal (PRESIENT). There is good news on that fron which suggests the proposal will go through with an 8 million pound budget!!!

Ad Hoc detn group.

Not much said on that (or at least I didn't note it) Phil -- you have some advice for me on that?

CLIVAR/PAGES

In the next 1-2 years there may be new reasonable quality ice core and sedimentation data available. Data availablity from the proxy and modelling groups is an issue (another reason for an EU proposal!).

Phil pointed out that there is a lot of instrumental data (in "funny" units) which could be digitised in Europe.

Keith is planning on writting a "call to arms" paleo data paper.

I think I need to come up with a list of actions.... Anyone want to volunteer.....

Simon

92. 0919450520.txt
##########

From: Eugene Vaganov <evag@ifor.krasnoyarsk.su>
To: k.briffa@uea.ac.uk
Subject: No Subject
Date: Fri, 19 Feb 1999 13:55:20 +0300 (MSK)

From: <dndr@ifor.krasnoyarsk.su>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Some information about the super-long tree-ring chronology the East of Taymir and Putoran

Dear Keith
I sent two variants of letter by mail few days ago.
Hope that you received fax copy of it.
There are the references you ask:

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Regards, Gene.

93. 0919980501.txt

##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Fred.Semazzi@soc.soton.ac.uk
Subject: Some things of possible CLIVAR interest
Date: Thu Feb 25 17:08:21 1999
Cc: t.osborn@uea,p.jones@uea

Dear Fred,

The following legends refer to the appropriately titled post-script files that will be sent to you separately by my colleague Tim Osborn.

Please note that these results are products of the European Community funded project ADVANCE-10K (Analysis of Dendrochronological Variability and Associated Natural Climates in Eurasia - the last 10,000 years).Environment and Climate Programme Contract ENV4-CT95-0127. See also http://www.cru.ac.uk/cru/research/

As I said on the 'phone , due acknowledgement of the above is important to us!

Figure 1
Annually averaged tree-ring density data from 400 high-latiude or high-elevation sites around the Northern Hemisphere. This series represents interannual and multidecadal summer temperature variability from A.D.1400 onwards. This series shows circum-hemispheric summer temperature variability on interannual and multi-decadal

timescales and demonstrates the relative cooling effect of known, and some probably as yet unknown, large explosive volcanic eruptions.

Figure 2
Normalized tree-ring -density anomalies around the Northern hemisphere showing patterns of likely summer temperature changes year by year through the relatively cool decade of the 1810s, in part caused by major volcanic eruptions in 1809 and 1815.

Figure 3
Decadally-smoothed timeseries of standardized radial tree growth at three high northern latitude regions during the last 2000 years : Tornetrask, N.Sweden (20E);Yamal(70E)and Taimyr(102E),Russia. Positive and negative values of these data represent relatively warm and cool summers, associated at each location with the strength and position of large-scale atmospheric circulation features.

I have asked Phil Jones here to send you a post script file and reference for the mean 1000-year Nortern Hemisphere curve. His email address is shown above.

You may be also interested in some reconstructions of the NAO made by various people. If so ask Tim about these.

best wishes
Keith

94. 0922988761.txt
##########

From: Mike Hulme <m.hulme@uea.ac.uk>
To: Jose Caicedo <jdpabon@bacata.usc.unal.edu.co>,cubasch@dkrz.de, desanker@mtu.edu,<giorgi@ictp.trieste.it>,tim.carter@vyh.fi, Xiaso Dai <daixs@pcux.ied.ac.cn>,Mohammed El-Raey <elraey@frcu.eun.eg>, djgriggs@meto.gov.uk,nleary@usgcrp.gov,m.hulme@uea.ac.uk, lautenschlager@dkrz.de,Luis Mata <lmata@t-online.de>, jfbmitchell@meto.gov.uk,Nguyen Nghia <nghia@iad-fsiv.ac.vn>, Dr M.Lal <mlal@cas.iitd.ernet.in>,lindam@ucar.edu,t-morita@nies.go.jp, Daniel Murdiyarso <biotrop@indo.net.id>,nobre@yabae.cptec.inpe.br, mnoguer@meto.govt.uk,hm_pitcher@pnl.gov,parryml@aol.com, bscholes@csir.co.za,phw@dar.csiro.au,crosenzweig@giss.nasa.gov
Subject: URGENT - IPCC DDC consultation
Date: Thu, 01 Apr 1999 12:46:01 +0100

Dear TGCIA'ers,

I have two questions to raise with you regarding the IPCC Data Distribution Centre. The first one concerns advice regarding a GCM submission to the DDC and the second concerns mirror web sites for the DDC.

1. GCM submission.

The LMD (through Herve Le Treut) has requested the runs from LMD coupled GCM be lodged with the DDC. His original request (July 1998) is appended below as text ATTACHMENT 1. We originally rejected the submission on the grounds that the runs were not historically forced, i.e., they were cold-start experiments with 1% p.a. forcing being introduced from 'current' baseline and different to all other DDC runs.

However, LMD have re-submitted their request for reasons outlined in ATTACHMENT 2 which is an email from my DDC Co-Manager Michael Lautenschlager (dated 12 February 1999). In this ATTACHMENT Michael makes a proposal to include the LMD model runs, but as 'related modelling

results' rather than as 'full status' DDC results.

We need to take TGCIA soundings on this. Strictly, the LMD runs do *not* qualify according to the criteria the TGCIA established back in May 1997. The question is how flexible are we prepared to be and whether including model runs with a different experimental design may either a) confuse impacts users and/or b) invalidate inter-model comparisons. Bear in mind also that if/when new GCM results forced by SRES forcings are generated this summer and beyond, we will need to consult again about how the DDC handles/presents these new SRES runs. At present the DDC does not have a mandate for these either.

Please would you submit your opinions to me by Monday 12 April. I will then compile the views expressed and make a recommendation.

2. DDC mirror web sites.

With the DDC web site now fully operational (and the CD-ROM about to be released) we need to consider our idea for mirror sites around the world. Users are picking up data and information from both the Yellow Pages (full GCM archive site) and Green Pages (synthesised GCM results, observed data, and other scenario data and visualisation), but for some users/regions/operations access is very slow.

Proposed mirror sites might include: CSIRO (Victoria), IIT (Delhi), NCAR (USA) and Cape Town (S.Africa). Maybe a Japanese site also.

The mirror sites could consist only of the Green Pages (about 0.5GB requirement) or both Green and Yellow Pages (several GB requirement, but I have not checked exactly how much with DKRZ). I know that we can arrange for the mirror sites to automatically refresh every 24 hours therefore reflecting perfectly any developments on the host mother-site (i.e., the mirror sites must be perfect mirrors).

Could I also ask for your views on the desirability of these options, whether Green only or Green plus Yellow, how many mirrors and where they should be? Please let me have your views on this also by Monday 12 April.

****
In considering both these questions it is perhaps worth thinking about the longer-term future of the DDC beyond TAR and into 4th IPCC Assessment. Although TGCIA and the DDC has now only a mandate through the lifetime of TAR, for us to really learn from our experiences and to achieve full benefits for IPCC, then we need to be thinking ahead beyond year 2000.
****

Mike Hulme

ATTACHMENT 1

Subject:
From: Herve.Letreut@lmd.jussieu.fr at internet
Date: 9/7/98 9:08 pm

Dear Maria,

At the IPCC meeting a week ago, I spoke with M. Hulme concerming the possibility of having our simulations being integrated in the IPCC data base (DDA?)

I think that our simulations meet a number of the criteria:
 - the control simulation is 200 years long
 - the model has participated to CIMP1 and CMIP2
 - it is described in details (description posted on the WEb in the Euroclivar Web site: http://www.knmi.nl/euroclivar)

Our main problem concerns the definition of the experiments. We have used a model without flux correction and have decided to start from observed Levitus data. The coupled model has some drift but it stabilizes rather quickly and the thermohaline circulation is quite stable
Accordingly our initial CO2 value corresponds to a recent past: 320 ppm. >From that value we have increased directly the CO2 concentration of 1 percent per year. We have therefore not allowed for an 'historic' increase of the CO2 before the actual 1percent increase, which is due to a lack of understanding of the IPCC rules.

My feeling is that scientifically this is not too important (we have no 'cold start' symptom when we look at the difference between the perturbed and controled run). I have realized that in the context of the IPCC, however, people may think otherwise.

My question is two-fold:
 - Can our experiment nevertheless be integrated in the IPCC data base. This is important to us: if it cannot we will not realize the sulfate experiment we had planned to do, and wait for the future scenarios to be decided.
- I hope that I will be more easily aware of the IPCC initiatives in the future. But is there any procedure through which we can make sure in advance that a given experiment we decide to carry out does get approoved by the IPCC?

Sincerely yours

Herve

Herve Le Treut
Laboratoire de Meteorologie Dynamique, Universite PetM Curie ,
Tour15-25, 5eme etage, boite 99, 4 place Jussieu 75252 Paris Cedex 05
(mail sent to Ecole Normale Superieure also reaches me)
tel: +33 (0)1 44 27 8406   fax : +33 (0)1 44 27 62 72
secretariat du LMD a Jussieu: +33 (0)1 44 27 50 15

ATTACHMENT 2

Hamburg, den 12. February 1999   (15:00)

Dear Maria and Mike,

last week I have a discussion with Herve LeTreut from LMD in Paris about the DDC rejection of the French contribution to the climate scenario calculations. He informed that the climate modellers are running into political difficulties because no French data are contained in the DDC.

We have rejected the data last year because they design of his experiments are not directly comparable to the DDC requirements. A

recalculation is not possible within short term.

In order to prevent the French colleagues from difficulties I suggest to install an additional section in our DDC page which may be entitled 'DDC related modelling results'. In this section Herve`s data as well as data from other groups can be disseminated. The processing priority is certainly lower than for the direct DDC data.

Do you agree with my suggestion?

Best regards, Michael

****
Dr Mike Hulme
Reader in Climatology tel: +44 1603 593162
Climatic Research Unit fax: +44 1603 507784
School of Environmental Science email: m.hulme@uea.ac.uk
University of East Anglia web site: http://www.cru.uea.ac.uk/~mikeh/
Norwich NR4 7TJ
****

Annual mean temperature in Central England during 1999
is about +1.5 deg C above the 1961-90 average
****
The global-mean surface air temperature anomaly for 1998
was +0.58 deg C above the 1961-90 average, the warmest year yet recorded
****

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##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: mann@snow.geo.umass.edu
Subject: Re: ipcc update
Date: Mon Apr 12 13:22:40 1999

Mike
I am off to Finland for a week but I am sending you (via Tim) a copy of a draft perspectives piece for Science on you recent 1000-year reconstruction paper . They want to run it in early May I think and I have been told I will see their edited draft on my return. The idea was to make a wider comment that just report on your latest curve so I decided to mention uncertainties in tree-ring data while pushing the need for more work on high-resolution proxies and especially interpretive work in the very recent context of high temperatures and other possible anthropogenic environmental disturbance. The trouble is that they would only give us 1000 words and one Figure. Anyway this Figure now contains a selection of various large-scale temperature average series - all recalibrated against northern warm season (april-sept) average land data north of 20 degrees north. This is just to provide a convenient common scale - all the original season /area references are given. You will see that this brings phil's curve nicely back in line and the correct (low frequency) density curve now fits better also. I have taken the opportunity to put our new longish (2000-year)tree-ring width curve in representing the north of Europe/Siberia . This is the average of Tornetrask(Sweden) and Yamal and Taimyr(Siberia) - all processed to retain low-frequency variance. These curves and a similar average incorporating all the Northern tree-ring data (not including the large density set) are in my paper for the Pages open science meeting publication. Tim and I will produce a short paper describing the new low-frequency density curve , probably for Geophysical Research Letters. For the meantime I hope you think the perspectives piece is O.K. Let me know if you have any problems with it - but

remember that they are going to hack it about anyway. By the way, how did you compare the high-elevation (PC1) timeseries with Jacoby and D'Arrigo's northern treeline data in your paper when the latter only go back to 1671 ? Did you use their reworked Gidding's dataset for Alaska?
Thanks for the message on the IPCC stuff . I am happy to write any additional bits or make suggestions . Sorry I did not get back to you last time but I was confused about the timetable . Thanks for putting my name on the list. I will make comments again as soon as I see the next draft. Cheers

Keith

At 06:20 PM 4/11/99 -0400, you wrote:
>
>Dear Phil, Keith,
>
>An update on IPCC. Almost done w/ my revisions, taking into account
>yours and Phil's comments, and included the *correct* briffa et al
>series. Keith--added your name in the contributor list. Sorry for
>the earlier omission (I hadn't heard from you at the point I
>wrote the initial draft)...
>
>A couple things--Phil can you send a copy of the in-press Rev
>Geophys. article as soon as possible? I'd like to have a copy
>for my own records...
>
>Also, I'm going to have to leave it to you to insert some
>of the references you mentioned in your comments which I'm
>not familiar with. Also, you'll need to supply an updated
>reference for the Briffa et al series as soon as it is
>ready.
>
>I'll send you the revised draft when I finish it within a day
>or two, at the same time I send it to Chris, Jim, and Jean. We'll
>need to incorporate Pfister's contribution (if it ever comes in),
>and Jim and Jean's suggestions at the next stage. I believe it
>will be Chris' responsibility to coordinate this. Anyways, more
>from me soon...
>
>best,
>
>mike
>_____
>   Michael E. Mann
>_____Current_____Starting Fall 1999_____
>Adjunct Assistant Professor | Assistant Professor
>Department of Geosciences | Dept. of Environmental Sciences
>Morrill Science Center | Clark Hall
>University of Massachusetts | University of Virginia
>Amherst, MA 01003 | Charlottesville, VA 22903
>_____|_____
>e-mail: mann@geo.umass.edu; memann@titan.oit.umass.edu (attachments)
>   Phone: (413) 545-9573   FAX: (413) 545-1200
>   http://www.geo.umass.edu/climate/mike
>

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From: mann@snow.geo.umass.edu
To: k.briffa@uea.ac.uk
Subject: No Subject
Date: Tue, 13 Apr 1999 15:05:02 -0400 (EDT)

Cc: juppenbrink@science-int.co.uk, mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu, t.osborn@uea.ac.uk

Dear Keith

(Tim, please get this to Keith by FAX or other means, if he is unlikely to have received this at his own email while traveling).

It's a good piece overall. As you might suspect, I do have several comments. Ray and Malcolm may send along a few of their own. Malcolm in particular may want to comment on some of your points regarding dendroclmiatic series and our ITRDB PC#1 series which figures so prominently in our millennial reconstruction.

1) page 2, top paragraph:

It's is very misleading to make it sound as if we are strictly reconstructing northern hemisphere mean temperature, and then say "4 of the records are actually from the southern hemisphere locations". This is misleading for a number of reasons. First of all, if one is going after true northern hemisphere areally-weighted mean temperature 0-90 degrees (as we are), then the southern hemisphere tropics are actually more relevant than the high-latitudes of the Northern Hemisphere. Careful diagnostics of surface temperature covariances by Alexey Kaplan, Mark Cane and others have shown this clearly to be true. BUt more than that, we are reconstructing the full 20th century surface temperature domain shown in Figure 1 of our '98 Nature paper. This is a GLOBAL domain, albeit sparse outside the southern hemisphere tropics/subtropics, particularly the southern oceans, for obvious regions. THe proxy network roughly overlaps the spatial domain of surface temperature we are reconstructing (ie, compare Nature '98 figure 1a and figure 1b). We choose to diagnose from this spatial domain the northern hemisphere mean only because that is the hemisphere for which we can meaningfully talk about a true hemispheric mean. But both the predictor and predictand have a global distribution. Without going on and on, I think its clear why your comments here are a bit unfair in how they represent why we use southern hemisphere data. This is probably the most important point that needs to be revised here.

2) page 2, 2nd paragraph

A minor point, but an important one: It is incorrect to say the our uncertainties are based only on "a consideration ...goodnest of fit...over the calibration period"! This is not correct. A key point is that the verification period (1854-1901) diagnostics (though based on a somewhat sparser distribution of gridpoint data from which NH mean temp can be estimated) give very nearly identifical diagnostics in terms of unresolved reconstructed NH mean temp variance. So our uncertainties are based both on 20th century calibration and independent confirmation from 19th century data. PLEASE MAKE SURE this is clear.

On the bigger point being made here, I agree w/ you in principle, and this is a point that Phil has raised too: what we *DONT* take into account (though I challenge anyone to really ever be able to take this into account!) is the unknown potential bias due to degradation from diminishing quality of the underlying proxy data back in time. However, on some of the specific points in that regard, it is very likely not a significant concern in our reconstructions. We closely examined the spectra of the underlying proxy data to insure that those upon which our reconstruction ultimately relies have the amount of

millennial scale trend/variability that would be expected for
a climatic series for at least the null hypothesis of red noise.
Malcolm independently examined the tree ring chronologies underlying
our ITRDB PC #1 to verify that the standardization was appropriately
conservative for a millennial-scale reconstruction. Furthermore,
Malcolm verified that the ITRDB PC #1 is made up of heavily replicated
chronologies as far back as we use them. So neither of the
points you raise appear to be all that relevant to our reconstruction.

With regard to this point, I have some issues with your Figure
that accompanies the piece. It is quite ironic
given your comments about the potential impacts of
standardization on the long-timescale veriations. For our
millennial reconstruction we have verified as carefully as
has ever been verified, that the millennial scale trend is
likely to be meaningful. I don't think you have done so for
the 2000 year-long trend in the series you show, and if you
have not verified that it is likely to have retained 2000 year
long trends, it is VERY misleading to show this series along
with the others. I don't believe that it is likely to accurately
represent the 2000 year long trend in NOrthern Hemisphere mean
temperature, as you imply by showing it here. I think this series
needs to be removed from the plot. I have a related comment
below (point #5).

3) page 3, 1st paragraph:

Remove "this is a moot point" and replace with more appropriate
language. It is not "a moot point" because the problem you point
out has largely been shown to apply to tree ring density data
(which you have largely been using), and much less so tree
ring width data (which we are using). Furthermore, the comparison
only goes through 1980 at which point there is little evidence
that there is a significant declinde in tree ring width response,
although more evidence that there is already a problem at that
point with density data. Your criticism is not quite fair here,
and the statements should be revised to reflect more accurately
on what we have done.

4) page 3, 2nd paragraph:

When you talk about proxy-based ENSO reconstructions, you
should mention our NINO3 reconstruction! This is complementary
to Stahle's SOI reconstruction in a number of ways. The appropriate
references here are both our Nature '98 papers, and the chapter
in Henry Diaz's latest book (in the press):

Mann, M.E., Bradley, R.S., and Hughes, M.K.,
 Long-term variability in the El Nino
 Southern Oscillation and associated teleconnections, Diaz, H.F. & Markgraf,V.,
 (eds) El Nino and the Southern Oscillation: Multiscale Variability and
 its Impacts on
 Natural Ecosystems and Society, Cambridge University Press, 321-372, Cambridge,
 UK, in press, 1999.

if you care to, you can download the galley version here:

   ftp://eclogite.geo.umass.edu/pub/mann/ONLINE-PREPRINTS/ENSO-recon/

in either pdf format (chapter-diaz.pdf) or postscript (chapter-diaz.ps)

5) accompanying figure (see also my point #3):

There are problems with the 2000 year series in terms
of your definition of the baseline for comparing with the
other series, and this differs quite a bit from what
we are likely to be showing in IPCC. It appears that both
the density NH reconstruction and your 2000 year long
series fall at least 0.1C below the other series during
the 20th century, and are probably running at least that
much too cold the whole way through.

Also, correct "global temperature and non-temperature proxies"
in your description of our series to "global climate proxies"
which is a more honest way of describing them given our
methodological approach, and make sure it is clear to the
readers which series are extratropical and warm season, and
which are full northern hemisphere/annual mean estimates (ours).
Such discussion will, again, figure prominently in IPCC, and
it would be a shame for Science to be publishing something
that is misleading in that respect. In part, it was this issue
that forced the publication of a followup to Phil's perspective
by me, Ray, Malcolm, and Phil a year ago, and it would be nice
to avoid that scenario this time around...

_____
Thanks for your consideration of the above comments. I believe
your piece will make an excellent "Perspectives" article for
Science, once these comments are appropriately taken into account.
I'll leave it to the Science editor in charge to determine if
that is the case.

best regards,

mike.

_____
Michael E. Mann
_____Current_____Starting Fall 1999_____
Adjunct Assistant Professor | Assistant Professor
Department of Geosciences | Dept. of Environmental Sciences
Morrill Science Center | Clark Hall
University of Massachusetts | University of Virginia
Amherst, MA 01003 | Charlottesville, VA 22903
_____|_____
e-mail: mann@geo.umass.edu; memann@titan.oit.umass.edu (attachments)
Phone: (413) 545-9573 FAX: (413) 545-1200
http://www.geo.umass.edu/climate/mike

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##########
##########

From: Brian Luckman <luckman@julian.uwo.ca>
To: K.BRIFFA@UEA.AC.UK
Subject: GROVE REVIEW
Date: Tue, 13 Apr 1999 16:33:08 -0400

Keith,

The attachment is in WORD and better formatted.

Brian

Dear Keith,

Enclosed please find my comments on Jean Grove's paper. It gives the impression of a cut and paste job written in haste with several minor annoying errors. It lacks the synthesis I would have expected and reads like a catalogue. The paper is also not as comprehensive as would appear from the title. Six months ago I reviewed a paper by her (for Astrid) on "The Initiation of the Little Ice Age in regions round the North Atlantic". The paper she submitted to you is clearly complementary and reviews " the rest of the world" for comparison with the classic areas discussed in the earlier paper. Yet the earlier paper is only alluded to once (rather coyly) and does not appear in the references. This surely has to be significantly recognised in the title and body of this paper, because as it stands, the review of this earlier (best dated) material is far from adequate.

I cannot speak for most of these data directly but the North American material I am familiar with is not particularly up to date (though in fairness most of Greg Wiles's stuff is still in press). I have sent her under separate cover copies of my Little Ice Age in the Rockies paper (about 6 months ago) and more recently the Luckman and Villalba review paper on glacier fluctuations of the last Millennium along the PEP-1 transect. (copies are on their way to you too).

I think her mixing the discussion of ice core records and glacier histories significantly muddies the waters on whether the term LIA should be used to refer to a glacier or a climate event. I feel this should be addressed and the paper needs a more effective conclusion. She must also decide whether she wants diagrams or tables.

I don't know how she will take these criticisms but, as she is just finishing revising the book, I would have thought she could have presented a better synthesis. I leave it to your judgement as to how to deal with these comments. The paper could be much better but that depends on how much she is willing to reorganise and to some extent rethink what she has written.

I am sending you this e-mail. Do you want me to return the manuscript to you? If you wish I can also e-mail WORD copies of the two papers to you (and her) if you wish a rapid turnaround. But you will only get the diagrams by mail. If I don't hear from you tomorrow I'll just put everything in the mail.

Cheers

Brian

Review of "The Initiation of the Little Ice Age" by Jean Grove

This paper is a useful summary but needs significant fine-tuning and possibly retitling before it should be accepted. The title promises a comprehensive review that the text does not deliver. When I first read this paper I kept asking myself- where is the discussion of all the well-dated early LIA material from Switzerland, Canada and Alaska? Then I remembered

the paper by the author that I reviewed 6 months ago entitled "The Initiation of the Little Ice Age in Regions round the North Atlantic". The present paper is not a global review of evidence but a companion paper that compares the "Rest of the World" with the "European/North Atlantic record" discussed in that earlier paper. The crux of the problem is the first sentence after the title "Little Ice Age Initiation …" at the top of page 3. I initially read this to mean that Holzhauser had submitted a paper on the European record to Climatic Change. Careful re-reading suggests that the author is actually referring to her own review paper. This misunderstanding could be avoided by explicitly acknowledging, in the introduction to the present paper, that the evidence for the circum North Atlantic Region has previously been reviewed by Grove (in press), giving the full citation in the references, and that the section entitled "LIA initiation in regions around the North Atlantic" is a brief summary of that review.

There are a number of general points that need to be made before discussing specifics.

1. This discussion begs the question of how one would recognise the beginning of the LIA (A question I raised in my earlier review) Why, for example is the line drawn between the 8-9th century medieval glacier advances and the 12-13th century ones? Possibly this is related to the author's definition of the so-called Medieval Warm Period which has recently been extensively discussed (Hughes and Diaz 1994). It might be useful to insert a brief discussion of the rationale for this boundary and a definition and defence of the use of the term Medieval Warm Period.in either the introduction or the final discussion section.

2. I also feel that there is a logical inconsistency in the way the author uses the ice core evidence in this paper. In her abstract Dr Grove indicates that "the term LIA refers to the behaviour of glaciers, not directly to the climatic circumstances causing them to expand " (abstract lines 3-4). I agree strongly agree with this usage to differentiate between a glacier event and a climatic event. However, the discussion of the definition of the LIA from the ice core work is based on either periods of greater annual snow accumulation or inferred paleotemperatures from isotopic records. i.e. these definitions are based on climatic events not glacial events. The author should perhaps address this dichotomy and discuss it more fully. If one wishes to argue for retaining the term LIA for the glacial event, it is inconsistent to identify it in ice core records based on temperature (or snowfall) records.

3. The author appears to have an implicit faith in the veracity of 14C dates which I do not share and a disdain for minimum age dating based on lichenometry or dendrochronology. There is a strong emphasis on calendar dated 14C ages throughout this paper and age determinations by other techniques are often significantly downplayed. The paper never specifically addresses the relative errors involved in age determinations by these various techniques. Lichenometry and minimum age tree-ring dating of moraines are disparaged yet, in this timeframe the error terms are almost certainly less than 14C dates from equivalent situations (i.e. dates above glacier deposits or on moraine surfaces). The comments made in this paper about lichenometric dating and dendrochronological dating of moraines (from minimum tree ages) only stress the likelyhood of large errors through the use of these dating techniques. These comments may be appropriate for some moraines that date from the 12-13th centuries but they should not be unqualified, universal statements cannot remain couched in those terms. In most situations lichen and tree-ring minimum ages for moraines of the last 500 years or so are considerably more accurate than 14C ages would be.

4. In my review of her earlier paper I commented that I did not consider that sites in the Canadian Rockies could be described as "around the North

Atlantic". In this paper, it makes no geographical sense to review the results from the Rockies separately from adjacent areas in British Columbia and Alaska which they closely resemble (see Luckman and Villalba, in press). I have no objection to the comment that the Rockies material was discussed in a previous paper (and will therefore not be repeated in detail) but surely in the context of this paper these results should be presented in the discussion of evidence from Western North America. Having recently reviewed the literature for North America I also note there are omissions of significant recent material that is recently published or in press (see Luckman and Villalba attached).

5. The Tables and diagrams appear identical except for Table 10. Tables 1-9 should be deleted?

More detailed and specific comments follow.

Page	Para	Line	
1	3	4	why is lichenometry excluded?
1	4	1	Reference to Grove in press??
1	4	3.	In this paper evidence from…….???
2	2	1-2	Is dating within the last millennium considered to be the critical defining factor in identifying a glacier advance as belonging to the LIA? See comment about the inception of the LIA, above.
2	3	1	delete orphan period before text
3	2	3	Holzhauser 1998 not in the references.
3	2	5	change phrase within brackets to (Grove, in press) and insert in references.
3	3	1	… Rockies dating derived from ring width and…..(revise)
3	3	6	Also Stutfield after 1272 (Luckman , in press)
3	3	11	Luckman 1995, 1996a and b??? (there is no 1995 a and b)
3	3	14	Luckman 1991 not in references. Could be Luckman 1993? Luckman et al. 1997 (never referenced) or Luckman 1996
3	4	3-4	Given the dispute about the universality of the Medieval Warm Period (see Diaz and Hughes 1994) perhaps it would be better to indicate the dating here e.g. 10-13th centuries?
4	2	1-5	based on what evidence? Lichens, historical data , 14C?
4	4		What are these moraine dates based on?
5	2	1	delete comma
5	2	3	1991a or b?
4	1	8-9	snow cover extended? = period of snow cover lengthened between

these dates?

5 3 end of several lines truncated in xerox copy sent to me

6 1 as above

5 3 3 not in references, Haeberli ?? Kuhn references also missing.

6 1 19 reference for Swiss example?

6 1 end negative summer temperature anomalies or negative annual anomalies?

7 1 2 said claimed ? = said or claimed?

7 1 5-8 admitted by who relative to what? This somewhat disparaging comment
seems dismissive. Perhaps lichenometry is the only available technique. Is the author aware whether or not these glaciers ever extended into forested areas. Is there any wood associated with these moraines? Does the evidence presented by these authors and their lichenometric dates indicate the presence of early LIA moraines?

7 2 7 delete end bracket

7 2 last What is being implied here? Were the samples dated of the same
species, were the records long enough to crossdate?

8 Table 1 etc Are these Tables or Figures? The Tables within the text seem almost identical to the diagrams appended at the end.

9 2 1 and Footnote 5; Rothlisberger 1986 not in refs. Rothlisberger and
Geyl??

9 3 2 Figure 2 and Table 2 seem identical which will be used? References should be R and G 1986 not Rothlisberger 1986?

10 1 6 is thought?

10 1 9 geographically close or close in age?

10 1 last sentence surely should come after the next section?

11 1 last The glaciers or monsoon cover 46,000 square kilometers?

12 2 13 Why must it have preceded the LIA? based on a 14C age?

13 Table/Figure 3 explain XXXs

13 1 8 "The Dunde record shows the Little Ice Age clearly" This section
needs to differentiate clearly between the glacier fluctuation record, the snow accumulation record and the isotopic temperature signal. If the term LIA is being used to define/describe glacier events then it cannot also be used- without qualification- to describe climatic events. The author is describing climate signals here not glacier advances. This section and the discussion on page 14 needs more clarification and discussion.

14 1 5 after 1264 based on what evidence?

15-16 Apart from a conference abstract listed in the references but not cited, there are no references to the spectacular work of Wiles in tree-ring dating of overridden forests in this area. In addition, the discussion of the abstract by Yager et al., is somewhat confusing. (how can one have a floating chronology from 911-1992?; are tree-ring dates or calendar equivalent 14C dates being cited here?) This section on Alaska is quite dated (see Luckman and Villalba and several references by Wiles and Calkin cited therein).

16 2 This section needs to be reworked. The data presented for Klinakini Glacier and Franklin Glaciers are presented and then queried without reaching any conclusion. Both indicate glacier advance after the dated materials and the comments qualifying these dates apply equally well to many other dates cited in this paper. (Lag time is ignored at several other sites in the discussion). The reporting of the Bridge Glacier site is incorrect. Ryder and Thomson only identify one advance here, not two and consider both 14C ages provide limiting dates for the same event. The till described is between the paleosol and the present surface not between two paleosols. Although scattered, there are several other papers on this region- Ryder 1987, Desloges and Ryder 1990, Clague and Mathews, 1992 etc - see Luckman and Villalba, in press).

16 As stated earlier, discussion of the Canadian Rockies should be included with western North America. There are also early LIA moraines on Mount Baker in Washington.

18 1 Rothlisberger and Geyh?.

19 1 1-2 Rationale for this statement?

20 1 1-2 See earlier discussion. The ice core data provide information about snow accumulation and climate- not necessarily glacier advances

20 1 end in-situ trees at what site? Again Thompson is referring to a climate event not a glacial event

20 Footnote 13 Based on what data? 1970 predates the 1976 Pacific Climate shift.

21 2 13-14 Again, is this bias? In my experience dating based on the oldest tree for most moraines has far smaller error terms than radiocarbon dating. In this specific case the moraine may be older but this does not justify the statement "approximate at best"

21 2 20 why is Rothlisberger's date of 1000-1220 cal AD acceptable in this circumstance but Ryder and Thomson's date of 1040-1210 (p16) not?

24 footnote 14 although the survey may have delimited glacier area, I assume it was an aerial survey !!

24 3 1 sub-fossil trees.

24 3 5 see comment on 21 2 20 above.

24 3 8 14C dates do not sample! Sample HV.xxx taken from a stump…..etc

26 footnote 16 see Gordon and Harkness, 1992 Quat Sci Rev, 11 697-709 for a comprehensive review

28 2 see earlier comments on ice core discussions.

28 3 4-5 what specifically is meant here? Warmer and cooler intervals for which dates?

29 2 5-6 see above. Lack of obvious period of significantly cooler temperatures?

30 2 1-2 But you don't present any "precise dates" in this table, nor are any of the calendar dendro dates from Alaska included . If this table is intended to be a summary should not it show all of the data being discussed?

31 1 3-4 The implication here seems to be that a 14C date from an in-situ log gives a more precise limiting date for the subsequent glacier event than date from a log that is not in-situ? Is this the case? Or is it that dates on wood are better calibrated than dates on soils, bones or other materials?.

32 1 4 Luckman 1995 I think.

32 1 10-11 These are not dates from moraines but dates from forests overridden by glacier ice. Are there any examples of moraines dated to the 13th century presented in this paper.

Table 10 is never referred to in the text. It needs a caption. Does 13=13th century or 1300s?

33 1 1-3 NO. decreased temperatures or increased accumulation correlated with the LIA have been identified in these cores.

This is not a very synthetic conclusion.

34 Barlow et al., 1997 delete in press

35 Bjork Antarctic

36 Corte CONICET not CONISET

36 Eglington Font

36 Fushimi Initials

36 Fuhimi 1978 delete reference to 1977!

37 include Grove in press

38 Holzhauser 1998??

39 Luckman 1993a should be Luckman B.H., Holdsworth, G and Osborn G.D., 1993
reorder Luckman 1993b as Luckman 1993

40 Luckman 1996b Dendroglaciology not Dendrochronology
Alberta not British Columbia

41 Nesje and Dahl 1991b delete)
Nesje et al., Jostedalsbreen ???
Nesje and Rye Geografiske ? capital G

42 Thompson 1980????

45 Wardle Omoeroa (capital)

Attachment Converted: "c:\eudora\attach\grove.norwich.doc"

98. 0924120405.txt
##########

From: mann@snow.geo.umass.edu
To: k.briffa@uea.ac.uk, mann@geo.umass.edu, mhughes@ltrr.arizona.edu,
rbradley@geo.umass.edu, t.osborn@uea.ac.uk
Subject: oops typo. disregard previous message
Date: Wed, 14 Apr 1999 16:06:45 -0400 (EDT)

Dear Tim,

Thanks for your comments. Some responses to them are
given below. I'll be too busy for further correspondance
as I prepare for travel, leaving Friday morning for a week.

Since I will be away and unreachable through next wednesday.
I would thus request that you and Keith correspond with
my co-authors Ray Bradley (who should be able to
respond upon his return from current travel on Sunday
/Monday) and Malcolm Hughes on the revisions (please cc to
me so I can read upon my return), as I will be unreachable.

I'm sure we can come up with something mutually agreeable
to all of us with this piece, as is my goal with IPCC, as
long as their is proper communication and mutual understanding
by all concerned. Lets strive for this--choice of language
is a nontrivial element...

best regards,

mike.

_____COMMENTS_____
One additional new comment:

0) 1st page, "In attemping to do this...Mann at al...exemplifies"
is unacceptable language to us. We confront the very problems
that are being discussed here, so it is a disservice to us
to say our paper "exemplifies" these problems. It "exposes"

or "confronts" would be fair language, but "exmemplifies" is
unacceptable.

responses to your responses to my original comments:

1) I'm not sure how to interpret your response vis-a-vis
my original comments here. My point is that our use
of southern hemisphere records in the reconstructions
is fundamentally sound, from the point of view of some very
basic principles of optimal interpolation, etc., and given
the domain we are reconstructing, which is not NH only,
although we diagnose NH from our pattern reconstructions
as a key index. There is no basis for what sounds like
a criticism of our use of such data. I couldn't tell
if you were agreeing with this or not from your commments.

2) The uncertainties are determined from the uncalibrated
variance given a certain predictor network. The predictor
network is unchanged from 1820 to present, so the verification
period (1854-1901) unresolved variance is an independent check on the
calibration period unresolved variance. Both gives numbers in
the range of 30% for the NH mean temperature reconstruction,
meaning that the error bars we determine from verification period
are essentially the same as those we determine from the calibration
period. IN this sense, the error bars as determined
from calibration and verification are essentially identical,
The bottom line, if we had used the verification period
to estimate the error bars, the eye would barely see the difference.

There may be a considerable misunderstanding on your/Keiths part, regarding
regarding what is actually shown by the spectrum of calibration residuals
in our GRL paper. It does not in any way conflict with what I indicate
above. What this particular diagnostic shows is that
there is no evidence of any increase in unresolved secular variance
(ie century-scale and longer) in our reconstructions at least
back to 1600. In contrast, there is evidence that such frequencies
are not as well resolved as higher frequencies with the sparser
predictor network available before 1600. Our estimates of
uncertainty TAKE THIS FACT *EXPLICITLY* INTO ACCOUNT. Our uncertainties
estimates are made up of two components that add in quadrature,
including a component of uncertainty in the lowest-frequency variability
as estimated from the spectrum shown, and a component of the
highest-frequency variability from the spectrum shown. THese are
approximated as a step-wise break in the mean (white noise) level of
unresolved variance at the edge of the secular band. Unlike any
previous study, we have actually estimated the increased uncertainty due
to the loss of low-frequency variability as it can best be estimated,
and this is explicitly incorporated into our error bars, which
is why those error bars expand considerably before 1600. This
is discussed in the GRL paper, and is a VERY important fact. It
would be very unfortunate if this fact were misrepresented!

3) I'll leave this to Keith and Malcolm to discuss (Malcom?). I
think it is pretty clear in the paper what our assumptions are
here, and what the justification is of those assumptions. There
is of course room for differing opinions on this stuff, as
it is all somewhat speculative, and we indicate that this is so
in our paper.

4) good enough

5) I really doubt that the 2000 year trend is meaningful and, unlike
the results we have shown, there is no confirmation that these 3

sites accurately reflect northern hemisphere mean temperatures
to any reasonable level during the modern era.

Work by us and others looking at similar
data would suggest that series in such regions are not adequately
representative of the largest-scale trends. There is, further, no
verification of the frequency-domain attributes pass any satisfactory
test. For these reasons, I have informed Julia Uppenbrink directly
that I don't believe this series should be shown in this context.
I agree it is an important series, and it will be appropriate
to discuss it in IPCC. But it should not be considered on a
par with more statistically-verified true Northern Hemisphere
mean temperature reconstructions, and it is very misleading to
show it along with the NH mean reconstructions. The 2000 year
trend runs absolutely counter to everything we know about
the mid holocene. Extratropical Northern Hemisphere summer
temperatures should have been at an absolute peak 4000-6000 ybp,
and the 2000 year trend *ought* to at least be heading in that
direction. The fact that is doesn't, and that the trend hasn't
been verified in the sense discussed above, causes me real
concern. It would be misleading to argue we have any reason
to believe that NH mean temperatures have done what that series
does 2000 years back in time...

Re, the adjustment of the series, I believe it is fundamentally
unsound. Essentially, agreement over the period we can best
constrained (20th century) has been sacrifices for agreement
during the period we can't constrain, apparently for the sake
of getting the different series to align during the 19th century.
Please download the figures I have prepared for the latest IPCC
report.

ftp://eclogite.geo.umass.edu/pub/mann/IPCC/nhemcompare-ipcc.gif

OR

ftp://eclogite.geo.umass.edu/pub/mann/IPCC/nhemcompare-ipcc.ps

You will see how I have aligned the series based on a 1961-1990
reference period for the instrumental series, and a 20th baseline
adjustment for the alignment of all series. To me, this is the
most reasonable adjusment of the series if they are to be shown
together. It also shows the different that latitudinal variations
make EXPLICITLY by showing the difference between our
TRUE (0-90 lat weighted) NH annual mean temp series, and
an extratropical (30-70 deg lat) average from our pattern
reconstructions, which approaches quite closely the Overpeck
et al '97 and Jones et al '98 series. Seasonal distinctions
then the key remain difference. This is, I believe, the
best approach to the comparisons, and the one I will favor
in IPCC.

The alternative is that true NH mean temperatures and
extratropical NH mean temperatures must be shown on separate plots,
because adjusting them the way Keith has provides a misleading
picture, and one that I don't believe can be justified for the
purposes of IPCC, regardless of what you choose to do with your
Science piece.

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##########

From: "Raymond S. Bradley" <rbradley@geo.umass.edu>
To: k.briffa@uea.ac.uk
Subject: CENSORED!!!!!
Date: Mon, 19 Apr 1999 10:41:31 -0400

>Date: Mon, 19 Apr 1999 10:06:52 -0400
>To: juppenbrink@science-int.co.uk
>From: "Raymond S. Bradley" <rbradley@geo.umass.edu>
>Subject: Climate warming prespctives article
>Cc: mann@snow.geo.umass.edu, mhughes@ltrr.arizona.edu
>
>I have just returned from Finland and have now read all the correspondence regarding the Science perspectives article you asked Keith Briffa & Tim Osborn to write. I've sent Tim Osborn & Keith Briffa a few suggestions re their perspectives article. If you would like to see them, let me know.
>I would like to diasassociate myself from Mike Mann's view that "xxxxxxxxxx" and that they "xxxxxxxxxxxx". I find this notion quite absurd. I have worked with the UEA group for 20+ years and have great respect for them and for their work. Of course, I don't agree with everything they write, and we often have long (but cordial) arguments about what they think versus my views, but that is life. Indeed, I know that they have broad disagreements among themselves, so to refer to them as "the UEA group", as though they all march in lock-step seems bizarre.
>As for thinking that it is "Better that nothing appear, than something unnacceptable to us" .....as though we are the gatekeepers of all that is acceptable in the world of paleoclimatology seems amazingly arrogant. Science moves forward whether we agree with individiual articles or not....
>
>Sincerely,
>
>
Raymond S. Bradley
Professor and Head of Department
Department of Geosciences
University of Massachusetts
Amherst, MA 01003-5820
Tel: 413-545-2120
Fax: 413-545-1200
Climate System Research Center: 413-545-0659
Climate System Research Center Web Site:
<http://www.geo.umass.edu/climate/climate.html

100. 0924613924.txt
##
##########

From: mann@snow.geo.umass.edu
To: k.briffa@uea.ac.uk, t.osborn@uea.ac.uk
Subject: Ray's coments
Date: Tue, 20 Apr 1999 09:12:04 -0400 (EDT)
Cc: mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu

Dear all,

Ray accurately (though w/ not the same level of detail) obviously recapitulates my main concerns here. As for the one area of disagreement (not understanding the reason for expecting an overal cooling during the 1st millennium), I'll refer Ray to the appropriate areas of his Paleoclimatology text book, and show him some

additional recent work relevant to this, upon my return.

Thanks again to all for working to make the final product one
we can all be happy with.

best regards,

mike

Michael E. Mann

Current	Starting Fall 1999
Adjunct Assistant Professor	Assistant Professor
Department of Geosciences	Dept. of Environmental Sciences
Morrill Science Center	Clark Hall
University of Massachusetts	University of Virginia
Amherst, MA 01003	Charlottesville, VA 22903

e-mail: mann@geo.umass.edu; memann@titan.oit.umass.edu (attachments)
Phone: (413) 545-9573 FAX: (413) 545-1200
http://www.geo.umass.edu/climate/mike

101. 0925158373.txt
##########

From: "Connie Woodhouse (by way of \"Henri D. Grissino-Mayer\" <grissino@valdosta.edu>)" <woodhous@NGDC.NOAA.GOV>
To: ITRDBFOR@LISTSERV.ARIZONA.EDU
Subject: Re: Problem with "az510.crn": No Correlation
Date: Mon, 26 Apr 1999 16:26:13 -0400
Reply-to: grissino@VALDOSTA.EDU

Dear Steve,

AZ510.crn is a bristlecone pine chronology. I suspect the others you are working with are ponderosa pine or Douglas-fir. In this region, these lower-elevation species have quite a different response to climate than the bristlecone. I haven't worked with the AZ510 chronology, but I would guess that bristlecone tree growth at this site would be favored by warm winter temperatures and perhaps somewhat drier conditions, while the ponderosa and Douglas-fir do well under cool, wet winter conditions. This may be the reason for your poor correlations.

regards,

Connie Woodhouse

Connie Woodhouse
NOAA Paleoclimatology Program
National Geophysical Data Center
325 Broadway
Boulder, CO 80303
ph: (303)497-6297
fax: (303)497-6513
email: woodhous@ngdc.noaa.gov

Institute of Arctic and Alpine Research
Campus Box 450

University of Colorado
Boulder, CO 80309
ph: (303)497-6297
fax: (303)497-6513
email: woodhous@culter.colorado.edu

102. 0925225547.txt
##

From: Matthew Salzer <msalzer@POSTAL.AERO.UND.EDU>
To: ITRDBFOR@LISTSERV.ARIZONA.EDU
Subject: AZ510: No Correlation
Date: Tue, 27 Apr 1999 11:05:47 -0500
Reply-to: grissino@VALDOSTA.EDU

Steve:

I've had some experience with bristlecone pine on the San Francisco Peaks and you are correct in noting their lack of correlation with precipitation records and with other precipitation sensitive tree-ring chronologies like Slate Mtn. Ponderosa. There is no "problem" with the AZ510 chronology; it is, as suggested by Dave, Connie, and Jim, a chronology constructed from trees whose growth is not primarily limited by precipitation. Site location and tree species are critical when comparing chronologies and evaluating climate - tree growth relationships.

We've collected in the Peaks recently as part of an ongoing archaeological and paleoclimate project and have built a chronology extending back to 663 BC, more than 1200 years longer than the AZ510 chronology collected by Don Graybill in the early 1980's. We're working on a temperature reconstruction from this chronology that should prove to be a valuable addition to the already extensive archive of southwestern USA paleoenvironmental research.

Matt Salzer

Laboratory of Tree-Ring Research
University of Arizona
msalzer@ltrr.arizona.edu

Upper Midwest Aerospace Consortium
msalzer@aero.und.edu

103. 0925507395.txt
##

From: "Mitchell, John FB" <jfbmitchell@meto.gov.uk>
To: 'Mike Hulme' <m.hulme@uea.ac.uk>
Subject: RE: GEC paper
Date: Fri, 30 Apr 1999 17:23:15 +0100

see inserts

jfbmitchell@meto.gov.uk
Hadley Centre for Climate Prediction and Research
The Met. Office, Bracknell
RG12 2SZ UK
Tel +44 1344 856613/6656
Fax+44 1344 856912

> -----Original Message-----
> From: Mike Hulme [SMTP:m.hulme@uea.ac.uk]
> Sent: Friday, April 30, 1999 12:31 PM
> To: Mitchell, John FB
> Subject: RE: GEC paper
>
> John,
>
> Could you have a quick look at this paragraph (see below) from the GEC
> fast-track paper. I do not understand:
>
> a) why CO2-doubling forcing for CM2 is cited (see your original email at
> the end of this message) as 3.26Wm-2 when I thought it was 3.471Wm-2 (I'm
> sure I've seen 3.471Wm-2 cited elsewhere for HadCM2).
[Mitchell, John FB] 3.471 in longwave, 3.26 when shortwave also taken into account. Unfortunately modellers do not always make clear how they have estimated their CO2 forcing.
> and
>
> b) why the forcing curves in the plot William Ingram sent show higher
> forcing in CM2 than CM3 (by about 0.5Wm-2) when the CO2-doubling forcing
> is
> *lower* in CM2 cf. CM3.
[Mitchell, John FB] HadCM2 is 1%/year increase in CO2 which is only approximately equivalent to IS92a. Hadcm 3 is "95a" - in fact "95a" I think differs only from in the conversion of the 92a emissions to concentrations, so strictly speaking is not an emissions scenario. As far as I know, Tom never did explain why his concentrations in 1995 were different form the ones Jonathan and I derived using his 1992 model- I think CH4 liffetimes and the CO2 sink were the main factors.
> [is this solely due again to the difference between IS92a and IS95a
> concentrations?]
>
> and
>
> c) why the global-mean warmings in CM2 and CM3 are quite similar when CM3
> has a higher sensitivity than CM2 (3.3 to 2.5K over the next century) and
> CM3 also has a higher CO2-doubling forcing (3.74Wm-2 to 3.26Wm-2, or
> 3.47Wm-2 - see a)). Surely this should lead to faster warming in CM3 cf.
> CM2?
[Mitchell, John FB] See above - HadCM2 uses 1%/year increase in CO2, which gives a greater forcing than HadCM3, even after the effect of explicit trace gases is added in.
(about 0.5Wm-2 by 2100). The greater climate sensitivity does not make as big a difference as one would expect. The difference in CO2 forcing per doubling is not the issue- the net forcing is, and that has ben calculated taking the difference in CO2 response into account
M aybe I have misinterpreted something here.

> Thanks,
>
> Mike
>
> _____
>
> Paragraph from GEC paper ......
>
> "In HadCM3, greenhouse gas concentrations were increased from their 1860
> values up to present (1990) as observed and then following the IPCC
> emissions scenario IS92a (Leggett et al., 1992) from 1990 to 2100. Only
> one simulation was carried out. The increase in radiative forcing during
> the twenty-first century is very similar to HadCM2, being only 0.5 Wm-2
> (about 10%) smaller by 2100 than in the HadCM2 experiment (Figure 2).

> Note
> that the ratio of the increases in CO2 concentration (HadCM2/HadCM3) is
> much greater than the ratio of the changes in radiative heating. There is
> a greater increase in heating in HadCM2, so a greater increase in CO2 is
> required to produce the same fractional increase in heating. Also,
> because
> the heating due to doubling CO2 in HadCM2 is less than in HadCM3 (3.26
> Wm-2
> compared to 3.74 Wm-2), a larger increase in CO2 is required to give the
> same change in heating. Note also that the increase in forcing varies as
> the logarithm of the change in CO2 concentration."
>
>
> At 14:54 09/04/99 +0100, you wrote:
> >Hi Mike.
> >
> >2xCO2
> >HadCM2   3.26 Wm-2 including stratospheric adjustment and allowance for
> >solar absorption.
> >hadCM3   3.74 Wm-2 as above.
> >
> >
> >Gordon C., C. Cooper, C. Senior, H. Banks, J. M. Gregory, T.C. Johns,
> J.F.B.
> >Mitchell and R. Wood, 1999. Simulation of SST, sea ice extents and ocean
> >heat transports in a coupled model without flux adjustments. Climate
> >Dynamics (provisionally accepted)
> >
> >Note year is 1997
> >Gregory, J. M. and J.F.B Mitchell, 1997. The climate response to CO2 of
> the
> >Hadley Centre coupled OAGCM with and without flux adjustment, J Geophys
> >Lett., 24, 1943 -1946.
> >
> >I will try and look at then text now
> >John
> >jfbmitchell@meto.gov.uk
> >Hadley Centre for Climate Prediction and Research
> >The Met. Office, Bracknell
> >RG12 2SZ UK
> >Tel +44 1344 856613/6656
> >Fax+44 1344 856912
> >
> >> -----Original Message-----
> >> From: Mike Hulme [SMTP:m.hulme@uea.ac.uk]
> >> Sent: 09 April 1999 14:11
> >> To: Mitchell, John FB
> >> Subject: RE: GEC paper
> >>
> >> John,
> >>
> >> Here is a Word 6 version of the GEC paper. You need to give me two
> >> references (Gregory and Mitchell 1998 and Gordon et al 1999?) and check
> >> through the bits I have added. See especially what I have worded about
> >> CO2
> >> concentrations in Section 7 - quite what we cite for HadCM3 I'm not
> sure.
> >> It depends what the impacts people say about the sensitivity of their
> >> results to CO2 concentrations. I also have a question in the text in
> >> Section 5 for you.
> >>
> >> Figure 10 is not made yet - I thought I would produce this inter-model
> >> comparison plot for the Amazon given the interesting results we were

> >> getting there.
> >>
> >> I will wait for your comments before sending it to Martin and the other
> >> impacts people, but I must do this by the 19th April at latest.
> >>
> >> I think I understand where the various CO2 numbers come from now.
> >>
> >> Regards,
> >>
> >> Mike
> >>
> >>
> >> At 11:59 09/04/99 +0100, you wrote:
> >> >Dear Mike,
> >> > I think we have traced where the different CO2 values have come from
> >> >   HadCM2   HadCM3
> >> >   assumed 'correct'   assumed   'correct'
> >> > 2020s   441   470   457   434
> >> > 2050s   565   590   574   528
> >> > 2080s   731   770   712   638
> >> >
> >> > The left hand HadCM2 value we think comes from SA90 - Peter Cox will
> >> >check. The second HadcM2 value is notional- I don't think the
> >> inconsistency
> >> >between the the columns matters that much, since there is no "correct"
> >> >HadCM2 value.
> >> >   The HadcM3 values do matter. The right hand side value is
> >> >what was used in the model, and what Willaim took from the TOM Wigley
> as
> >> >being the SAR IS95a values. I do not know where these are publicaly
> >> >available, and I have asked Dave Griggs that if we use new scenarios
> (eg
> >> >SRES) in the TAR, they are publicly available and well documented. The
> >> left
> >> >hand column appears to be from the 1992 IPCC supplement.(The annex by
> >> >Mitchell and Gregory). This used the then current UEA enrgy
> >> balance/carbon
> >> >cycle model to convert CO2 emissions to concentrations. I presume the
> >> >discrepancy comes from changes to the carbon cycle model and anything
> >> elses
> >> >affecting the conversion from emissions to concentrations.
> Unfortunately,
> >> as
> >> >far as I can tell, the SAR never refers to these or explains why the
> >> >concentrations are different.
> >> >   This could easily happen again. The situation with the new
> >> >SRES scenarios to me seems rather chaotic, anad again they are
> emissions
> >> >scenarios, not concentration scenarios. The initial GCM runs will use
> CO2
> >> >concentrations from one particular model. The TAR may report (probably
> >> will
> >> >report) different values since they will use a different model. The
> best
> >> >thing is to talk to the people who set up the GCM run to find out
> exactly
> >> >what was used in the model
> >> >
> >> > With best wishes
> >> > John
> >> >
> >> >
> >> >jfbmitchell@meto.gov.uk

> >> >Hadley Centre for Climate Prediction and Research
> >> >The Met. Office, Bracknell
> >> >RG12 2SZ UK
> >> >Tel +44 1344 856613/6656
> >> >Fax+44 1344 856912
> >> >
> >> >> -----Original Message-----
> >> >> From: Mike Hulme [SMTP:m.hulme@uea.ac.uk]
> >> >> Sent: 08 April 1999 17:35
> >> >> To: N.W.Arnell; Sari Kovats; Matt Livermore; parryml@aol.com;
> >> Andrew
> >> >> White; jfbmitchell@meto.gov.uk; gjjenkins@meto.gov.uk;
> >> >> r.nicholls@mdx.ac.uk
> >> >> Subject: HadCM3 CO2 concentrations
> >> >> Importance: High
> >> >>
> >> >> Dear Fast-trackers,
> >> >>
> >> >> In putting the scenario paper together for the GEC issue, John
> Mitchell
> >> >> and
> >> >> I have come up with slightly different CO2 concentrations for HadCM2
> >> and
> >> >> HadCM3 to what we had earlier assumed. These CO2 concentrations
> will
> >> >> really have to appear in the scenario paper to be consistent with
> the
> >> GCM
> >> >> experiments. Given the differences from the values (I think) you
> have
> >> all
> >> >> used in the impacts work, what significance does this have for your
> >> work?
> >> >>
> >> >>

	HadCM2		HadCM3	
	assumed	'correct'	assumed	'correct'
2020s	441	470	457	434
2050s	565	590	574	528
2080s	731	770	712	638

> >> >>
> >> >>
> >> >> The difference is that the assumed HadCM2 concentrations are
> 20-30ppmv
> >> too
> >> >> low while the assumed HadCM3 concentrations are 20-70ppmv too high.
> >> >>
> >> >> The assumed HadCM2 concentrations came from Cox and Friend (they had
> >> >> already run Hybrid with these concentrations before the FT work got
> >> under
> >> >> way, so we adopted their values). I cannot yet trace where the
> assumed
> >> >> HadCM3 concentrations came from, but the 'correct' values are what
> both
> >> >> John Mitchell and the IPCC (1996 report) have calculated for the
> IS92a
> >> >> scenario.
> >> >>
> >> >> Your suggestions on how best to handle this inconsistency would be
> >> >> appreciated. How big a difference do these differences make to your
> >> >> impacts?
> >> >>
> >> >> Thanks,

> >> >>
> >> >> Mike
> >> >>
> >> >>
> >> >>
> >>
> **
> >> >> ***
> >> >> Dr Mike Hulme
> >> >> Reader in Climatology tel: +44 1603 593162
> >> >> Climatic Research Unit fax: +44 1603 507784
> >> >> School of Environmental Science email: m.hulme@uea.ac.uk
> >> >> University of East Anglia web site:
> >> >> http://www.cru.uea.ac.uk/~mikeh/
> >> >> Norwich NR4 7TJ
> >> >>
> >>
> **
> >> >> ***
> >> >> Annual mean temperature in Central England during 1999
> >> >> is about +1.5 deg C above the 1961-90 average
> >> >> **********
> >> >> The global-mean surface air temperature anomaly for 1998
> >> >> was +0.58 deg C above the 1961-90 average, the warmest year yet
> >> recorded
> >> >>
> >>
> **
> >> >> ***
> >> > << File: gec.fasttrack.doc >>
> >

104. 0925823304.txt
##
##########

From: Trevor Davies <t.d.davies@uea.ac.uk>
To: m.kelly@uea.ac.uk,j.palutikof@uea.ac.uk,k.briffa@uea.ac.uk,
m.hulme@uea.ac.uk,p.jones@uea.ac.uk
Subject: Re: CRU Board
Date: Tue, 04 May 1999 09:08:24 +0100

Mick,

CONFIDENTIAL

I think I'm missing out on something here (refer also to Keith's email
where he talked about "CRU being railroaded by ENV"). My recollection was
that it was agreed that I should approach Reading to see if they are up to
anything & sound out if they might be interested in talking about a joint
bid. The suggestion may have been mine originally, but I do not have
absolute recollection over that. Southampton have approached us via the
Registrar and via Peter Liss. As far as I am aware, nobody from UEA has
approached them (although I have certainly argued with Jean that we should
at least talk with them).

I now have a leaked document which spells out some of the research
councils' thinking. I will get a copy over to CRU today. Please keep this
document within the CRU5, since it may compromise the source. NERC and
EPSRC are signed up. ESRC are not yet. Given the EPSRC stake, it will

certainly be be useful to get RAL etc involved. The funding might be 2million per year. That might imply that the Councils favour multi-site, clusters, etc, but they stress they have no preconceptions.

Given some of their requirements, the JIF bid may be useful.

An important requirement seems to be to attract an "internationally renowned and charismatic scientist" to be overall Director. Do you think we should sound out Schneider? Watson? ??

Trevor
At 11:17 01/05/99 +0100, Mick Kelly wrote:
>I can't make the re-arranged date so here is my input on some of the items
>I know are on the agenda:
>
>National Climate Centre:
>
>1. I feel even more strongly after learning more of the opposition that we
>should make a single site bid and capitalise on our proven track record as
>the only UK university which has covered and can cover all aspects of the
>climate issue from hard science to policy and philosophy.
>We should
>continue to firm up our links with NERC institutes, Hadley Centre, etc.
>But if we reach out to other universities we will:
>a) reveal what we see to be our sectoral weaknesses - a very bad strategic
>move
>b) have to split what is a limited pot of cash
>c) create a potential adminstrative monster that we know ERSC don't like
>from CSERGE experience
>d) weaken our comparative advantage as the place where all aspects of the
>issue are covered.
>It's my understanding that the CRU 5 have already decided in previous
>discussions that this is the way we should go? Trevor - do you want to
>argue against this? It's notable that we haven't been approached by other
>universities!
>
>2. Kerry reckons that likely limited lifetime of ESRC presence
>(Global Env programme office) at SPRU means it's not worth approaching
>them - so I haven't.
>
>3. I propose a working group be set up to move forward the centre proposal
>and ensure coordination/representation of views. 2 from CRU Bd,
>2 from CSERGE (Kerry and Neil?), Dean. Chair from CRU would be my vote -
>this should not all be loaded on Trevor's shoulders.
>
>Studentships
>To report on situation re my proposals:
>1. Craig Wallace (ex MSc) is reserve candidate (joint with Tim Osborn).
>2. My candidate for my solo topic was switched to the ESRC/NERC
>interdisciplinary bid by the studentship committee even though I'd told
>them we definitely couldn't put him forward for this - so that's
>scratched. They thought my topic was not NERC-friendly - but didn't tell
>me this till after the event. A number of phrases spring to mind but maybe
>they were just having a bad day.
>3. My feeling is best tactic for next year
>if we want more students - do we or are we at saturation point? - is to
>advertise early (now?), advertise applicants must have/be in line for a
>first or MSc with distinction, ensure we get feedback on topics from the
>committee and submit candidates early on in the process. Obvious, really.
>
>CRU 5 employment/salaries situation
>What is the current situation?
>

>AOB: Desk space for students
>Can I repeat that I think we should have policy on registration only ie
>post three year grad. students to be adopted when Nick finishes and before
>we hit the next late submitter? My feeling is a desk for 6 months then
>they move out to our overflow rooms in ENV. We should prioritise desk
>space in CRU for first year students. What does ENV do in this situation?
>
>Regards
>Mick
>
> _____
>
>Mick Kelly Climatic Research Unit
>University of East Anglia Norwich NR4 7TJ
>United Kingdom
>Tel: 44-1603-592091 Fax: 44-1603-507784
>Email: m.kelly@uea.ac.uk
>Web: http://www.cru.uea.ac.uk/tiempo/
>_____
>
>
>

++++++++++++++++++++++++++
Professor Trevor D. Davies
Dean, School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
United Kingdom

Tel. +44 1603 592836
Fax. +44 1603 507719
++++++++++++++++++++++++++

105. 0925829267.txt
##
##########

From: mann@snow.geo.umass.edu
To: k.briffa@uea.ac.uk
Subject: Re: Perspective Science piece
Date: Tue, 4 May 1999 10:47:47 -0400 (EDT)
Cc: mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu, t.osborn@uea

Hi Keith,

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

best regards,

mike

p.s. I mentioned to Phil it would be nice to get at least one
spatial pattern of your summer dendro temperature estimates into

IPCC, along with a pattern or two from our multiproxy recons.
I haven't heard back to Phil, but perhaps you can make a specific
suggestion, and send me an appropriate postscript file? It's
not too late to get this to Chris Folland for inclusion in the
initial draft. Thanks in advance...

Michael E. Mann

Current	Starting Fall 1999
Adjunct Assistant Professor	Assistant Professor
Department of Geosciences	Dept. of Environmental Sciences
Morrill Science Center	Clark Hall
University of Massachusetts	University of Virginia
Amherst, MA 01003	Charlottesville, VA 22903

e-mail: mann@geo.umass.edu; memann@titan.oit.umass.edu (attachments)
Phone: (413) 545-9573 FAX: (413) 545-1200
http://www.geo.umass.edu/climate/mike

106. 0926010576.txt
##########

From: mann@snow.geo.umass.edu
To: p.jones@uea.ac.uk
Subject: Re: Straight to the Point
Date: Thu, 6 May 1999 13:09:36 -0400 (EDT)
Cc: k.briffa@uea.ac.uk, mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu,
t.osborn@uea.ac.uk

Hi Phil,

SOrry that you have taken such a negative spin from this. I had hoped it was
all resolved pretty amicably, and emphasized to Keith and Tim that I was
being perhaps overly picky this time PRECISELY to avoid the misunderstanding
that happened last time around w/ Science.

Trust that I'm certainly on board w/ you that we're all working towards a common
goal. That is what is distressing about commentarys (yours from last year, and
potentially, without us having had approprimate input, Keith and Tim's now) that
appear to "divide and conquer". The skeptics happily took your commentary last
year as reason to doubt our results! In fact, your piece was references in several
commentaries (mostly on the WEB, not published) attacking our work. So THAT is
what this is all about. It is in the NAME of the common effort we're all engaged
in, that I have voiced concerns about language and details in this latest
commentary--so as to avoid precisely that scenario.

Please understand the above to be a complete and honest statement about the source
of my concerns. It really doesn't have anything to do about who did what first, etc.
I trust that history will give us all proper credit for what we're doing here.

The millennial-scale trend issue appears to be a source of contention. Malcolm can
address the replication issue better than any of us--it's not a problem w/ our
reconstruction. Furthermore, WE HAVE EXPLICITLY TAKEN INTO ACCOUNT THE LOSS OF
LOW-FREQUENCY VARIANCE IN OUR ESTIMATES OF UNCERTAINTY. I don't know how many times
I need to stress this. It is of fundamental importance in framing our conclusions.
Our own analysis convinces me that things are already quite uncertain a millennium
back in time. With regard to longer timescale variations, the evidence is all
over the place. At EGS I saw some convincing evidence that many new paleo proxies
indicate steadily decline at least over several millennia, and so do, in large part,
the available long borehole estimates (though we should all take that w/ a good
dose of NaCl). So I'm skeptical of estimates more than a millennium back in time

until we have multiple proxies we can trust at that timescale, and can verify somehow the DC component of the estimates, or at least replicate them. This was my concern about the latest 2000 year recon that was shown.

You are right, the Milankovitch forcing argument is ONLY A NULL HYPOTHESIS. I hope I haven't argued anything more than that. That our millennial scale trend, which we reasonably trust, and have some idea of the uncertainties in, is in line w/ that null hypothesis is information that cannot be ignored. That Kutzbach, Berger, and others are showing increasingly convincing model integrations over several millennia suggesting this, is more evidence. In the real word, anything *could* have happened. But lets not loose site of the appropriate null hypothesis here.

I hope the above clears things up somewhat. I'm sorry things have been construed in more negative light than I had ever intended. Call me anytime to discuss, here at the office (not sure how well our schedules overlap though).

Thanks, and sorry for the miscommunication here,

mike

Michael E. Mann

Current	Starting Fall 1999
Adjunct Assistant Professor	Assistant Professor
Department of Geosciences	Dept. of Environmental Sciences
Morrill Science Center	Clark Hall
University of Massachusetts	University of Virginia
Amherst, MA 01003	Charlottesville, VA 22903

e-mail: mann@geo.umass.edu; memann@titan.oit.umass.edu (attachments)
Phone: (413) 545-9573 FAX: (413) 545-1200
http://www.geo.umass.edu/climate/mike

107. 0926012905.txt
##########

From: mann@snow.geo.umass.edu
To: p.jones@uea.ac.uk
Subject: Re: Straight to the Point
Date: Thu, 6 May 1999 13:48:25 -0400 (EDT)
Cc: k.briffa@uea.ac.uk, mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu, t.osborn@uea.ac.uk

HI Phil,

Thanks for your message. I'm with you 100%, and honestly am very much looking forward
to moving towards close collaboration between us. I've already talked a bit w/ Tim about
those plans and the possibility of him spending some time in Charlottesville, etc. Will
be in touch w/ you guys soon about trying to solidify some of these plans...

Yes, I will be in the Lion's den, so to speak. Not sure how much must stands behind his roar though...We do have to deal w/ the skeptics here somewhat directly. At least,
to the extent that I do presentations on capitol hill for USGCRP (I do one w/ Jim Hansen and Malcolm on the 17th of this month), I'm a bit in the fray. Mostly, though,
I've been trying to help Mike McCracken and company behind the scenes. We all know what happens when a U.S. scientists becomes a thorn in the side of big business...

Anyways, I'm really happy that the air is cleared. More soon,

mike

Michael E. Mann

Current	Starting Fall 1999
Adjunct Assistant Professor	Assistant Professor
Department of Geosciences	Dept. of Environmental Sciences
Morrill Science Center	Clark Hall
University of Massachusetts	University of Virginia
Amherst, MA 01003	Charlottesville, VA 22903

e-mail: mann@geo.umass.edu; memann@titan.oit.umass.edu (attachments)
Phone: (413) 545-9573 FAX: (413) 545-1200
http://www.geo.umass.edu/climate/mike

108. 0926026654.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@snow.geo.umass.edu
Subject: Straight to the Point
Date: Thu, 06 May 1999 17:37:34 +0100
Cc: k.briffa@uea.ac.uk,t.osborn@uea.ac.uk,mhughes@ltrr.arizona.edu,
rbradley@geo.umass.edu

Mike,
Just back from two weeks away and from discussions with Keith and Tim and some emails you seem quite pissed off with us all in CRU. I am somewhat at a loss to understand why. It is clear from the emails that this relates to the emphasis placed on a few words/phrases in Keith/Tim's Science piece. These may not be fully resolved but the piece comes out tomorrow. I don't want to open more wounds but I might by the end of the email.
I've not seen the censored email that Ray has mentioned but this doesn't, to my way of working, seem to be the way you should be responding - ie slanging us all off to Science. We are all trying to work together for the good of the 'Science'. We have disagreements - Ray, Malcolm, Keith and me have in the past, but they get aired and eventually forgotten. We have never resorted to slanging one another off to a journal (as in this case) or in reviewing papers or proposals. You may think Keith or I have reviewed some of your papers but we haven't. I've reviewed Ray's and Malcolm's - constructively I hope where I thought something could have been done better. I also know you've reviewed my paper with Gabi very constructively.

So why all the beef now ?

Maybe it started with my Science piece last summer. When asked to do this it was stressed to that I should discuss how your Nature paper fitted in to the current issues in paleoclimatology. This is what I thought I was doing. Julia Uppenbrink asked me to do the same with your GRL paper but I was too busy and passed it on to Keith. Again it seems a very reasoned comment.

I would suspect that you've been unhappy about us coming out with a paper going back 1000 years only a few months after

your Nature paper (back to 1400). Ray knew all about this as he was one of the reviewers. Then the second Science comment has come out with a tentative series going back 2000 years. Both Science pieces give us a chance to discuss issues highly relevant to the 'science', which is what we have both tried to do.

Anyway that's enough for now - I'll see how you'll respond, if at all.

There are two things I'm going to say though :

1) Keith didn't mention in his Science piece but both of us think that you're on very dodgy ground with this long-term decline in temperatures on the 1000 year timescale. What the real world has done over the last 6000 years and what it ought to have done given our understandding of Milankovic forcing are two very different things. I don't think the world was much warmer 6000 years ago - in a global sense compared to the average of the last 1000 years, but this is my opinion and I may change it given more evidence.

2) The errors don't include all the possible factors. Even though the tree-ring chronologies used have robust rbar statistics for the whole 1000 years (ie they lose nothing because core numbers stay high throughout), they have lost low frequency because of standardization. We've all tried with RCS/very stiff splines/hardly any detrending to keep this to a minimum, but until we know it is minimal it is still worth mentioning. It is better we (I mean all of us here) put the caveats in ourselves than let others put them in for us.

3) None of us here are trying to get material into IPCC. I've given you my input through the review of the chapter in Asheville. I may get a chance to see the whole thing again at some stage, but I won't be worried if I don't.

I can't think of a good ending, but hoping for a favourable response, so we can still work together.

Cheers
Phil

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

---

109. 0926031061.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@snow.geo.umass.edu
Subject: Re: Straight to the Point
Date: Thu, 06 May 1999 18:51:01 +0100
Cc: k.briffa@uea.ac.uk,mhughes@ltrr.arizona.edu,rbradley@geo.umass.edu,
t.osborn@uea.ac.uk

Mike,
  We'll differ a bit on a few points, but let's wipe the slate
clean and get back to improving our estimates of past changes
over the last millennium.
  I must admit to having little regard for the Web. Living over
here makes that easier than in the US - but I would ignore the
so-called skeptics until they get to the peer-review arena. I know
this is harder for you in the US and it might become harder still
at your new location. I guess it shows though that what we are
doing in important. The skeptics are fighting a losing battle.

Cheers
Phil

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

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110. 0926087421.txt
##
##########

From: James Hansen <jhansen@giss.nasa.gov>
To: D Parker <deparker@meto.gov.uk>
Subject: Re: Temperatures
Date: Fri, 07 May 1999 10:30:21 -0400
Cc: ckfolland@meto.gov.uk, imacadam@meto.gov.uk, p.jones@uea.ac.uk,
makis@giss.nasa.gov

Hi, David,

I don't think that Antarctic is the principal source of differences. When
we compare only the common areas it doesn't really come into play. There
are areas in Mexico and Northern Africa that seem to contribute more to the
differences. Makiko will put the plots that you requested at
http://giss.nasa.gov/~cdmss/Parker

Regards, Jim

At 05:35 PM 5/5/99 +0100, D Parker wrote:
>To Jim Hansen jhansen@giss.nasa.gov
> (& copies to Chris Folland, Ian Macadam, Phil Jones)
>Jim

>
>Thanks for the mailed illustrations comparing your surface temperature data
>set with Phil Jones's.
>
>We are trying to understand the cooling of your data relative to Phil Jones's
>in the Southern Hemisphere during the 1990s (Table 1 below) in the annual
>series you sent to Ian Macadam. Plots of these were shown at the IPCC meeting
>in Asheville in March and showed the same relative cooling, but Figure 2 of
>your mailed illustrations does not show it. I note that the comparison in
>Figure 2 was made over the common area. If you use all available grids, do
>you get the relative cooling in the GISS dataset? I expect you will, because
>I have been perusing your web site and have noted that most recent years are
>cold over Antarctica in your dataset. This could be the focus of the problem,
>as your stations (with 1200km influence) will have more weight than Phil's
>unless you use common grids.
>
>As an aside, recent cooling over Antarctica could be partly forced by ozone
>losses, though I note that the cooling is strongest in March-May, not in
>Sept-Nov when the ozone hole occurs. If Antarctica cools, there will be
>consequences for Southern Hemisphere atmospheric circulation patterns,
>conceivably even contributing to the recent cooling of marine air temperature
>relative to sea surface temperature.
>
>To help further, can you provide annual maps, 1989 through 1998, of Jones
>(land), GISS (stations, 1200 km) and Jones minus GISS in the format of Figure
>3 of your mailed illustrations? Web or ftp access would be better than
paper,
>if possible.
>
>Thanks and regards
>
>David   5 May 1999
>
>   **
>
>Table 1.   Annual Southern Hemisphere Anomalies (deg C) Relative to 1961-1990
>
>   GISS   Jones
>
> 1990   0.250   0.30
> 1991   0.265   0.32
> 1992   0.023   0.14
> 1993   -0.027   0.24
> 1994   0.033   0.35
> 1995   0.069   0.37
> 1996   0.191   0.23
> 1997   0.033   0.34
> 1998   0.317   0.60
>
>
>   **
>
>
>
>   David E Parker
>   Room H001
>   Hadley Centre for Climate Prediction and Research
>   Meteorological Office
>   London Road
>   BRACKNELL
>   Berkshire
>   RG12 2SY
>   UNITED KINGDOM

>
>
>   Tel +44-1344-856649
>   Fax +44-1344-854898
>
>   email   deparker@meto.gov.uk
>
>

James Hansen
NASA Goddard Institute for Space Studies
2880 Broadway, New York, NY 10025
e-mail jhansen@giss.nasa.gov
212-678-5500 fax (678-5622)

111. 0926681134.txt
##

From: "Raymond S. Bradley" <rbradley@geo.umass.edu>
To: k.briffa@uea.ac.uk
Subject: vomit
Date: Fri, 14 May 1999 07:25:34 -0400

Excuse me while I puke...
Ray

>From: mann@snow.geo.umass.edu
>Date: Wed, 12 May 1999 13:00:09 -0400 (EDT)
>To: juppenbrink@science-int.co.uk, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk
>Cc: mann@geo.umass.edu, mhughes@ltrr.arizona.edu, p.jones@uea.ac.uk,
>   rbradley@geo.umass.edu
>
>Dear all,
>
>Thanks for working so hard to insure a final product that was
>acceptable to all. I think that Keith and Tim are to be
>commended on a fine job w/ the final version of the
>Perspectives piece that appeared, and I thank Julia for her
>especially difficult editorial task.
>
>I appreciate having had the opportunity to respond to the
>original draft. I think this opportunity is very important
>in such cases (ie, where a particular author/groups work
>is the focus of a commentary by someone else), and hope
>that this would be considered standard procedure in the
>future in such instances.
>
>I think we have some honest disagreements amonst us about
>some of the underlying issues, but these were fairly treated
>in the piece and that's what is important (The choice of
>wording in the final version was much better too. Wording
>matters!).
>
>Thanks all for the hard work and a job well done. I like
>to think that may feedback helped here--so I take some
>pride here as well.
>
>best regards,
>
>mike

>
>_____
>   Michael E. Mann
>_____Current_____Starting Fall 1999_____
>Adjunct Assistant Professor | Assistant Professor
>Department of Geosciences | Dept. of Environmental Sciences
>Morrill Science Center | Clark Hall
>University of Massachusetts | University of Virginia
>Amherst, MA 01003 | Charlottesville, VA 22903
>_____|_____
>e-mail: mann@geo.umass.edu; memann@titan.oit.umass.edu (attachments)
>   Phone: (413) 545-9573   FAX: (413) 545-1200
>   http://www.geo.umass.edu/climate/mike
>
>
Raymond S. Bradley
Professor and Head of Department
Department of Geosciences
University of Massachusetts
Amherst, MA 01003-5820
Tel: 413-545-2120
Fax: 413-545-1200
Climate System Research Center: 413-545-0659
Climate System Research Center Web Site:
<http://www.geo.umass.edu/climate/climate.html

112. 0926947295.txt
##
##########

From: Dave Schimel <schimel@cgd.ucar.edu>
To: Shrikant Jagtap <sjagtap@agen.ufl.edu>
Subject: RE: CO2
Date: Mon, 17 May 1999 09:21:35 -0600 (MDT)
Cc: franci <franci@giss.nasa.gov>, Benjamin Felzer <felzer@ucar.edu>, Mike Hulme
<m.hulme@uea.ac.uk>, schimel@ucar.edu, wigley@ucar.edu, kittel@ucar.edu,
nanr@ucar.edu, Mike MacCracken <mmaccrac@usgcrp.gov>

I want to make one thing really clear. We ARE NOT supposed to be working
with the assumption that these scenarios are realistic. They are
scenarios-internally consistent (or so we thought) what-if storylines.
You are in fact out of line to assume that these are in some sense
realistic-this is in direct contradiction to the guidance on scenarios
provided by the synthesis team.

If you want to do 'realistic CO2 effects studies, you must do sensitivity
analyses bracketing possible trajectories. We do not and cannot not and
must not prejudge what realistic CO2 trajectories are, as they are
ultimatley a political decision (except in the sense that reserves and
resources provide an upper bound).

'Advice' will be based on a mix of different approaches that must reflect
the fact that we do not have high coinfidence in GHG projections nor full
confidence in climate ystem model projections of consequences.

Dave

 On Sun, 16
May 1999, Shrikant Jagtap wrote:

> Friends,
>
> I'm enjoying the current debate about CO2 levels. I feel that we are using
> the GCM scenarios, and we MUST use exactly those CO2 levels for crop model
> runs, so all data is consistent. So if we are wrong, we are uniformly wrong
> and adjust our explanations accordingly whenever we agree on things. Now to
> use different data will be hard to explain.
>
>
> Shrikant
>
> Dr. Shrikant Jagtap
> 104 Rogers Hall, Ag & Biol. Engineering
> University Of Florida
> Gainesville, FL 32611
> Tel: 352 392 7719 (Work) & Fax: 352 392 4092 (Work)
> http://www.agen.ufl.edu/~sjagtap/ssj/
>
> Tel: 352 379 0698 (Home)
>
>
>
>
>
> -----Original Message-----
> From: franci [mailto:franci@giss.nasa.gov]
> Sent: Saturday, May 15, 1999 3:58 PM
> To: Benjamin Felzer
> Cc: Mike Hulme; schimel@ucar.edu; wigley@ucar.edu; kittel@ucar.edu;
> sjagtap@agen.ufl.edu; nanr@ucar.edu; Mike MacCracken
> Subject: Re: CO2
>
>
> dear ben,
>
> You just showed that the Hadley transient run we are supposed to use for the
> national assessment is too high, forcing-wise, because it assumes an overall
> 1.2% increase in total forcing.
>
> My question is then the following:
>
> -why are we using a 1% annual increase in GHG forcing (corresponding to the
> 1.2% increase) as a criteria for GCM simulations to then be used for the
> national assessment? Is it because of the possible confusion you refer to
> below? If so, that criteria needs to be revised.
>
> I still have a problem with the real CO2 calculations, in connections to
> hadley or CCCM. It seems to me it is still arbitrary to use one or another
> CO2 curve.
> However, in this arbitrariness, two easy solutions are possible (i am just
> summarizing previous e-mails, at the cost of being highly repetitive and
> obvious):
> -one is dave's, i.e, assume no change i GHG forcing mix from today, and
> apply 1% compounded increase to the 1990 actual levels.
> That gives a concentration of real CO2 in 2100 that is > 1050 ppm. THAT'S
> 50% higher than projected by IS92a, and even 17 % higher than the worst
> emission case devised in IS92f.
> -the second is tom's. Just use the co2 in IS92a, and assume that all other
> further changes necessary to get the hadley forcing (whatever they are)
> happen in GHG other than CO2.
> I will repeat that I like the latter solution.
>

>
> Whatever the consideration of self-consistency and physics are when you make
> this decision, I do not think we should carry out the national assessment by
> using "unrealistic" CO2 numbers. I thought the numbers that come out of our
> exercises (from the impact side of things) were supposed to serve as some
> basis to be used in the process of decision making at the national and
> regional level. Am i out of line here? There are dozens of people right now,
> out there, including our group at giss, who are gathering data, fine-tuning
> models, making connections among physical and socio-economic variables,
> etc., at a very low "effort spent/retribution received", and then we are
> going to run things at 1000 ppm in 2100?
> As far as my specific contribution is concerned, it surely might make a big
> difference in crop yield changes under climate change whether I use 700 ppm
> in 2100 (the IS92a) curve, or >1000 ppm (the 1% compounded increase).
>
> The problem is the same for the 2040's (the other decade we have decided to
> simulate), although possibly not as bad as the 2090's case.
>
> Either solution we opt for, we have to make clear to whomever will receive
> our results that the climate forcing scenario is on the "high" side of
> things.
>
> Ah! It was so nice and easy when we were working with doubled-CO2
> equilibrium runs!
>
> cheers,
>
> francesco
>
> PS what about the CCCM scenario?
>
>
>
>
>
>
>
>
>
>
>
> -----Original Message-----
> From: Benjamin Felzer <felzer@ucar.edu>
> To: franci <franci@giss.nasa.gov>
> Cc: Mike Hulme <m.hulme@uea.ac.uk>; schimel@ucar.edu <schimel@ucar.edu>;
> wigley@ucar.edu <wigley@ucar.edu>; kittel@ucar.edu <kittel@ucar.edu>;
> sjagtap@agen.ufl.edu <sjagtap@agen.ufl.edu>; nanr@ucar.edu <nanr@ucar.edu>;
> Mike MacCracken <mmaccrac@usgcrp.gov>
> Date: Friday, May 14, 1999 8:12 PM
> Subject: Re: CO2
>
>
> >Please disregard the previous message and replace with this message (1st
> >paragraph is unchanged).
> >
> >On Fri, 14 May 1999, Benjamin Felzer wrote:
> >
> >> Going back to some of the original radiative forcing values, it would
> >> appear that the 1% increase is true of RADIATIVE FORCING, whether of CO2,
> >> CH4, etc, or the total (to an approximation). However, once we convert
> >> back to CO2 concentration (using the exponential relationship), the
> actual
> >> increase in concentration is more along the order of 0.7% (all

> >> compounded). Is it possible that the original 1% assumption was
> >> mistakenly applied to CO2 concentrations for the modelers when it was
> >> actually meant for radiative forcing??
> >>
> >Therefore for the ecological models we should use Dave's original
> >suggestion, because the models really did use a 1% increase in equivalent
> >CO2, which approximates a 1% increase in CO2 alone. The point here is
> >that this 1% increase is much higher than IS92a, but that might be because
> >of the confusion between radiative forcing increase and concentration
> >increase discussed above. In fact a 0.7% increase in equivalent CO2 might
> >have been a more realistic assumption for IS92a, but the 1% increase in
> >concentration is what was actually used in these earlier models. The CO2
> >concentrations used in the ecological model should correspond to those
> >used in the GCMs, not to what we think they should be.
> >
> >
> >> Any other thoughts?
> >>
> >> Ben
> >>
> >>
> >>
> >
> >
>
>

113. 0927042520.txt
##
##########

From: David Viner <d.viner@uea.ac.uk>
To: m.hulme@uea.ac.uk, s.raper@uea.ac.uk
Subject: Fwd: Re: Climate Sensitivity
Date: Tue, 18 May 1999 11:48:40 +0100

Mike

The climate sensitivity of HadCM2.....pick a value between 2.5 and 4.1K

D

>Envelope-to: f046@cpca11.uea.ac.uk
>Date: Tue, 18 May 1999 11:27:48 +0100 (BST)
>From: T Johns <tcjohns@meto.gov.uk>
>Subject: Re: Climate Sensitivity
>To: d.viner@uea.ac.uk
>Cc: tcjohns@meto.gov.uk
>Status:
>
>Hi David,
>
>I have just got back from leave today - sorry for the lack of response
>to your emails.
>
>On climate sensitivity, the equilibrium sensitivity in HadCM2 was difficult
>to get a definitive answer for initially as the conventional slab experiment
>was unstable, so we estimated it from part of a transient coupled run
>instead. We quoted 2.5 K in the original Nature paper. Recently we

>have done a HadAM2 slab experiment (modified sea ice and slab ocean physics)
>which indicated 4.1 K rather than 2.5 as an equilibrium value. This is
>quoted in a paper submitted as a CMIP study. The HadAM3 conventional
>slab experiment gave the 3.3 K figure I think. The HadCM2 discrepancy
>indicates the perils of this yardstick; other research here suggests that
>the effective climate sensitivity does respond to climate change feedbacks
>in transient experiments (with HadCM2 particularly). The early 2.5 K
>estimate has been revised upwards based on a long coupled run of HadCM2 to
>be closer to the 3.3 K we got from HadCM3 equilibrium slab experiments.
>
>Comparing transient temperature responses to similar time-varying forcing
>may be a better indication of real sensitivity, but so long as we quote
>single climate sensitivity numbers I fear that there is scope for confusion.
>
>Tim.
>
>PS: I will try to get an update on the HadCM3 references sorted out for you.
>
>> Tim
>>
>> I'm a bit confused as now I have seen a numeber of different values, in
>> HCTN2 you mention that HadAM3 has a climate sensitivity of 3.3 degrees K
>> and that this is similar to HadCM2. Is this the case and is such a value
>> available from a comparable HadAM2 experiment.
>>
>> Many regards
>>
>> David
>>
>> PS Did you get my message about references?
>
#---
Dr. David Viner
Climate Impacts LINK Project
Climatic Research Unit
University of East Anglia
Norwich NR4 7TJ
UK
mailto://d.viner@uea.ac.uk
WWW: http://www.cru.uea.ac.uk/link
WWW: http://ipcc-ddc.cru.uea.ac.uk
Tel: +44 (0)1603 592089
Fax: +44 (0)1603 507784
#---

114. 0927145311.txt
##
##########

From: Tom Wigley <wigley@meeker.ucar.edu>
To: Mike Hulme <m.hulme@uea.ac.uk>
Subject: Re: CO2 concentrations
Date: Wed, 19 May 1999 16:21:51 -0600 (MDT)
Cc: Mike MacCracken <mmaccrac@usgcrp.gov>

Dear Mike,

Yes, I am aware of the confusion surrounding what the Hadley Centre did
and why. It is even messier than you realize. I have forcing data sets
(more than one!) from Jonathon Gregory that differ from the numbers you
gave in your email!! The Hadley people have clearly screwed things up,
but their "errors" don't really matter given all of the uncertainties. I

didn't mention this because I thought that opening up that can of worms would confuse people even more.

In my view (trying to keep things as simple as possible), the key points are these:

(1) The HadCM2 run purports to be IS92a, and it is a good approximation to this.

(2) Their use of 1% compounded for CO2 *is* a reasonable approximation to the IS92a GHG forcing (which, itself, is uncertain).

(3) The climate model output is also uncertain.

(4) The pure CO2 input to IS92a is what I have distributed from the Bern model.

(5) Hence, the best and simplest combination is to use HadCM2 climate output with these (point (4)) *a priori* defined "pure" CO2 concentrations for IS92a.

On Wed, 19 May 1999, Mike Hulme wrote:

> Tom,
>
> Thanks for clarifying your thinking on this.
>
> I still have a problem with HadCM2 forcing and making sense of what Hadley
> have published, esp. the numbers in the Feb. 1997 J.Climate paper by
> Mitchell and Johns. There, they make it clear that the model was presented
> with CO2-equiv. rising from 473ppmv in 1990 to 1414ppmv in 2100, i.e., a 1%
> p.a. increase. This *seems* precise and unambiguous, so I don't think they
> do adjust the CO2-equiv. growth ratio (C2100/C1990) to 3.127 (i.e., about
> 1.05% p.a.) as you suggest.
>
> This concentration scenario yielded a 1990-2100 model forcing of 6.5Wm-2
> (sic), "close to that reported by Mitchell and Gregory in 1992" [Mitchell
> and Johns, 1997] using STUGE (my estimate for that is about 6.2Wm-2). Both
> of these are quite a bit higher than the 5.8Wm-2 forcing in IPCC SAR for
> IS92a. With this (apparently) higher forcing, I reasoned that all else
> being equal, the actual CO2 concentrations that are consistent with HadCM2
> should also be *higher* that those cited in IPCC SAR and hence we could not
> just use the CO2 concentrations from MAGICC (or the Bern model). Hence my
> somewhat higher CO2 estimates of 790ppmv by 2100 were arrived at by using:
>
> pCO2 = 279ppmv * (exp(F/(3.47/ln(2))))) where F is the proportion in
> MAGICC of total forcing due to CO2 alone for IS92a.
>
> The Mitchell/Johns J.Climate paper is confusing, however, because it also
> presents results in their Table 1 which shows a 1990-2100 HadCM2 forcing of
> only 5.5Wm-2 (sic), a value that relates to their text-cited value of
> 6.5Wm-2 only by using DQ of 5.05Wm-2 (i.e., the sensitivity of HadCM2)
> rather than DQ = 6.3Wm-2. Yet the text of the paper continues to imply the
> HadCM2 forcing is '12% higher' than Kattenburg, rather than 5% lower.
>
> The bottom line ... the IS92a SAR forcing of 5.758Wm-2 and DQ of 6.3Wm-2
> only yields a CO2-equiv. growth rate of just over 0.8% p.a., rising to
> nearly 0.9% p.a. if the HadCM2 DQ of 5.05Wm-2 is used. These are still
> some way short of 1% p.a.
>
> Regards,
>
> Mike

> p.s. this is now more a matter for my own curiousity since I agree that for
> most assessment purposes the Wigley/Joos numbers are the best to use.
>
> At 15:36 18/05/99 -0600, you wrote:
> >Dear all,
> >
> >I've just read the emails of May 14 onwards regarding CO2. I must say
> >that I am stunned by the confusion that surrounds this issue.
> >Basically, I and MacCracken are *right* and Felzer, Schimel and (to a
> >lesser extent) Hulme are *wrong*. There is absolutely, categorically no
> >doubt about this. Let me explain.
> >
> >(1) The Hadley Centre run is meant to simulate the climate change
> >consequences of the full IS92a emissions scenario.
> >
> >(2) In this scenario, there are the following concentration and forcing
> >changes over 1990-2100:

Item	C(2100)	DQ(1990-2100)
CO2	708	4.350
CH4	3470	0.574
N2O	414	0.368
Halos		0.315
TropO3		0.151
GHGs		5.758
SO4 (dir)		-0.284
SO4 (indir)		-0.370
TOTAL		5.104

> >
> >These are the numbers I used in Ch. 6 of the SAR. They do not agree
> >precisely with numbers in Ch. 2, because I used the models and formulae
> >embedded in MAGICC. The differences between Ch. 2 and Ch. 6 are
> >irrelevant to the present issue.
> >
> >(3) How does one simulate the combined effects of all the GHGs in a
> >climate model that only has CO2? The standard way is to take the GHG
> >radiative forcing (ending in 5.758W/m**2 in 2100 in this case) and
> >convert this to *equivalent* CO2 concentration changes. If one uses
> >the old (IPCC90) forcing formula for CO2 (which is what was used in the
> >SAR), viz DQ=6.3 ln(C/C0), then C(2100)/C(1990) is 2.494. Note that the
> >1% compounded change would be C(2100)/C(1990)=(1.01)**110=2.988. Thus,
> >1% compounded CO2 gives roughly the correct *forcing*.
> >
> >NOTE THAT THE ACTUAL CO2 CHANGES ARE *NOT* THE CO2 CHANGES USED IN THE
> >MODEL. THE MODEL USES ARTIFICIAL CO2 CHANGES, SCALED UP TO ACCOUNT FOR
> >FORCING FROM OTHER GHGs.
> >
> >NOTE THAT THE ACTUAL CO2 CHANGE IS FROM 354ppmv IN 1990 to
> >708ppmv IN 2100. THIS IS *NOT* A 1% COMPOUNDED INCREASE.
> >
> >NOTE, FURTHER, THAT WHAT MIKE HULME SUGGESTS IN HIS POINT 8 IS ALSO
> >WRONG. IT IS WRONG TO *BACK OUT* THE CO2 FROM FORCINGS. THE CO2 WAS
> >SPECIFIED A PRIORI.
> >
> >NOTE FINALLY THAT MIKE *DOES* GIVE THE 708ppmv VALUE IN HIS POINT 9.
> >USING THIS WOULD BE OK, BUT I RECOMMEND USING THE SLIGHTLY DIFFERENT
> >BERN MODEL RESULTS (SEE BELOW).
> >
> >(4) Now, some minor wrinkles. In the Hadley Centre model for CO2,
> >DQ=5.05 ln (C/C0). Hence, to get a forcing of 5.758W/m**2, they need to
> >use C(2100/C1990)=3.127. Note that this is a little closer to the 1%

> >compounded result than my above calculation. The Hadley Centre may well
> >have used a slightly different total 1990-2100 GHG forcing than mine, so
> >they may have backed out a compounded CO2 increase rate even closer to
> >1% than the above. In any event, if they decided to go with 1%, then
> >this was a perfectly reasonable choice in order to capture the total GHG
> >forcing.
> >
> >(5) The 708ppmv C(2100) value is what comes out of my carbon cycle
> >model. In the SAR, in Ch. 2, we considered results from three different
> >carbon cycle models; mine, the Bern (Joos) model, and Atul Jain's
> >model. For illustrations in the SAR, we used the Bern model. The
> >mid-2100 value with this model, for IS92a, was 711.7ppmv. A later
> >version of this model, used in IPCC TP4, gives 711.5ppmv. Jain's model
> >gave 712.3ppmv.
> >
> >(6) The bottom line here is that, for a consistent pairing of Hadley
> >Centre climate and CO2, one MUST use the ACTUAL CO2 numbers that went
> >into calculating the radiative forcing, NOT the equivalent CO2 numbers.
> >The climate response reflects all GHGs, whereas the plants are
> >responding only to CO2.
> >
> >(7) I am attaching the Joos CO2 time series. I recommend using the
> >actual values rather than trying to fit a compound CO2 increase to
> >them---which, in any event, should not be done using just the end point
> >values. This, however, is your choice. Differences will be negligible
> >in terms of plant response.
> >
> >I hope this clarifies things. It has always seemed pretty obvious and
> >clear cut to me. I hope it will now to all of you.
> >
> >Cheers,
> >Tom
> >
> >
> >   **
> >   *Tom M.L. Wigley   *
> >   *Senior Scientist   *
> >   *National Center for Atmospheric Research   *
> >   *P.O. Box 3000   *
> >   *Boulder, CO 80307-3000   *
> >   *USA   *
> >   *Phone: 303-497-2690   *
> >   *Fax: 303-497-2699   *
> >   *E-mail: wigley@ucar.edu   *
> >   **
> >Attachment Converted: "c:\eudora\attach\Is95a.dat"
> >
>

       ****
       *Tom M.L. Wigley   *
       *Senior Scientist   *
       *National Center for Atmospheric Research   *
       *P.O. Box 3000   *
       *Boulder, CO 80307-3000   *
       *USA   *
       *Phone: 303-497-2690   *
       *Fax: 303-497-2699   *
       *E-mail: wigley@ucar.edu   *
       ****

115. 0927817076.txt
##

From: Tim Osborn <t.osborn@uea.ac.uk>
To: Orson Vandeplassche <ovdplassche@mail.wesleyan.edu>
Subject: Re: tree rings
Date: Thu May 27 10:57:56 1999
Cc: k.briffa@uea

Dear Orson

Very sorry for such a slow reply.

The individual curves (Tornetrask, Taimyr and Yamal) have not been calibrated against their local temperature records yet, and so only exist as standardised (or normalised) anomalies.

For the calibrated Tornetrask record of Briffa et al. (1992), the calibrated reconstruction made use of both tree-ring width and tree-ring density and so it will look different to the ring-width only record shown in the PAGES newsletter recently. For the earlier extension to this record, only ring-width will be available - which is why the calibrated record cannot be simply extended with the new data. Instead, a new calibration needs to be made, using ring-width only. This hasn't been done yet, and - while it *might* be a simple linear regression - sometimes ring-widths from one year and from the previous year are used together as predictors, so I cannot guarantee that it will be a simple rescaling of the uncalibrated curve. Nevertheless, the uncalibrated curve *is* correlated with summer temperature, so it certainly provides useful information.

The average of the three series was calibrated *after* they were averaged, and was calibrated against the April-September mean temperature over all land north of 20N. This was purely for comparison with the other curves shown in our Science piece; for this curve, this region is by no means the optimum, and the temperature anomalies would no doubt differ in magnitude if a regional temperature from northern Eurasia had been used instead. This offers one explanation of why the 650-750 warming differs from Briffa et al. (1992). The second is that only ring-width has been used. The third reason is that it is the average of 3 curves - if the other two don't show the warming, or not as strongly, then of course the signal will be less pronounced in the average. So, you can still use the Briffa et al. (1992) calibration - it is certainly not wrong.

Hope this helps with your choice of what to use.

We will send you a reprint to your Middletown address when they arrive. I am now going to mail you hard copy (black & white) of the Tornetrask uncalibrated ring-width record (annual and 50-yr smoothed) from the PAGES article, and also a hard copy of the calibrated northern Eurasia record from the Science paper. The northern Eurasian record should preferably be referenced using both Briffa & Osborn and Briffa et al.

Best regards

Tim

116. 0929044085.txt
##

##########

From: Phil Jones <p.jones@uea.ac.uk>
To: "Folland, Chris" <ckfolland@meto.gov.uk>
Subject: RE: VARIANCE PROBLEM
Date: Thu, 10 Jun 1999 15:48:05 +0100
Cc: d.parker@meto.gov.uk,t.osborn@uea.ac.uk

Chris,
Sorry to be flooding you with another email, but I was discussing this with Tim. Tim reminded me of a paper that he'd written in that well known journal Dendrocronologia ! I've sent down a copy of the proofs to you both. The paper has been in press for the last 2 years ! This must be the slowest journal in the world. This has some more theory in it and some variance corrections for tree-ring and temperature series.

We are going ahead with the method I've outlined over the last few emails. Tim and I have modified a couple of things slightly :

1) Using the present combined dataset (Jones, 1994 and Parker et al. 1995) we will calculate monthly rbars for each 5 by 5 box. The grid-box time series will be filtered with a 30-year Gaussian filter. rbar will be calculated from the residual grid-box time series. Tim reckons that a longer filter is better (an analysis in the paper). He suggests 40 years, but this involves more problems with the ends, so we'll go with 30. I don't think 20,30,40 will make that much difference to the rbar values.

We are using the combined dataste for the estimation as this should produce better rbar values around coasts and islands. If we used the land only dataset we would have real problems with isolated islands and with some coasts (where all neighbouring boxes will be in one direction from the coastal box).

2) Having got fields of the monthly rbars we'll then apply the formula to the land-only dataset. As you're doing something similar with the marine dataset, we can remerge the two variance corrected datasets using David's merging (growing land and neighbour checking) program.

3) We will then write this up as a small paper for GRL, about the land only results. Both of you can be on this if you want. We can decide later what to do about the merged dataset.

4) applying the correction in real time in the future will mean that we will always be slightly changing approximately the last 15 years data - because of the filter end effects. Best would seem to be to maintain the present version we have and apply this variance correction every few years (eg the IPCC cycle !).

Cheers
Phil

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

---

117. 0929392417.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Paul Valdes <P.J.Valdes@reading.ac.uk>
Subject: Re: PRESCIENT
Date: Mon Jun 14 16:33:37 1999
Cc: njs5@cam.ac.uk

Paul
I have been told PRESCIENT is positive. It has been factored into NERC finances -for the full 8 million I believe. No official written statement has been declared as far as I know but someone from NERC visited here while I was away in Russia last week and talked of a first call for proposals in April 2000. At present this is all I know. Will keep you informed if I here more.

best wishes
Keith

At 04:41 PM 5/29/99 +0100, you wrote:
>Hi Keith,
>
>I met Simon Tett the other day and he said that you thought that the
>thematic proposal had definetely been funded. Is that true? The
>last thing I heard was very promising but not the final word!
>
>Best Wishes
>
>Paul
>
>----------
>Dr. Paul Valdes Dept. of Meteorology,
>Email: P.J.Valdes@reading.ac.uk University of Reading,
>Phone: + 44 118 931 6517 Earley Gate, Whiteknights,
>Fax: + 44 118 931 8905 PO Box 243
> Reading. RG6 6BB. UK
>----------
>
>

118. 0929565152.txt
##########

From: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>
To: k.briffa@uea.ac.uk
Subject: Density data from Polar Urals
Date: Wed, 16 Jun 1999 16:32:32 +0500
Reply-to: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>

Dear Keith,

I am reminding your promise to send me raw density data from Polar Urals

remnants of larches as soon as possible, as I must prepare samples for Fritz until the end of June. Leonid Agafonov will bring them to Slovenia to Fritz.

Tomorrow I will lie down in hospital for 7-9 days, as I get the infection from a tick in Iremel area, not encephalitis, but a new kind of infections from ticks, namely "lime-borrelious" (I do not know its name exactly in English). The sign of this disease is red field approximately 5-8 centimeters in diameter around the point where a tick bite a body. This place itches greatly. If you have such characteristics, you must apply to doctor. This disease is not so dangerous as encephalitis and can be easy recovered from antibiotics. I hope that your tick did nod contained such infection.

I wish you the best.

Sincerely yours,

Dr. Stepan G. Shiyatov

Lab. of Dendrochronology
Institute of Plant and Animal Ecology
8 Marta St., 202
Ekaterinburg, 620144, Russia
e-mail: stepan@ipae.uran.ru
Fax: +7 (3432) 29 41 61
Phone: +7 (3432) 29 40 92

119. 0929719270.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>
Subject: Re: Density data from Polar Urals
Date: Fri Jun 18 11:21:10 1999

Stepan
I am attaching the raw density measurements (max. latewood den.) for the Sob River site as we extracted them from Fritz data bank. The format is Tucson like (index) except for a different header on each sample series. For your purposes the start and end date of each series are shown as the 2 I4 fields in columns 5-12 of these identifier lines. I hope this is all you need. You may also refer to Figure 2a in our paper in the NATO ASI Volume edited by Phil. The article on Low Frequency Signal problems that you are a co author on. This Figure shows the number of density samples through time in this chronology - very low before 1200 and between 1400 and 1600!!

I am sorry to hear of your tick infection. This is no laughing matter and you should ensure that you are well treated and rested. As of yet I have no problems other than worrying about how we will organise future proposals to the EU. Thankyou again for your hospitality and the warm reception from your excellent group. I sincerely hope we will be able to continue our collaboration for many years to come. I hope too that Eugene also feels committed to this working relationship. Perhaps he was tired but I got the impression his priorities were not so much concerned with our work.

I await detailed description of the full network - locations and correspondence with the density network positions and names - that I believe Valerie will work on.

Perhaps the outline and draft of something from Rashit would also be forthcoming soon.

Meanwhile I send my best wishes to you and I await news of your continued health
Keith

At 04:32 PM 6/16/99 +0500, you wrote:
>Dear Keith,
>
>I am reminding your promise to send me raw density data from Polar Urals
>remnants of larches as soon as possible, as I must prepare samples for
>Fritz until the end of June. Leonid Agafonov will bring them to
>Slovenia to Fritz.
>
>Tomorrow I will lie down in hospital for 7-9 days, as I get the
>infection from a tick in Iremel area, not encephalitis, but a new
>kind of infections from ticks, namely "lime-borrelious" (I do not know
>its name exactly in English). The sign of this disease is red field
>approximately 5-8 centimeters in diameter around the point where
>a tick bite a body. This place itches greatly. If you have such
>characteristics, you must apply to doctor. This disease is not so
>dangerous as encephalitis and can be easy recovered from antibiotics.
>I hope that your tick did nod contained such infection.
>
>I wish you the best.
>
>Sincerely yours,
>
>Dr. Stepan G. Shiyatov
>
>Lab. of Dendrochronology
>Institute of Plant and Animal Ecology
>8 Marta St., 202
>Ekaterinburg, 620144, Russia
>e-mail: stepan@ipae.uran.ru
>Fax: +7 (3432) 29 41 61
>Phone: +7 (3432) 29 40 92
>
>
>
>

120. 0929985154.txt
##########

From: sdecotii@ncdc.noaa.gov
To: christy@atmos.uah.edu, clarkea@mar.dfo-mpo.gc.ca, climate@cabel.net,
pfrich@meto.gov.uk, pgroisma@ncdc.noaa.gov, jwhurrell@meto.gov.uk,
m.hulme@uea.ac.uk, p.jones@uea.ac.uk, Jouzel@obelix.saclay.cea.fr,
mann@snow.geo.umass.edu, j.oerlemans@fys.ruu.nl, deparker@meto.gov.uk,
tpeterso@ncdc.noaa.gov, drind@giss.nasa.gov, drobins@rci.rutgers.edu,
j.salinger@niwa.cri.nz, walsh@atmos.uiuc.edu, swwang@pku.edu.cn
Subject: Plan of action for Chapter 2
Date: Mon, 21 Jun 1999 13:12:34 -0400

Below is the text and attached is a file in MSWord regarding a plan of

action for Chapter 2 leading up to the IPCC Meeting in Arusha, Tanzania.

June 21, 1999

Dear Lead Authors and Key Contributors,

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

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

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

Section 2 ----- Tom Karl* and Chris Folland* Executive Summary – total revision and update
Section 2.1 ---- Chris Folland* Changes needed regarding uncertainty guidelines
Section 2.2.1 ---- Chris Folland* Okay for now
Section 2.2.2 ---- David Parker, Phil Jones, Tom Peterson, Chris Folland* Length okay, but reduce number of figures.
Section 2.2.3 ---- John Christy* Check for accuracy
Section 2.2.4 ---- John Christy* Check for accuracy
Section 2.2.5 to 2.2.6 ---- Oelermans*, Nick Rayner, John Walsh, David Robinson, Tom Karl and Chris Folland. Glacier section needs to be updated
Section 2.2.7 ---- Oelermans, Tom Karl* Check for accuracy
Sections 2.3 through Section 2.3.5---- Mike Mann*, Phil Jones Reduce in size by about 10%
Section 2.4 through Section 2.4.5 ----Jean Jouzel* Reduce in size about 10%
Section 2.5 through 2.5.4 ---- Jim Salinger*, Pasha Groisman, Mike Hulme, Wang. Provide a better context for why this section is important, more on upper tropospheric water vapor if possible
Section 2.5.5 ---- Steve Warren, Dale Kaiser, Tom Karl* Add new analyses of cloud amount
Section 2.5.6 ----Jim Salinger*
Section 2.6 through 2.6.6 ----Jim Salinger*, George Gruza, Alynn Clarke, Wang. Reduce in size by at least 50%. Identify a rationale section at the beginning. IPCC 1995 will help here. Some material may go elsewhere. May

need to consult Mike Mann or Jean Jouzel. Please send revised section to Chris Folland to finally review (even if not complete) by 16 August. Chris will feed back changes to Jim by 23 August. Jim Salinger should interact with Chris during this work too. Jim should prepare presentational material
Section 2.7 through 2.7.4 ----David Easterling, Pasha Groisman, Tom Karl* Review for accuracy
Povl Frich: please interact and be prepared to present extremes parts. Jim Salinger: you may have more material on extremes in the South Pacific. Please feed this to Tom Karl and Povl Frich.
Section 2.8 ---- Tom Karl, Chris Folland* Develop a summary, including strawman cartoon

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

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

Cheers and thanks,

Chris and Tom

(See attached file: ARUSHA INSTR LEAD AUTHORS.doc)

National Climatic Data Center

Attachment Converted: "c:\eudora\attach\ARUSHA INSTR LEAD AUTHORS.doc"

121. 0930776203.txt
##########

From: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>
To: k.briffa@uea.ac.uk
Subject: State of health
Date: Wed, 30 Jun 1999 16:56:43 +0500
Reply-to: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>

Dear Keith,

I recovered from tick's infection, at any case I do not have high temperature during the last week. I hope that your health is also good. Now I am preparing for field work.

I selected 32 new samples of dead larch trees from the Polar Urals and sent them to Fritz via Leonid Agafonov. A new version of the chronology will be up to 170 years longer and a better replicated between 1400-1700 AD.

The hard disk is working perfectly, thank you very much.

My best wishes to your family and Phil.

Sincerely yours,

Dr. Stepan G. Shiyatov

Lab. of Dendrochronology
Institute of Plant and Animal Ecology
8 Marta St., 202
Ekaterinburg, 620144, Russia
e-mail: stepan@ipae.uran.ru
Fax: +7 (3432) 29 41 61
Phone: +7 (3432) 29 40 92

122. 0930934311.txt
##
##########

From: Trevor Davies <t.d.davies@uea.ac.uk>
To: c.bentham@uea,p.jones@uea,j.palutikof@uea,p.liss@uea,m.hulme@uea,
r.k.turner@uea,k.brown@uea,j.darch@uea
Subject: Climate change centre info.
Date: Fri, 02 Jul 1999 12:51:51 +0100

>Envelope-to: t.d.davies@uea.ac.uk
>From: "Andrew Watson" <a.j.watson@uea.ac.uk>
>To: "Trevor Davies" <t.d.davies@uea.ac.uk>
>Subject: Climate change centre info.
>Date: Fri, 2 Jul 1999 11:11:01 +0100
>X-MSMail-Priority: Normal
>X-Mailer: Microsoft Outlook Express 4.71.1712.3
>X-MimeOLE: Produced By Microsoft MimeOLE V4.71.1712.3
>
>Hi Trevor
>I was with John Shepherd earlier this week. He told me he
>was phoned up last Friday by Tariq Ali at Imperial College,
>seeking to sign him up to the IC bid; it seems that IC's
>relations with Oxford may have gone sour. If that is the
>case, IC will probably make strenuous efforts to detach some
>of the members of the consortium that UEA is trying to put
>together.
>I was attending a meeting on the "miilliesym" proposal, and
>we were treated to a talk from Ian Dwyer of NERC (new
>position to co-ordinate global change research) on the
>climate change centre. Two things I picked up that I didn't
>know before (but you may) were
>(1) All the decisions, both on the outline proposals and
>full proposals, will be taken by a panel of experts
>(academics from overseas and industry). There will not be
>the normal peer review system. I asked if there would be the
>opportunity to suggest names for this panel, but the answer
>appeared to be no; the panel will be selected and organised
>by the research councils, chiefly NERC.
>(2) The split of funding for the centre will be (per year) 1
>million NERC, 0.75 million EPSRC, 0.25 million ESRC.
>Cheers, Andy
>**********
>Prof Andrew J. Watson
>email: a.watson@uea.ac.uk
> or : a.j.watson@uea.ac.uk

>phone: (44) 1603 593761 direct
>   1603 456161 switchboard
>   1603 507719 fax
>School of Environmental Sciences
>University of East Anglia
>NORWICH NR4 7TJ
>U.K.
>http://www.uea.ac.uk/~ajw/ajw.htm
>**********
>
>

++++++++++++++++++++++++++
Professor Trevor D. Davies
Dean, School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
United Kingdom

Tel. +44 1603 592836
Fax. +44 1603 507719
++++++++++++++++++++++++++

123. 0931964410.txt
##
##########

From: Janice Darch <J.Darch@uea.ac.uk>
To: env.faculty@uea
Subject: Modeling & Data Analysis Research NRA-99-OES-04 <fwd>
Date: Wed, 14 Jul 1999 11:00:10 +0100 (GMT Daylight Time)
Reply-to: J.Darch@uea.ac.uk

--- Begin Forwarded Message ---
Date: Tue, 13 Jul 1999 16:45:56 -0400
From: OES Comments <oescomm@hq.nasa.gov>
Subject: Modeling & Data Analysis Research NRA-99-OES-04
Sender: OES Comments <oescomm@hq.nasa.gov>
To: OESCOMM@caffeine.public.hq.nasa.gov

Reply-To: OES Comments <oescomm@hq.nasa.gov>
Message-ID:
<3.0.32.19990713164217.0069a378@mail.hq.nasa.gov>

Investigations that Contribute to the NASA Earth Science Enterprise's
Modeling and Data Analysis Research

General Information

Solicitation Number: NRA-99-OES-04
Response Date: Sep 27, 1999

Description

NASA is soliciting proposals for investigations that will contribute to
modeling and data analysis research that is
supported by NASA's Earth Science Enterprise. This NRA solicits proposals
directed to the interests of disciplinary
research and analysis, interdisciplinary science, and data analysis
programs that include global and regional modeling
activities and large-scale data analysis, especially model-driven analysis.

It also solicits proposals from instrument science teams and/or guest investigators being newly competed or recompeted in which global and regional modeling and/or
model-driven data analysis constitute major elements of the proposal. This NRA is expected to result in research funding
of approximately $65 million over three years. The individual program elements included in this NRA, and the responsible NASA Program Managers are:

Program Element	Manager
a. Global Modeling and Analysis Program (GMAP)	K Bergman
b. Atmos. Chemistry Modeling & Analysis Pgm. (ACMAP)	J Kaye
c. Phys. Oceanogr. Research & Analysis Pgm. (PORAP)	E Lindstrom
d. Ocean Vector Winds Science Team (OVWST)	E Lindstrom
e. Pathfinder Data Set & Associated Science Pgm.(PDSP)	J Dodge
f. EOS Interdisciplinary Science Program (EOS/IDS)	J Dodge

In keeping with overall NASA goals and those of the Office of Earth Science, research supported by this NRA will be
directed toward demonstrating successful use of data from satellite observing systems, in conjunction with other kinds of
data, to improve models and assimilation systems for the Earth system or one or more of its components.

Participation in this program is open to all categories of domestic and foreign organizations, including educational
institutions, industry, non-profit institutions, NASA centers, and other U.S. agencies. In accordance with NASA policy
as described in Appendix C, all investigations by foreign participants will be conducted on a no-exchange-of-funds basis,
i.e., investigators whose home institution is outside the United States cannot be funded by NASA. Proposals may be submitted at any time during the period ending September 27, 1999. Proposals
submitted to NASA will be evaluated using scientific peer review. Proposals selected for funding will be announced in November, 1999.

All prospective proposers are strongly encouraged to submit a letter of intent (LOI) to propose to this Announcement by
August 27, 1999. This letter should contain a brief description of the research to be proposed. Please see Appendix E of
the NRA for details.

Point of Contact
 Name: Kenneth H. Bergman
 Title: Manager, Global Modeling and Analysis Program
 Phone: (202) 358-0765
 Fax: (202) 358-2770
 E-mail: kbergman@hq.nasa.gov

--- End Forwarded Message ---

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

Tel : 01603 592994

124. 0932158667.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Edward R. Cook" <drdendro@ldeo.columbia.edu>
Subject: Re: Vagonov et al. Nature paper
Date: Fri Jul 16 16:57:47 1999

Ed
  to be really honest, I don't see how this was ever accepted for publication in Nature. It is a confusing paper that leaves me asking what actually have they done and what is the so-called testable Hypothesis of which they speak. Why didn't they do the testing? Yes Sob river is the Polar Urals site and I don't know why they get the results they do for it. Thei precip. trends are dubious and our detailed regional response functions do not show a significant effect of high precip. in winter. I really have not had time to fully digest their message but I can't see why either they or Nature did not ask my opinion of it. My instinctive first reaction is that I doubt it is the answer but we do get results that support a recent loss of low-frequency spring temperature reponse in our data that may be consistent with their hypothesis of prolonged snow lie in recent decades. I have not spoken to Iain yet about the isotope data but I will. If you get any detaied thoughts on the Nature paper please let me know, as I don't know how to respond , if at all.
            best wishes
                        Keith
At 04:11 PM 7/14/99 EDT, you wrote:
>Hi Keith,
>
>What is your take on the Vagonov et al. paper concerning the influence of
>snowfall and melt timing on tree growth in Siberia? Frankly, I can't
>believe it was published as is. It is amazinglly thin on details. Isn't Sob
>the same site as your Polar Urals site? If so, why is the Sob response
>window so radically shorter then the ones you identified in your Nature
>paper for both density and ring width? I notice that they used Berezovo
>instead of Salekhard, which is much closer according to the map. Is that
>because daily data were only available for the Berezovo? Also, there is no
>evidence for a decline or loss of temperature response in your data in the
>post-1950s (I assume that you didn't apply a bodge here). This fully
>contradicts their claims, although I do admit that such an effect might be
>happening in some places.
>
>Cheers,
>
>Ed
>
>
>

125. 0932773964.txt
##########

From: Sarah Raper <s.raper@uea.ac.uk>
To: tar13@meto.gov.uk
Subject: Chapter 13 review
Date: Fri, 23 Jul 1999 19:52:44 +0100
Cc: mnoguer@meto.gov.uk, pvanderlinden@meto.gov.uk

COMMENTS ON CH. 13 (SCENARIOS) FROM TOM WIGLEY
(Page and line numbers are from the May 14 zero order draft.)

****

Dear contributors to Ch. 13,

Here are my comments on your chapter. I think you all know me well enough that you will not be offended by my occasional bluntness. The chapter needs a lot of work (not surprisingly), but it has at least touched most of the bases. It suffers from a lack of overview perspective, making the detail hard to wade through. I was disturbed by the lack of credit given to MAGICC/SCENGEN, since this software already addresses many of the key issues that arise in scenario development.

Apologies for not proof reading this. By the time I got to the end of typing it, I'd had enough.

****

Page 3 (lines 86-89) : Critically, this information doesn't give a full assessment of uncertainties.
3 (110-115) : Sentence too long.
3 (117) : State 'illuminate uncertainty' earlier, since this is a primary purpose of, e.g., MAGICC/SCENGEN.
3 (118) : 'indeterminate' is far too strong.
4 (124-125) : Not clear.
4 (155) : What is 'integrated assessment'? Define and/or explain earlier.
5 (170) : Clumsy grammar.
5 (171-172) : Silly! Scenarios per se do not have ANY uncertainty associated with them, by definition. They are, however, a very (if not the most) useful tool for assessing and quantifying uncertainties. For example, a primary purpose of MAGICC/SCENGEN is to quantify uncertainties. Major text revision is needed to clarify this point.

Part of the problem here is that the boundary between scenarios and predictions/projections is indistinct (as is the distinction between predictions and projections -- this too needs to be clarified). One could argue that 'scenarios' developed using MAGICC/SCENGEN are actually better predictions of some aspects of future climate change than any O/AGCM results. Certainly, 'scenarios' based on scaling are much more than just scenarios as defined here -- they are true predictions, based on some assumed scenario (this is the correct word here!) for future emissions.

Substantial work is required to the present text to clarify these issues -- they are the crux of the matter.

5 (178-179) : Note earlier that scenarios (a word I will continue to use even though it may be inappropriate in many cases) usually define CHANGES in climate. They are not, in these cases, 'scenarios', but 'scenarios of change'. Strict (i.e., absolute) scenarios are then constructed from them by adding the changes to a baseline climatology. This needs to be explained up front.
5 (187) : Delete '(and art)'. This is a derogatory term, likely to be misinterpreted/misrepresented.
6 (220) : Comma after 'scenarios'. The text contains many stylistic and grammatical errors (the most common being the failure to isolate parenthetical clauses). I will assume that someone with a better grasp of grammar will catch all these at some stage, so I will not comment further on them.
6 (225+) : A critical item missed here is inter-variable consistency. Later, consistency between climate and CO2 is

mentioned; but there is no mention of consistency between, e.g., temperature and precipitation, etc. This is a major issue!

7 (257) : Instrumentally-based analogue scenarios were first introduced by Wigley et al. (Nature, 1979). Credit should be given. Also, the USDOE 'State of the Art (sic)' reports (1985) and the Bolin et al. SCOPE report (1986) both review this and other methods. This reviews should be cited.

7 (267-268) : What does 'extrapolating ...' mean?

7 (296) : Wigley et al. (1979) should also be cited here.

8 (306) : Nevertheless, they may do a better job of getting the inter-variable correlations 'right' than GCMs!

8 (315) : Delete 'questionable'. This word is entirely unnecessary here. More importantly, the authors need to be more careful in their choice of words, since there are many critics out there who will be looking for things that can be taken out of context, misinterpreted, or misrepresented.

8 (344-345) : Control run? So what? This is only relevant if the control is used in scenario development. This raises the issue of 'Definition 1' versus 'Definition 2' for defining climate change (a terminology introduced by Santer et al., 1994, JGR). (Later, this difference is attributed to Cubasch et al., but it was first clearly enuncited by Santer et al.) The difference is whether or not one subtracts the control from the perturbed result. More needs to be said about this. It is often assumed that subtracting the control will remove any spurious drift in the perturation experiment. This, of course, is clearly wishful thinking, both a priori, and as shown by Raper and Cubasch (1996). Basically, there is no way to reliably remove drift in a perturbation experiment; which makes it all the more important to have drift-free models. Flux adjustments do not necessarily remove drift -- just look at some of the ECHAM control-run results. There are some very important issues here, central to the use of O/AGCMs in scenario generation. They need better coverage. More is said later, but this is still inadequate.

9 (357) : Yes, they can be different, but so what? The issue is whether the differences are statistically significant. To my knowledge, no one has addressed this issue properly.

9 (358) : I'm sure (at least I hope) you don't mean 'observed'. The issue is the difference between the equilibrium PATTERNS of change and the MODELLED (NOT 'observed') transient patterns of change.

9 (to 361) : You've missed the most inportant point! The advantage of an equilibrium result over an O/AGCM result is that the former is pure signal.

9 (to 376) : The Definition 1 versus Definition 2 issue is relevant here.

9 (379) : Please don't propogate garbage. The issue here is natural internally generated variability. There is no need for such variability to be chaotic, so you should eschew use of this word.

9 (to 387) : I presume here that you are talking about O/AGCMs. You should not use just 'GCM' -- you must be specific. Also, you've missed some vital points: the natural internal variability problem (i.e., output is signal plus noise -- noted elsewhere, but must be stated here); and the model-specific natureof the climate sensitivity.

10(399) : Please give credit to the first work on this (Santer et al., 1990). I should point out that this was actually my idea.

10(404-406) : Totally unclear.

10(420-421) : Poor wording. Should be '.. to which changes are added'.

10(423) : Delete 'appropriate'.

10(429) : Insert 'based' after 'period'.

10(431)   : 'weather generators' comes as a non sequitur here. In any event, you haven't said what they are!
10(435-437) : So what? The issue is what period one is measuring the impacts from. In most cases it will be some nominal 'present-day', so the baseline climatology must refer to the same period. Whether or not the period has some sulphate effect in it is utterly irrelevant.
10(437-438) : What garbage. See above.
11(448-450) : More garbage -- think about it! The reason 1990 is not so useful as a reference 'period' is because the impacts variable is probably not adequately definable over a single year. You have really messed up this issue.
11(460)   : Yet more garbage! Given what I have tried to explain above, it is ludicrous to consider daily data as part of the baseline climatology. The impacts variable may require daily data from a baseline period in order to define ITS reference level (but probably not), but this is NOT the same thing. Either all this is very badly worded, or you don't know what your doing.
11(468)   : No!! Think about it!
11(470)   : No!! This is NOT the reason.
11(473)   : No!! Not 'observed' (which is past or present), but FUTURE data.
11(482-483) : Duplication.
12(to 492)   : This is a very confused paragraph.
12(497-499) : Wrong. For upper air, their is a major paper by Santer et al. (JGR, 1999), which also touches on some surface issues. There are also a number of papers by Trenberth that are relevant.
12(507)   : Again, introduction of an undefined term/concept (downscaling).
12(510)   : At last, mention of changes. Sadly, it is inappropriate here, since this is NOT the reason.
12(514)   : Why should this Figure be here?
12(518)   : Wrong. As a scenario, this could be justified. You are confusing scenario (as you have defined it, which I have already criticized) with prediction/projection.
12(521)   : See above.
12(525-527) : This is the Def. 1 vs Def. 2 issue. However, you have the history and motivation wrong.
12(527-531) : Wrong. This issue has nothing to do with cold start vs warm start; it is to get over the drift problem (which it fails to do).
12(537)   : Not 'especially'; mor appropriate may be 'but only'.
13(543)   : 'were'; grammar!
13(543-545) : Not clear.
13(552-553) : Not clear.
13(579-581) : So what? Given your definition of scenario, this doesn't matter.
14(594)   : Why use 'perceived'?
14(604)   : This issue was first raised by Kim et al. (1987?). It was first addressed in a credible manner by Wigley et al. (1990).
14(606)   : 'appending' is a ridiculous word to use. Try 'adding'.
14(608)   : 'often' to 'usually'.
14(613)   : 'appended' to 'added'.
14(616)   : 'appended' to 'added'.
14(617)   : 'appended' to 'added'.
14(627,628) : Please cite the key initial papers by Kim et al. and Wigley et al.
15(635,636) : Clumsy sentence.
15(638)   : Isn't the word 'physical' usually used? The process does not just involve dynamics.
15(642-648) : Mention of 1-way vs 2-way nesting needed here.
15(657-659) : You have failed to mention the most important reason

for using LAMs, orography/topography.
16(667) : Please cite the key initial papers by Kim et al. and Wigley et al.
16(673) : 'predict and' to 'predictand'.
16(679-683) : Once again, you fail to mention the main advantage; viz. that statistical downscaling involve real-world data and so ensures that inter-variable relationships are realistic. Of course, these relationships may change; but LAMs don't even get the correct relationships for the present.
16(703)-17(716): These are VERY important results. They need far greater emphasis.
17(720) : In Australia? Or anywhere for that matter.
17(723-724) : See, e.g., Wigley (1999 - Pew report- and material cited therein).
17(725) : 'mulitple'?
17(730-732) : Not clear.
17(739-740) : This sentence sounds stupid. Rephrase.
17(744) : You cannot say 'most areas' and then cite only agriculture cases.
17(748) : The first clear exposition of this is in the oft-cited paper by Wigley (Nature, 1985). See also later paper in Climate Monitor.
17(755-756) : I disagree. Both methods have strengths and weaknesses.
18(770) : At last! A definition of 'weather generators'.
18(778-779) : Unclear.
18(798) : What means 'more definitive'?
18(803) : "Wilk's" to "Wilks'".
18(805) : Hence, the work is irrelevant in the present context. Delete irrelevant text.
19(to 821) : Most of the agriculture studies dealing with the effects of variability changes are flawed since they fail to separate the low-frequency effect of induced changes in winter soil moisture levels from the specific effect of within-growing-season variability changes.
19(826-839) : Since this should refer back to lines 823,824, this whole section amounts to a giant non sequitur.
20(880) : One could be much stronger than this. The use of high spatial resolution information is more than just 'warranted', it is absolutely essential. However, there is another approach that you have failed to mention at all. This is 'upscaling' of the impacts model. There is some relevant work on this in papers by Jarvis and McNaughton (and vice versa). Another related approach is the direct modelling of spatial patterns of agricultural yield (as in work by Wigley and Tu Qipu, which relates yield patterns to climate patterns). Presumably one could apply a similar approach to direct modelling of river flow. These approaches complement the rather boring direct approach of downcsaling, and they may well circumvent some of its problems.
20(898) : Under this comes: model errors; sensitivity uncertainties; aerosol forcing uncertainties; lag uncer- tainties, regionalization versus global-mean uncertainties.
21(905) : lesser or greater than what??
21(916) : 'adequacy' is not the right word; hoe about 'appropriateness'?
21(928) : I disagree. Re-analysis data for precipitation are simply not good enough, and precipitation is the key variable in most impact areas. Also, in the regions where scenario data are most needed, real observational data are available. Re-analyses largely provide useful new data in regions where data are not needed. The authors seem not to have thought this through.
21(to 931) : There are two papers by Wigley (conference

proceedings, edited by Hanisch) which address the issue of the relative magnitudes of different sources of uncertainty in global-mean projections (emissions, aerosol forcing, carbon cycle, other trace gases, climate sensitivity). These papers are singularly relevant to this section.

21(939) : Actually, the range for total emissions is from 7.9 to 29.0GtC/yr. For fossil CO2 emissions, the range is 6.5 to 28.8GtC/yr.

21(943) : Not just 'time-dependent evolution', but anything that has a specific time attached to it.

22(948) : The reference to Alcamo et al. here seems either perverse or ignorant. Recall that the topic is CLIMATE scenarios. In this context, MAGICC/SCENGEN is FAR better suited to exploring the consequences (right down the line) of emissions 'uncertainties'.

22(959-960) : MAGICC/SCENGEN already does this at the global-mean level. Furthermore, at least three O/AGCMs have fully embedded sulphur cycles already.

22(968) : 'specifications' is the wrong word. These things are NOT 'specified'.

22(970) : 'determine' to 'have'

22(972) : See also Wigley's Pew report (1999).

22(974-976) : Not straightforward? This really is utter garbage. In MAGICC/SCENGEN, this is extremely easy and straightforward.

22(985) : Ah ha! The 1-way/2-way nesting issue surfaces at last!

22(989-990) : See above.

23(999) : Actually, this issue was first raised in Santer et al. (1990). It has also been addressed in papers by Wigley and Palutikof (probably before anyone else).

23(1010-1011): The wording here is not quite right.

23(1022) : First done in Santer et al. (1990).

23(1030) : If one assumes stable patterns, which has been shown to be okay for the CO2 component of change, then the SNR problem can be minimized by using changes over a long time interval.

23(1033) : This average response method was alluded to in Santer et al. (1990). It was first implemented in ESCAPE and later in MAGICC/SCENGEN. A good illustration of the method, including some relevant discussion of it, is given in the Wigley Pew report (1999). One of the critical aspects of this method (which is not even mentioned here!) is that the results must be normalized by the global-mean temperature before averaging.

24(1040) : Is this the ACACIA program run out of NCAR? This program was established some years ago, and it would be extremely confusing if there were two programs with the same acronym.

24(1047) : Not 'a few', but many -- CMIP1.

24(1060) : 'rations' to 'ratios'.

24(1060-1062): Not clear.

24(1073) : What means 'non-standard forcing'? In my view, something like IS92a forcing would be 'standard', whereas 1% compound CO2 is 'non-standard' (i.e., unrealistic and artificial).

24(1076-1078): Really? Why? I think this statement is wrong. There are a number of ways to determine SNR values from a single O/AGCM run. (Note the continuing confusing use of 'GCM', instead of O/AGCM.)

24(1085) : I don't think 'uncertainties' is quite the right word here. Input emissions scenarios, which are scenarios in the strict sense of the word, do not directly address uncertainty issues (although they can, with some trepidation and a not-inconsiderable amount of ingenuity, be used to define uncertainties). By the way, as far as I can see, the only scenario development method/software that does address the

input and uncertainty issues is MAGICC/SCENGEN.
25(1090) : Again, these are not the most appropriate references. Key references are Santer et al. (1990), and papers on ESCAPE and MAGICC/SCENGEN.
25(1093) : What means 'annotation' here?
25(1102) : Actually, it was my idea.
25(1105,1106): No! The key assumption is actually linear superposition. This is the way that SO4 effects are handled. There are a number of papers that show that this assumption works well for temperature, and a paper by Ramaswamy and Chen in GRL that shows that it works also for precipitation. The tricky thing for this variable would be to prove statistically that it doesn't work. Given the SNR, it would be very difficult to reject the null hypothesis that P(A)+P(B)=P(A+B), where A,B are the forcings and P(.) is the response pattern.
25(1108) : Plus numerous other papers.
25(1112,1113): This is very galling. The method may have been used in IMAGE, but they got it from ESCAPE, which goes back to Santer et al. (1990). MAGICC/SCENGEN pushes the idea as far as is possible. Schlesinger's COSMIC does things quite similarly tp MAGICC/SCENGEN. (Schlesinger was a co-author of the Santer et al. paper.)
25(1115) : Not clear.
25(1122) : All you can say here is 'may not hold', not 'probably does not hold'. Indeed, there are reasons to expect it to hold quite well.
25(1123) : Could begin new paragraph with 'Uncertainties'.
25(1123,1124): I think this statement is categorically wrong. MAGICC/SCENGEN incorporates SO4 influences, as does COSMIC. There is no evidence at all that the uncertainties are thereby amplified. Indeed, there is evidence to the contrary (e.g. Penner et al., 1997). Idle and unsupported speculations like this do nobody any good.
25(1124,1125): I suspect you argument here would have to hinge on the possible spatial effects of a THC slowdown or shutdown. If so, say so. But, if this is the case, you must also note that the latest non-flux-corrected O/AGCMs do not show these major THC changes, and scaling approaches may well work out very well for these situations, even in stabilization cases. Please avoid jumping to unsubstantiated conclusions.
25(1125) : I refereed this paper, and I judged it to be an appalling display of ignorance. It should not be cited.
26(1134) : Why is this Figure here?
26(1138) : Ah ha! At last the normalization issue. This must come much earlier.
26(1144-1147): This is simply wrong. It is true that Ramaswamy and Chen dreamed up a case with big hemispheric-scale responses but little global-mean response, but this was totally unrealistic. In all cases that I have looked at, using the method employed by MAGICC/SCENGEN and COSMIC, this is simply NOT a problem.
26(1147,1148): Again, this is just WRONG!
26(1150+) : Again, this is my idea, and it was first implemented in MAGICC/SCENGEN. Please give credit where due.
26(1156-1159): Isn't this ALWAYS the case. In other words, the scaling method is almost universally applicable and useful.
26(1159-1162): I do not think this has been proven.
26(1164,1165): There are other methods, too.
26(1172) : Oh come on! Scaling handles MANY types of uncertainty (perhaps all), not just 'one type'.
27(1181) : 'documented' to 'quantified'?
27(to 1185) : etc., etc.
27(1193) : MAGICC/SCENGEN allows the user to consider this issue

by providing data on global precipitation pattern correlations. Indeed, this software was the first to consider this issue (in spite of the Whetton and Pittock paper cited on line 1199).
27(1198-1201): Very clumsy text.
27(1203-1204): This is an issue we considered years ago in developing ESCAPE and MAGICC/SCENGEN. The trouble with judging a model on its regional performance is one of statistical significance. It is much easier to get a good regional result by chance than to get results that are good globally.
27(1208-1211): Very clumsy text.
27(to 1214) : You have failed to mention a key issue. Is model skill in simulating present-day climate a reliable indicator of its skill in predicting future climate change? There is no evidence to support this idea, although it does sound a priori reasonable. You must at least raise the issue.
28(1227) : Cite Morgan and Keith (1995) here.
28(1231) : This is a critical point. It needs more emphasis.
28(1235+) : What about inter-variable consistency? This needs to be discussed.
28(1236) : 'the manifold' to 'possible'.
28(1239) : Insert 'give' after 'chapters'.
28(1252) : Not clear.
28(1255) : So what? It is almost certainly irrelevant unless the CO2 changes are bigger than anything anticipated, or unless there are nonlinear effects associated with THC changes (which looks increasingly unlikely).
28(1257) : 'mimics'? You must be joking! How about 'approximates'?
28(1262) : 'equal' (grammar).
28(1262,1263): How can smart people like this make such an elementary mistake!
29(1280,1281): This does not seem to be an appropriate reference.
29(1282) : 'albino' to 'albedo'.
29(1294) : This sea level consistency issue was first addressed by Wigley and Raper (Warrick et al. sea level book). It is, of course, avoided in MAGICC/SCENGEN.
29(1295) : 'dependable' to 'dependent'.
29(1295-1301): A giant red herring! Maybe some ignorant people produced inconsistent scenarios like this years ago, but the issue was also resolved years ago. All you need to say is that comprehensive software suites avoid these naive problems. Concentrate on the strengths of existing methods/software; don't reraise issues that were solved long ago.
29(1305-1308): Another misleading red herring, that fails to reflect the current state of the science. Global-mean responses to aerosol forcing CAN be used to drive regional patterns. This is just what is done in MAGICC/SCENGEN and COSMIC.
29(1310,1311): Not clear.
29(1314) : Delete 'scenario'.
29(1318) : 'to daily' to 'in daily'.
30(1329,1330): 'stimulated new techniques' Oh yeah? The MAGICC/SCENGEN method has not changed in 7 years, and it still represents the state of the science.
30(1332,1333): True, but you have not explained them very well. Could you not have a summary Table that lists the strengths and weaknesses of the various methods, including the direct use of O/AGCM output. This would have helped you a lot in planning and structuring this chapter. It can still help in revising it; and be useful to readers.
30(1336-1339): Not clear.
30(1342) : You have mentioned this before, but you have failed to tell us what it is or given any example. A mention alone is valueless.
30(1344) : What means 'semi-formal'. I thought it was a dress

protocol.
30(general) : A crucial need for scenarios (and for simple models)
is to expand the range of cases covered by O/AGCMs.

END **********

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* Dr. Sarah Raper *
* Climatic Research Unit *
* University of East Aglia *
* Norwich *
* NR4 7TJ *
* *
* Tel. + 44 1603 592089 *
* Fax. + 44 1603 507784 *
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From: Mike Hulme <m.hulme@uea.ac.uk>
To: Jennifer F Crossley <J.Crossley@uea.ac.uk>
Subject: Re: masking of WWF maps
Date: Thu Jul 29 09:13:24 1999

Jenny,

Thanks for these.

After entering into debate with Barrie Pittock, I have decided to shift to using the 1 sigma level as a mask for all maps. This will not affect any of the temperature plots you have done until now, but means that the China and C.America precipitation maps will need re-drawing using 1 sigma. Please let me know when these are done. Note also for Russia and that everything from now on for WWF (both T and P) should use 1 sigma as the mask.

Sorry about this and I realise this squeezes even more time away from the RCM.

Given what has happened and your role in producing these plots, you may interested in the exchanges I have had with Barrie Pittock - it illustrates nicely the nuances of presenting climate scenarios in different Fora. Read these three emails in reverse order.

Mike

Dear Mike,

Thank you for your careful consideration of my "trenchant comments". I am now much happier with what you are doing, and indeed grateful for your hard work and enterprise is getting the new scenarios out so quickly for both IPCC and WWF. Shifting to a one standard deviation is certainly an improvement, along with some discussion of possible changes in extremes. I fully appreciate that analysis of daily output is a time-consuming future task, but meantime an appropriate caveat is needed. Maybe an additional upfront paragraph discussion of the very issues we have discussed re providing best estimates of changes, even if their statistical detectability can only be established after a long

time period has elapsed, would be useful?

I should perhaps explain my delicate position in all this. As a retired CSIRO person I have somewhat more independence than before, and perhaps a reduced sense of vested interest in CSIRO, but I am still closely in touch and supportive of what CAR is doing. Also, I have a son who is now a leading staff member of WWF in Australia and who is naturally well informed on climate change issues. Moreover, Michael Rae, who is their local climate change staffer, is a member of the CSIRO sector advisory committee (along with some industry people as well) and well known to me. So I anticipated questions from WWF Australia, and from the media later when the scenarios are released, regarding the scenarios. I did not want to be in the position of feeling the need to seriously question in public their presentation or interpretation. You have allayed my fears on that score, so that is great.

Roger may still follow up with some more detailed comments he is collating from people in CAR.

Best regards,

Barrie.

_____

Barrie,

Thanks for your trenchent comments re. the scenario maps.

Let's get the bit about extremes out of the way because in what WWF have asked us to do (or what Tim Carter and I have done for WGII) we cannot produce new detailed analyses for all the 15 regions we are doing of GCM-based changes in daily or sub-daily events. Clearly for (some, many?) impacts such changes will be important and we (do and will) make comments to this effect in various places. [By the way, we do show some analyses of changes in the probability of extreme *seasons*, if not extreme days].

Your main point of contention, however, is about the portrayal of changes in mean seasonal T and P (and we are talking about 30-year climate averages here).

My reason for introducing the idea of only showing changes in T and P that *exceed* some level of 'natural' variability was a pedagogic one, rather than a formal statistical one (I concede that using '95% confidence' terminology in the WWF leaflet is misleading and will drop this). And the pedagogic role of this type of visual display is to bring home to people that (some, much or all of) GCM simulated changes in mean seasonal precip. for some regions do *not* amount to anything very large in relation to what may happen in the future to precip. anyway - a classic example is the African Sahel where *none* of the GCMs get precip. changes anything like as large as have been seen this century.

The reasons for this may be 1) because the GHG signal is poorly defined, i.e., a scatter of GCM P changes both above and below zero, and/or 2) because even with a tighter bunching of GCM predictions in one direction these may still not be large relative to 'natural' variations in 30-year mean precip. My approach of taking a pseudo-ensemble of GCMs, standarising and scaling and then plotting the Median *in relation to* natural variations is I think one of the more elegant ways of showing this. Of course, we could define natural variability to be the 1 sigma rather than the 2 sigma level, or simply the interquartile range of control climates or even just the 40-60 percentile range. What one chooses is a matter of judgement and probably for WWF I should use a less extreme threshold than 2 sigma.

The point behind all this is to emphasise that precip. changes are less well-defined

than temp. changes *and* that we should be thinking of adaptation to *present* levels of precip. variability, rather than getting hung up on the problems of predicting future precip. levels. This pedagogic thinking is hard to communicate in a short WWF brochure.

Your concern about my message is well taken, however, and I intend to remove any reference to 95% confidence levels, to re-word the text to indicate that we are plotting precip. changes only 'where they are large relative to natural variability', and to reduce my threshold to the 1 sigma level of HadCM2 control variability (e.g. this has the effect of showing precip. changes for the majority of Australia even in the B1 scenario).

But I do not intend to abandon the concept. I think it important - even for Greenie groups - to present sober assessments of magnitudes of change. Thus making it clear that future changes in T are better defined that future changes in P, and also to point out that future emissions (and therefore climate change) may be as low as the B1 scenario (is B1 climate change negligible? I almost think so), whilst also being possibly as high as A2 is I think very important.

The alternative is to think that such a more subtle presentation is too sophisticated for WWF. But I think (hope) not.

Thanks again Barrie for forcing me to think through this again.

Mike

At 17:52 28/07/99 +1000, you wrote:
>Hello Mike,
>
>I am giving a preliminary response to your suggestion that Peter Whetton
>comment on your scenario material in case there is some urgency. Peter
>did write an email last Friday night before going on a week's holiday,
>but unfortunately the email system failed and it probably did not go and
>has been lost. He asked Roger Jones to respond on behalf of the group
>but Roger is snowed under at present.
>
>Peter and I did discuss it on Friday. Our main concern (although there
>are other more detailed ones) is your use of the 95% confidence limits
>of natural climatic variability as some sort of threshold for change.
>This is a reasonable thing to do if you are addressing the question of
>whether climatic change will be detectable at a "scientific level" of
>confidence, but that is certainly not the question I would expect WWF to
>want answered, nor is it the one most relevant to giving policy advice.
>The relevant question is "What is the best estimate of climate change,
>given the assumption that increasing GH gases will cause change?". The
>contrast between these questions, the statistical criteria they require,
>and thus the answers, is what I was driving at in my comment on your
>paper in Nature. It is a very serious difference with serious
>consequences for how people will interpret your advice. The results as
>you present them suggest that many areas will have precipitation changes
>(particularly) which are small compared to natural variability, and
>therefore it does not matter. But if the change in mean is some
>appreciable fraction of natural variability, say, 50%, that is a very
>serious matter which ought to concern policy makers, because it will
>have cumulative impacts, especially in regard to large changes in the
>frequency and magnitude of extremes (floods and droughts). Surely you
>understand that! - refer to the standard diagrams of the impact on
>extremes of shifting a normal distribution by one standard deviation.
>
>What you are doing is using a strict Type I error criterion when others
>(WWF?) might think a Type II error criterion is more suitable (the

>Precautionary Principle), and reasonable people (like me of course!?)
>think a criterion in between which measures risk of serious impacts is
>what is needed for policymakers. The reference I gave in my comment in
>Nature may not be the best - but look at my argument in QJRMS, 109,
>pp.46-48 (1983) for a clearer exposition on this point.
>
>The other related matter is that your scenarios for WWF, and for that
>matter for IPCC WG2, do not discuss the importance of changes in
>extremes, which are arguably the most important changes, however poorly
>understood they may be at present. This and the other caveats you are
>intending to include in the IPCC material, re scaling, sulfate aerosol
>effects, longer timescales, and change after stabilisation of
>concentrations, should be in the WWF material also, even if they
>complicate things a bit (I have not checked whether some of that is in
>your WWF stuff as yet).
>
>I would be very concerned if the material comes out under WWF auspices
>in a way that can be interpreted as saying that "even a
>greenie group like WWF" thinks large areas of the world will have
>negligible climate change. But that is where your 95% confidence limit
>leads.
>
>Sorry to be critical, but better now than later!
>
>Best regards,
>
>Barrie.
>
>Dr A. Barrie Pittock
>Post-Retirement Fellow*, Climate Impact Group
>CSIRO Atmospheric Research, PMB 1, Aspendale 3195, Australia
>Tel: +61 3 9239 4527, Fax: +61 3 9239 4688, email:
><barrie.pittock@dar.csiro.au>
>WWW: http://www.dar.csiro.au/res/cm/impact.htm
>
>* As from 1 March 1999 I have become a CSIRO Post-Retirement Fellow.
>This means I do not have administrative responsibilities, and am
>working part-time, primarily on writing for the Intergovernmental Panel
>on Climate Change. Please refer any administrative matters or contract
>negotiations for the CIG to Dr. Peter Whetton, the new Group Leader, at
><peter.whetton@dar.csiro.au>, tel. +61 3 9239 4535.
>
>"Far better an approximate answer to the right question which is often
>vague, than an exact answer to the wrong question which can always be
>made precise." J.W. Tukey as cited by R. Lewin, Science 221,636-639.
>
>

127. 0933255789.txt
##########

From: Adam Markham <Adam.Markham@WWFUS.ORG>
To: m.hulme@uea.ac.uk, n.sheard@uea.ac.uk
Subject: WWF Australia
Date: Thu, 29 Jul 1999 09:43:09 -0400
Cc: mrae@wwf.org.au

Hi Mike,

I'm sure you will get some comments direct from Mike Rae in WWF
Australia, but I wanted to pass on the gist of what they've said to me so

far.

They are worried that this may present a slightly more conservative approach to the risks than they are hearing from CSIRO. In particular, they would like to see the section on variability and extreme events beefed up if possible. They regard an increased likelihood of even 50% of drought or extreme weather as a significant risk. Drought is also a particularly importnat issue for Australia, as are tropical storms.

I guess the bottom line is that if they are going to go with a big public splash on this they need something that will get good support from CSIRO scientists (who will certainly be asked to comment by the press). One paper they referred me to, which you probably know well is: "The Question of Significance" by Barrie in Nature Vol 397, 25 Feb 1999, p 657

Let me know what you think. Adam

128. 0933716462.txt
##########

From: mann@snow.geo.umass.edu
To: pedersen@eos.ubc.ca
Subject: No Subject
Date: Tue, 3 Aug 1999 17:41:02 -0400 (EDT)
Cc: calvert@unixg.ubc.ca, k.briffa@uea.ac.uk, rbradley@climate1.geo.umass.edu, weaver@ocean.seos.uvic.ca

Dear Tom,

Thanks for bringing that to our attention...

I checked out that page and, unfortunately what he has done is *so* ridden with problems that it isn't even worth confronting. Many of us (e.g., me, Phil Jones, Henry Pollack, Shao-Yang Huang, Rob Harris, and others) have been scratching our heads trying to find a statistically defensible way of combining the information in boreholes and "conventional" proxy indicators, and as yet it is not clear if it can be done, given in particular the loss of information due to geothermal diffusion, and the overriding important of land-usage changes and snowcover variations, on borehole temperature profiles. I don't think Hoyt has added anything scientifically productive in this regard. Looks more like he has wrecklessly convoluted borhole data with our reconstructions to get just the kind of result he wants to get...

Of course, there are issues with regard to secular trends in dendroclimatic reconstructions (which form an important, but not exclusive, role in oure reconstructions) and nobody is better qualified to discuss these than Keith, or Malcolm Hughes, who have highlighted these issues in recent publications (there is a link to a nice recent "Nota Bene" Science piece by Keith and Tim Osborn on my webpage:
http://eclogite.geo.umass.edu/climate/mike/mbh99.html

With regard to "Co2 fertilization", it is ironic that Hoyt frames his analysis in these terms, when it precisely this effect (for better or for worse)

we took great pains to account for in our recent millennial temperature reconstruction (see the above web page for more info). At least, we have done this in a reasonably statistically-defensible, if imperfect, manner, rather than an ad hoc attempt to get an answer, rather than follow a scientifically meaningful process.

This thing wouldn't have a chance at passing peer-review (at least, not on this planet), so he posts it on the web--the downside of absolute freedom of dissemination I suppose. The material in question is the scientific equivalent of trash, plain and simple.

Like a lot of the "skeptics" out there, D.H. appears far less interested in honest scientific discourse, than in misleading as many unlucky soles as possible who wander into his den of disinformation (kind of like the "scientist" equivalent of an Ant Lion I suppose).

Every once and a while, I do choose to respond to this type of crap (e.g., with regard to Pat Michaels--my soon-to-be "neighbor"'s recent pieces in his "World Climate Report"). In D.H.'s case, I doubt even more that this would be at all productive. We just have to wait and see if he ever tries to get this kind of thing published in the peer-reviewed literature. For our part, I think the best approach is to, as Jonathan Overpeck has so effectivley been doing, try whenever possible to educate the lay public about the essential distinction between peer-reviewed science and un-peer-reviewed...., well, whatever you want to call it.

Again, thanks for the head's up on this.

best regards,

mike mann

>X-Sender: tfp@pop.unixg.ubc.ca
>Date: Tue, 3 Aug 1999 13:36:36 -0700
>To: rbradley@climate1.geo.umass.edu
>From: Tom Pedersen <pedersen@eos.ubc.ca>
>Subject: Skeptics
>Cc: calvert@unixg.ubc.ca (Steve Calvert), k.briffa@uea.ac.uk,
>   weaver@ocean.seos.uvic.ca
>
>Hi Ray:
>My colleague, Steve Calvert, has just brought to my attention a website of
>which I was unaware but you probably know well. It's at
>http://www.erols.com/dhoyt1
>and run by Doug Hoyt.
>Amongst other things Hoyt has taken the Mann reconstruction and
>reconstructed it by "removing the effect on tree ring thickness that
>results from CO2 fertilization" (paraphrased). You will see the figure on
>his site. He concludes that there is no significant warming in the last
>half of this century relative to the last millenium. Do you know this guy?
>Are you familiar with his reconstruction of your reconstruction? Didn't
>Keith Briffa correct his tree-ring reconstructions for CO2 fertilization?
>[Keith: any comments?]. Steve and I would be most interested to hear your
>collective comments...
>
>To close this, here is a bit cut and pasted from Hoyt's sight:

>
>
>   Three Final Points
>
>There are three important points to make about the reported warming of the
>last 20 years:
>
>1. The warming has occurred mostly at night and not during the day. This
>result is inconsistent with a warming
>caused by greenhouse gases, but is consistent with urban heat island and
>other surface effects.
>
>2. The reported warming has occurred only at the surface and not in the
>upper atmosphere. This type of warming is
>completely opposite to what is predicted if greenhouse gases are the cause.
>Again these observations are consistent
>with problems in the surface measurements.
>
>3. The warming has occurred primarily in the Northern Hemisphere
>mid-latitudes with little in the polar and tropical
>regions. This result is consistent with urban influences, but is
>incompatible with the climate warming predicted from
>greenhouse gases which predict it to be largest in the polar regions.
>
>In short, the reported warming is inconsistent with warming due to
>greenhouse gases in its temporal, vertical, and
>geographical distribution. The reported warming is consistent with problems
>in the surface network.
>
>
>Cheers, Tom
>
>
>T.F. Pedersen
>Oceanography, Earth and Ocean Sciences, 6270 University Boulevard,
>University of British Columbia, Vancouver, B.C. Canada V6T 1Z4
>Telephone: 604-822-5984   Fax: 604-822-6091   Email: pedersen@eos.ubc.ca
>http://www.eos.ubc.ca/
>
>

_____

Michael E. Mann

Current	Starting Fall 1999
Adjunct Assistant Professor	Assistant Professor
Department of Geosciences	Dept. of Environmental Sciences
Morrill Science Center	Clark Hall
University of Massachusetts	University of Virginia
Amherst, MA 01003	Charlottesville, VA 22903

e-mail: mann@geo.umass.edu; memann@titan.oit.umass.edu (attachments)
Phone: (413) 545-9573 FAX: (413) 545-1200
http://www.geo.umass.edu/climate/mike

129. 0934921858.txt
##########

From: "Karl E.Taylor" <taylor13@llnl.gov>
To: mmaccrac@usgcrp.gov
Subject: to mask or not
Date: Tue, 17 Aug 1999 16:30:58 -0700

Cc: taylor13@llnl.gov, santer1@llnl.gov, wigley@meeker.ucar.edu, p.jones@uea.ac.uk

Mike,

I thought maybe I could contribute a few comments to your concern over using a common coverage mask for surface and MSU temperatures. (Copy of your relevent paragraph copied below.)

Whether or not to mask depends on the question being addressed. If we wanted the best estimate of global mean MSU temperatures, then clearly we wouldn't want to mask. The issues we address, however, are largely based on an expectation (from models and observations) that over large portions of the globe strong vertical coupling tends to lead to large positive correlations between surface and lower tropospheric temperatures. There is a further (model-based) expectation that any warming trend at the surface should be slightly amplified higher up in the troposphere. These expectations seem to be contradicted by the MSU data (at least for global mean trends).

Masking makes most sense if there is in fact strong coupling between the surface and troposphere. Suppose the CO2 warming signal were one with relatively strong warming over land areas and weaker warming over ocean. Suppose further that we only had surface temperature measurements over land, but had MSU retrievals over all the globe. Also assume a case of perfect coupling (1K rise in local upper air temperature for every 1K rise in local surface temperature).

In this case the unmasked global mean MSU temperature increase would be less than the "global" mean surface temperature increase, falsely indicating a damping with height of the CO2 signal. If we masked the MSU temperature (sampling only over land), then the global means would be computed over the same area as the surface temperature and the MSU temperature change would equal the surface temperature change, indicating no damping of the response with height. This second conclusion would be the correct one. Note, however, that the true global mean temperature change (both at the surface and aloft) would be best estimated using the MSU unmasked data (under the conditions of this hypothesized case).

Under different conditions, and again depending on what question is being addressed, it might be best not to mask the MSU data. In our paper we wanted to determine whether the apparent discrepancy between the MSU trend (very small) and the surface trend (positive, and larger) could be explained by coverage differences. This makes sense since models seem to indicate that the trends should be comparable. One explanation for the discrepancy is that in models true global means had been considered until now, whereas in the data the MSU mean was computed from global coverage, but the surface changes were computed from data covering about 70% of the globe. In our study both model data and observations were treated with the same mask so we rule out different sampling as a full explanation for the difference between surface and MSU temperature trends.

Hope this doesn't confuse things further.

cheers,
Karl

Mike wrote (in part):

I think one needs to be very careful about this coverage

argument--basically becuase the atmosphere can move anomalies around compared to the surface. One would just not expect therir spatial patterns to be the same, so taking a common spatial maskwill not resolve this (even if it seems plausible). To illustrate, take an extreme example of there only being sfc msmts for the equatorial eastern Pacific (the El Nino region). There, the MSU and sfc temp go in opposite directions for quite plausible physical reasons. Doing a mask and comparing for that small region would make no sens and give negative correlations, etc. Now, in that sfc obs cover most of the globe, the problem will not be so severe, but it persists (it was for this reason that I was suggesting extrapolating to the global value for sfc temp based on changing coverage--not sure how to do that however). In any case, I believe taht MSU and sfc should only be compared, if at all, for the globe as a whole.

130. 0935431006.txt
##########

From: Rashit Hantemirov <rashit@ipae.uran.ru>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Holocene paper
Date: Mon, 23 Aug 1999 13:56:46 +0500
Reply-to: Rashit Hantemirov <rashit@ipae.uran.ru>

Dear Keith,
I just come back from Yamal. We collected subfossil wood in Yuribey River basin (50-150 km northward of recent timberline) and have found about one hundred remains of trees.
Before departure for Yamal, on July 17, I have sent you draft outline of paper for Holocene. I asked Valery Mazepa to send it one more if any problems in connection. Now Valery is in Polar Ural and I don't know did you receive this outline.
Could you inform me about this.
Thank you.

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
Fax: +7 (3432) 29 41 61; phone: +7 (3432) 29 40 92

131. 0936652724.txt
##########

From: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>
To: k.briffa@uea.ac.uk
Subject: Proposal to IARC
Date: Mon, 6 Sep 1999 17:18:44 +0500
Reply-to: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>

Dear Keith,
Some days ago we have got "JOINT ANNOUNCEMENT OF OPPORTUNITY" from International Arctic Research Center and Cooperative Institute for Arctic Research University of Alaska Fairbanks. The

general theme is Global Change Research in the Arctic (full text with description is attached bellow). As we have read Research Themes from announcement they seem to be very congenial to our laboratory. What do you think about this? Is there point in submitting proposal to IARC and CIFAR at the University of Alaska Fairbanks? Research theme would be 5,000 year summer air temperature reconstruction from tree rings and impacts and consequences of global climate change on forest ecosystems in the Polar Ural and Yamal Peninsula (Subarctic regions of Russia).
We have no wide experience to submit proposal to any foreign administration. We need in some advice. Could you give us a piece of good advice how to do this well.
The questions are:
1. We are not sure whether this action and theme is contrary to our future cooperative work?
2. If not, how big our chance to get award?
3. Could we submit a proposal from our Institute only without U.S. partner? (Proposals from foreign institutions should preferably have a U.S. partner. See description bellow). If U.S. partner should be, who in your opinion would be?

Best regards.
Stepan.

From: ArcticInfo
<arcticInfo@mail.arcus.org>
To: arcticinfo@arcus.org
Subject: IARC Announcement of Opportunity
For more information on these research opportunities contact:
Professor Syun Akasofu, Director, IARC, Phone: 907/474-6012, Fax: 907/474-5662, or E-mail: sakasofu@iarc.uaf.edu.

RESEARCH OPPORTUNITIES
JOINT ANNOUNCEMENT OF OPPORTUNITY
International Arctic Research Center and Cooperative Institute for Arctic Research University of Alaska Fairbanks
Global Change Research in the Arctic

INTRODUCTION
Proposals are invited on topics of global change and its effects in the Arctic (detection; interactions and feedbacks; paleoclimates, arctic haze, ozone and UV; contaminants; impacts and consequences of change). The proposal deadline is 1 October 1999 and awards will be made in January 2000.
DESCRIPTION
The International Arctic Research Center (IARC) and the Cooperative Institute for Arctic
Research (CIFAR) at the University of Alaska Fairbanks announce the availability of funding for global change research in the Arctic. The IARC is a new international research center at the University of Alaska Fairbanks, established jointly with Japan. The mission of the IARC is to provide an environment that will nurture multidisciplinary research by integrating and synthesizing past, present, and future studies in global change.
CIFAR is the NOAA-UAF Cooperative Institute
for Arctic Research; it is combining the resources of its Arctic Research Initiative (ARI) with those of IARC under this announcement. The goal is to develop a focal point for a pan-Arctic synthesis of global change in which researchers from many different institutions throughout the United States and the rest of the world participate to combine their research results. Further details on IARC can be found on its web page at http://www.iarc.uaf.edu/ and on CIFAR at

http://www.cifar.uaf.edu/.

Proposals may be submitted from U.S. or
foreign institutions that address studies on any of the following
themes drawn from the IARC Science Plan and the CIFAR Arctic Research
Initiative. Proposals from foreign institutions should preferably have
a U.S. partner. The starting date for proposed work should be 1
January 2000, with a duration of up to 24 months. Funding for the
second year will be contingent on the availability of additional
funds, therefore each proposal should have a clear, achievable
objective for the first year's work.
RESEARCH THEMES
1. Detection of
contemporary climate change in the Arctic by ground observations,
remote sensing and climate "fingerprinting".
2. Arctic paleoclimatic
reconstructions from ice cores, tree rings, permafrost, lake and ocean
sediments.
3. Atmosphere-ice-land-ocean interactions and feedbacks in
the Arctic that affect change, including observations and modeling.
4. Arctic atmospheric chemistry, arctic haze, ozone and UV radiation and
their effects.
5. Impacts and consequences of global climate change,
including effects on biota and ecosystems in the Arctic.
6. Contaminant sources, transport pathways, and exposure to higher
trophic levels and humans in the Arctic.

It is planned to fund several
large projects and a number of medium ($100K) or smaller projects.
Proposals must include the full cost of logistics support required. A
total of about $ 4.5M is available in year 1 for this Announcement of
Opportunity.
Proposals can request support for the following:
*Research on any of the above six themes. Proposals that add value to
ongoing research projects, or that share costs with other funded
investigators, are encouraged.
* Conducting workshops at the IARC to
further define priorities or synthesize available information on any
of the research themes listed above, or any theme from the IARC
Science Plan.
* Visiting scientists, for short- or longer-term visits,
to the IARC in Fairbanks.
* Development of generally useful curricula
and courses in global change, or conducting global change outreach and
educational activities.
* U.S. participation in the work of the Arctic
Council and its AMAP, CAFF, or PAME working groups.

All proposers
should meet the following conditions:
* PIs must attend an annual
synthesis meeting of all IARC/CIFAR investigators in Fairbanks at
which research results will be presented and working groups will
synthesize results. Proposal budgets should include travel to
Fairbanks.
* All activities will be required to acknowledge the
financial support from IARC and CIFAR in reports, papers,
dissertations, etc.
* Progress reports are due from all funded
projects on 1 August 2000.
* Copies of all publications resulting from
funded projects are to be provided to IARC/CIFAR.

Proposals should not
exceed 15 pages in text and illustrations, not counting CVs, budget
page, and appendices. Further details on proposal preparation are
attached below as an appendix.

Review criteria for research proposals are:
* Does the proposal address the research themes listed above?
*Does it propose high-quality research?
* Does it advance the NOAA mission?
* Is the PI (or are the PIs) well qualified to do the
research?
* Can the research be done in a timely manner?
* Is it likely to lead to significant results?
* Is it likely to contribute to
a synthesis of research results on global change?

Proposals must be
received by 1 October 1999. All proposals will be reviewed by a
scientific peer review panel of prominent researchers that will advise
a program management team drawn from NOAA, IARC, and CIFAR. Funds will
be available in early 2000. Please submit proposals (originals and 10
copies) to the address below. Further information can also be obtained
from the same office.
Professor Syun Akasofu, Director
International Arctic Research Center
University of Alaska Fairbanks
930 North Koyukuk Drive
P. O. Box 757340
Fairbanks, AK 99775-7340
Tel 907/474-6012
Fax 907/474-5662
e-mail: sakasofu@iarc.uaf.edu

Program Management Team:
Syun Akasofu, Director, IARC, University of Alaska, Fairbanks, AK
John Calder, Director, Arctic Research, NOAA-OAR, Silver Spring, MD
Gunter Weller, Director, CIFAR, University of Alaska, Fairbanks, AK
****

APPENDIX
INSTRUCTIONS FOR PROPOSAL PREPARATION
FORMAT OF THE PROPOSAL
Proposals should be stapled in the upper left-hand corner, but
otherwise be unbound, and have 2.5-cm margins at the top, bottom, and
on each side. The type size must be clear and readily legible, in
a standard font size of 10-12 point. The original signed copy should
be clipped together (not stapled) and printed on one side of each sheet
only. The 10 additional copies of the proposal may be printed on both sides.

When submitting collaborative proposals involving more than one
institution, each institution should submit its own cover page with
appropriate signatures and its own budget. The title of the proposal,
the text, disclosures, vitae, etc., should be the same and a cover
letter should indicate that the proposal is a collaborative one
jointly submitted with another (or other) institution(s) which should
be named.

SECTIONS OF THE PROPOSAL
1. Cover page. The cover page
should include a title, the Principal Investigator's name(s) and
affiliation(s), complete address, phone, fax, e-mail information, and
budget summary broken out by year. It must be signed by an official
authorized to legally bind the submitting organization.

2. Half-page
abstract (on a separate page). This should list the nature of the proposed work (e.g., hypotheses to be tested, the relationship of the proposed studies to the research themes, the goals of any proposed workshops, relationship to the Arctic Council, etc.) and a summary of the key approach.
3. Project Description. This section should present
the problem or opportunity to be addressed by the project, and state the questions, hypotheses, and project objectives, clearly relating them to the goals of this competition. Proposals should: summarize the approach that will be used to address the questions, hypotheses, and objectives; describe how the PIs and co-PIs would contribute to the overall study approach; describe the methods to be used; and present expected results.
4. Data Plan. The proposal should include a plan on
how the data generated by the proposed research will be made available to other scientists (e.g., web pages) and deposited in a recognized data archive.
5. References cited.
6. Milestone chart for the project.
7. Statement of the project responsibilities of each Principal Investigator and participant.
8. Budget. Pattern your budget after NSF
budget Form 1030. Budget categories include the following: salaries and wages, fringe benefits, equipment, travel, materials and supplies (expendable), publication costs, consultant services, computer services, sub-awards, tuition, other expenditures, and indirect costs (facilities and administration). The full cost of logistics should be included in the budget. Travel to an annual PI meeting in Fairbanks should be included. Travel expenses need to be broken down by airfare and per diem. Salaries for Government PIs will not be supported.
9. Biographical Sketch. This is limited to two pages for each Principal Investigator and should be focused on information directly relevant to undertaking the proposed research.
10. A short list of possible peer
reviewers with whom you have no close working or personal relationship (optional).
11. Federal employees. Proposals are welcome from those
Federal agencies whose legislated mission allows participation.
NONDISCRIMINATION The National Oceanic and Atmospheric Administration provides awards for research in the sciences. The awardee is wholly responsible for the conduct of such research and preparation of the results for publication. NOAA, therefore, does not assume responsibility for such findings or their interpretation. IARC and CIFAR welcome proposals on behalf of all qualified scientists and engineers, and strongly encourage women, minorities, and persons with disabilities to compete fully in any of the research and research-related programs described in this document. In accordance with Federal statutes and regulations, and NOAA policies, no person on the grounds of race, color, age, sex, national origin, or disability shall be excluded from participation in, denied the benefits of, or be subjected to discrimination under any program or activity receiving financial assistance from NOAA.

Lab. of Dendrochronology

Institute of Plant and Animal Ecology
8 Marta St.,
202 Ekaterinburg, 620144, Russia
e-mail: stepan@ipae.uran.ru
Fax: +7 (3432) 29 41 61
Phone: +7 (3432) 29 40 92

132. 0936728245.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>
Subject: Re: Proposal to IARC
Date: Tue Sep 7 14:17:25 1999

Stepan
I have just returned from a week at a PAGES meeting in Switzerland. I presented the Yamal and Taimyr chronologies along with our recent analyses of the spatial patterns of variability in the northern network and the pressure and temperature interpretation of the patterns. All of this was well received.

As for you questions, it is very short notice to consider getting a well organised proposal together. My answers to your specific questions are
1. Such work would not necessarily be contrary to our current and future plans but there is undoubtedly a potential overlap and possible problem in distinquishing tasks and outputs. The next EC proposal must be clearly separate and I would be concerned if the potential referees asked what was the clear difference.

2. I have no experience (and presumably neither has anyone else as this is a new initiative) but I think the chances would depend on the degree of synthesis involved in the work and possibly how extensive the overall scope of the work is and also maybe who the U.S. collaborator is. I think your chance would be better as part of a large project , somewhat as we envisage for the next EC application. This is my opinion only and it may , of course, be wrong.

3.I see nothing preventing an application from your laboratory alone . If you do put in an application I wolud hope it made clear our ongoing collaboration. If you went for a collaborator in the U.S. the obvious person is Gordon Jacoby. I do not know if he is already submitting but I would think so. Please let me know what you decide . I will be phoning Gordon anyway to ask him about future collaboration on the EC front. I will keep you informed on that.
very best wishes
Keith

At 05:18 PM 9/6/99 +0500, you wrote:
>Dear Keith,
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>OPPORTUNITY" from International Arctic Research Center and Cooperative
>Institute for Arctic Research University of Alaska Fairbanks. The

>general theme is Global Change Research in the Arctic (full text with
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>in your opinion would be?
>
>Best regards.
>Stepan.
>
>From: ArcticInfo
><arcticInfo@mail.arcus.org>
>To: arcticinfo@arcus.org
>Subject: IARC Announcement of Opportunity
>For more information on these research
>opportunities contact:
>Professor Syun Akasofu, Director, IARC, Phone: 907/474-6012,
>Fax: 907/474-5662, or E-mail: sakasofu@iarc.uaf.edu.
>
>RESEARCH OPPORTUNITIES
>JOINT ANNOUNCEMENT OF OPPORTUNITY
>International Arctic Research Center and Cooperative Institute for
>Arctic Research University of Alaska Fairbanks
>Global Change Research in the Arctic
>
>INTRODUCTION
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>effects in the Arctic (detection; interactions and feedbacks;
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>The International Arctic Research Center (IARC) and the Cooperative
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>IARC is a new international research center at the University of
>Alaska Fairbanks, established jointly with Japan. The mission of the
>IARC is to provide an environment that will nurture multidisciplinary
>research by integrating and synthesizing past, present, and future
>studies in global change.
>CIFAR is the NOAA-UAF Cooperative Institute
>for Arctic Research; it is combining the resources of its Arctic
>Research Initiative (ARI) with those of IARC under this announcement.
>The goal is to develop a focal point for a pan-Arctic synthesis of
>global change in which researchers from many different institutions
>throughout the United States and the rest of the world participate to
>combine their research results. Further details on IARC can be found
>on its web page at http://www.iarc.uaf.edu/ and on CIFAR at

>http://www.cifar.uaf.edu/.
>
>Proposals may be submitted from U.S. or
>foreign institutions that address studies on any of the following
>themes drawn from the IARC Science Plan and the CIFAR Arctic Research
>Initiative. Proposals from foreign institutions should preferably have
>a U.S. partner. The starting date for proposed work should be 1
>January 2000, with a duration of up to 24 months. Funding for the
>second year will be contingent on the availability of additional
>funds, therefore each proposal should have a clear, achievable
>objective for the first year's work.
>RESEARCH THEMES
>1. Detection of
>contemporary climate change in the Arctic by ground observations,
>remote sensing and climate "fingerprinting".
>2. Arctic paleoclimatic
>reconstructions from ice cores, tree rings, permafrost, lake and ocean
>sediments.
>3. Atmosphere-ice-land-ocean interactions and feedbacks in
>the Arctic that affect change, including observations and modeling.
>4. Arctic atmospheric chemistry, arctic haze, ozone and UV radiation and
>their effects.
>5. Impacts and consequences of global climate change,
>including effects on biota and ecosystems in the Arctic.
>6. Contaminant sources, transport pathways, and exposure to higher
>trophic levels and humans in the Arctic.
>
>It is planned to fund several
>large projects and a number of medium ($100K) or smaller projects.
>Proposals must include the full cost of logistics support required. A
>total of about $ 4.5M is available in year 1 for this Announcement of
>Opportunity.
>Proposals can request support for the following:
>*Research on any of the above six themes. Proposals that add value to
>ongoing research projects, or that share costs with other funded
>investigators, are encouraged.
>* Conducting workshops at the IARC to
>further define priorities or synthesize available information on any
>of the research themes listed above, or any theme from the IARC
>Science Plan.
>* Visiting scientists, for short- or longer-term visits,
>to the IARC in Fairbanks.
>* Development of generally useful curricula
>and courses in global change, or conducting global change outreach and
>educational activities.
>* U.S. participation in the work of the Arctic
>Council and its AMAP, CAFF, or PAME working groups.
>
>All proposers
>should meet the following conditions:
>* PIs must attend an annual
>synthesis meeting of all IARC/CIFAR investigators in Fairbanks at
>which research results will be presented and working groups will
>synthesize results. Proposal budgets should include travel to
>Fairbanks.
>* All activities will be required to acknowledge the
>financial support from IARC and CIFAR in reports, papers,
>dissertations, etc.
>* Progress reports are due from all funded
>projects on 1 August 2000.
>* Copies of all publications resulting from
>funded projects are to be provided to IARC/CIFAR.
>

>Proposals should not
>exceed 15 pages in text and illustrations, not counting CVs, budget
>page, and appendices. Further details on proposal preparation are
>attached below as an appendix.
>
>Review criteria for research proposals are:
>* Does the proposal address the research themes listed above?
>*Does it propose high-quality research?
>* Does it advance the NOAA mission?
>* Is the PI (or are the PIs) well qualified to do the
>research?
>* Can the research be done in a timely manner?
>* Is it likely to lead to significant results?
>* Is it likely to contribute to
>a synthesis of research results on global change?
>
>Proposals must be
>received by 1 October 1999. All proposals will be reviewed by a
>scientific peer review panel of prominent researchers that will advise
>a program management team drawn from NOAA, IARC, and CIFAR. Funds will
>be available in early 2000. Please submit proposals (originals and 10
>copies) to the address below. Further information can also be obtained
>from the same office.
>Professor Syun Akasofu, Director
>International Arctic Research Center
>University of Alaska Fairbanks
>930 North Koyukuk Drive
>P. O. Box 757340
>Fairbanks, AK 99775-7340
>Tel 907/474-6012
>Fax 907/474-5662
>e-mail: sakasofu@iarc.uaf.edu
>
>Program Management Team:
>Syun Akasofu, Director, IARC, University of Alaska, Fairbanks, AK
>John Calder, Director, Arctic Research, NOAA-OAR, Silver Spring, MD
>Gunter Weller, Director, CIFAR, University of Alaska, Fairbanks, AK
>**********
>
>APPENDIX
>INSTRUCTIONS FOR PROPOSAL PREPARATION
>FORMAT OF THE PROPOSAL
>Proposals should be stapled in the upper left-hand corner, but
>otherwise be unbound, and have 2.5-cm margins at the top, bottom, and
>on each side. The type size must be clear and readily legible, in
>a standard font size of 10-12 point. The original signed copy should
>be clipped together (not stapled) and printed on one side of each sheet
>only. The 10 additional copies of the proposal may be printed on both sides.
>
>When submitting collaborative proposals involving more than one
>institution, each institution should submit its own cover page with
>appropriate signatures and its own budget. The title of the proposal,
>the text, disclosures, vitae, etc., should be the same and a cover
>letter should indicate that the proposal is a collaborative one
>jointly submitted with another (or other) institution(s) which should
>be named.
>
>SECTIONS OF THE PROPOSAL
>1. Cover page. The cover page
>should include a title, the Principal Investigator's name(s) and
>affiliation(s), complete address, phone, fax, e-mail information, and
>budget summary broken out by year. It must be signed by an official
>authorized to legally bind the submitting organization.

>2. Half-page
>abstract (on a separate page). This should list the nature of the
>proposed work (e.g., hypotheses to be tested, the relationship of the
>proposed studies to the research themes, the goals of any proposed
>workshops, relationship to the Arctic Council, etc.) and a summary of
>the key approach.
>3. Project Description. This section should present
>the problem or opportunity to be addressed by the project, and state
>the questions, hypotheses, and project objectives, clearly relating
>them to the goals of this competition. Proposals should: summarize the
>approach that will be used to address the questions, hypotheses, and
>objectives; describe how the PIs and co-PIs would contribute to the
>overall study approach; describe the methods to be used; and present
>expected results.
>4. Data Plan. The proposal should include a plan on
>how the data generated by the proposed research will be made available
>to other scientists (e.g., web pages) and deposited in a recognized
>data archive.
>5. References cited.
>6. Milestone chart for the project.
>7. Statement of the project responsibilities of each Principal
>Investigator and participant.
>8. Budget. Pattern your budget after NSF
>budget Form 1030. Budget categories include the following: salaries
>and wages, fringe benefits, equipment, travel, materials and supplies
>(expendable), publication costs, consultant services, computer
>services, sub-awards, tuition, other expenditures, and indirect costs
>(facilities and administration). The full cost of logistics should be
>included in the budget. Travel to an annual PI meeting in Fairbanks
>should be included. Travel expenses need to be broken down by airfare
>and per diem. Salaries for Government PIs will not be supported.
>9. Biographical Sketch. This is limited to two pages for each Principal
>Investigator and should be focused on information directly relevant to
>undertaking the proposed research.
>10. A short list of possible peer
>reviewers with whom you have no close working or personal relationship
>(optional).
>11. Federal employees. Proposals are welcome from those
>Federal agencies whose legislated mission allows participation.
>NONDISCRIMINATION The National Oceanic and Atmospheric Administration
>provides awards for research in the sciences. The awardee is wholly
>responsible for the conduct of such research and preparation of the
>results for publication. NOAA, therefore, does not assume
>responsibility for such findings or their interpretation. IARC and
>CIFAR welcome proposals on behalf of all qualified scientists and
>engineers, and strongly encourage women, minorities, and persons with
>disabilities to compete fully in any of the research and
>research-related programs described in this document. In accordance
>with Federal statutes and regulations, and NOAA policies, no person on
>the grounds of race, color, age, sex, national origin, or disability
>shall be excluded from participation in, denied the benefits of, or be
>subjected to discrimination under any program or activity receiving
>financial assistance from NOAA.
>--
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>questions please contact the list administrator: dan@arcus.org ARCUS
>600 University Avenue, Suite 1 Fairbanks, AK 99709 907/474-1600
>907/474-1604 fax
>
>Lab. of Dendrochronology
>
>Institute of Plant and Animal Ecology
>8 Marta St.,
>202 Ekaterinburg, 620144, Russia
>e-mail: stepan@ipae.uran.ru
>Fax: +7 (3432) 29 41 61
>Phone: +7 (3432) 29 40 92
>
>
>
>

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##########

From: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>
To: k.briffa@uea.ac.uk
Subject: Proposal to IARC
Date: Wed, 8 Sep 1999 16:44:52 +0500
Reply-to: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>

Dear Keith,

Thank you for answers to my questions. We decided do not participate
in this project, as many problems are originated. And there is no time
to write such proposal.

Last week I came back from the Polar Urals. The fieldwork was
successful this summer. We remeasured all trees and seedlings along
the transect, mapped forest-tundra ecosystems and tree-line over a
large territory, made about 100 photos. I found very old living twigs
of Juniperus sibirica (up to 700-800 years)and took samples from many
dead twigs. We also collected many wood samples from living and dead
larches of various ages. But we were bited by many thousands of mosquitos
especially small ones.

Sincerely yours,

Dr. Stepan G. Shiyatov

Lab. of Dendrochronology
Institute of Plant and Animal Ecology
8 Marta St., 202
Ekaterinburg, 620144, Russia
e-mail: stepan@ipae.uran.ru
Fax: +7 (3432) 29 41 61
Phone: +7 (3432) 29 40 92

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##########

From: Trevor Davies <t.d.davies@uea.ac.uk>
To: c.flack@uea,c.bentham@uea,p.jones@uea,j.palutikof@uea,p.liss@uea,
m.hulme@uea,r.k.turner@uea,a.watkinson@uea,k.brown@uea,j.darch@uea, parryml@aol.com
Subject: Discussion document for Tues/Wed
Date: Sun, 12 Sep 1999 12:21:08 +0100

Attached is a discussion document. It incorporates material provided by Simon Shackley (UMIST) & Mike Hulme. Jean has commented on it. It is intended to circulate this to consortium partners on Monday. if you have chance to read it & comment on it before it goes, that would be good; but I recognise that - in practice - time is too short. My apologies for that. (However, I do think there is a danger in presenting our partners with too 'final' a draft application at this stage. And we do need their bright ideas!).

CHRIS - please will you liaise with Jean to:

1. Get this document out to outside attendees.
2. Send out a list of attendees
3. Give outside people details of where to get the Research Councils' document 'Information for applicants to run the Centre' (web), if they don't already have it.
4. Send out an agenda (Jean is doing this)
5. Send out Kerry's diagram (Jean has)

CHRIS - will you also please fax copies of the ICER document (in your tray) to John Shepherd (Southampton 596258) and Nigel Arnell (I don't have fax number). [For info to others - we didn't send Soton a copy of the ICER bid earlier, because they were sitting on the fence].

Very many thanks.

Trevor
Attachment Converted: "c:\eudora\attach\Climate Change Centre.doc"

++++++++++++++++++++++++++
Professor Trevor D. Davies
Dean, School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
United Kingdom

Tel. +44 1603 592836
Fax. +44 1603 507719
++++++++++++++++++++++++++From ???@??? Fri Sep 24 13:44:11 1999
Received: from [139.222.104.46] (helo=taff.cru.uea.ac.uk)
        by mailserver1.uea.ac.uk with smtp (Exim 3.02 #1)
        id 11UUP8-0001QM-00; Fri, 24 Sep 1999 13:24:46 +0100
Message-Id: <3.0.3.32.19990924132145.00a5ea6c@pop.uea.ac.uk>
X-Sender: e022@pop.uea.ac.uk
X-Mailer: QUALCOMM Windows Eudora Light Version 3.0.3 (32)
Date: Fri, 24 Sep 1999 13:21:45 +0100
To: n.adger@uea,j.alexander@uea,g.bigg@uea,k.briffa@uea,p.brimblecombe@uea,
 s.dorling@uea,k.heywood@uea,t.jickells@uea,m.kelly@uea,b.maher@uea,
 j.plane@uea,a.jordan@uea,m.penkett@uea,s.raper@uea,c.vincent@uea,
 a.j.watson@uea
From: Trevor Davies <t.d.davies@uea.ac.uk>
Subject: Outline bid for new Climate Change Centre (CCC)
Cc: c.bentham@uea,p.jones@uea,j.palutikof@uea,p.liss@uea,m.hulme@uea,

r.k.turner@uea,a.watkinson@uea,k.brown@uea,j.darch@uea,parryml@aol.com
Mime-Version: 1.0
Content-Type: text/plain; charset="us-ascii"
Status:

CONFIDENTIAL TO ENV - State of Play

The research councils want a 5000 word outline bid by mid-October. The councils are putting up 2 million pounds per year for 5 years are NERC, EPSRC and ESRC. The research councils are putting the emphasis on "solutions" to climate change. They are clearly not looking for another version of CRU, the Hadley Centre, or any other existing centre in the UK. The focus is "downstream" of these existing centres.

Much of what they appear to want we anticipated in our JIF ICER (Institute for Connective Environmental Sciences) bid and, indeed, we made a provisional early strike for the CCC in that bid, although the research councils' intentions were not known at that point. Even if the JIF ICER bid is unsuccessful (& at this stage we are still optimistic), then we will still be able to take advantage of this "early" thinking in the final CCC bid.

We are aware of 3-4 competitors, which are mainly consortia of some form. Our consortium includes UMIST (number of departments), Southampton (number of departments), Cambridge (Dept of Econometrics), Sussex (Science Policy Research Unit), Cranfield (Ecotechnology Unit- Complex Systems Modelling), and Leeds (Institute for Transport Studies). There will also be a number of institutes associated with us, including Inst. Hydrology, BAS, Inst. Terrestrial Ecology, Rutherford Appleton Laboratory, Building Research Establishment, John Innes Centre, and possibly other Institutes such as the Plymouth Marine Lab & the Proudman Oceanographic Lab. The hub of this consortium will be UEA. Visiting fellows etc will work in the Centre (& possibly also at 'secondary' centres like UMIST).

Business/industry links are important, as are links with relevant institutes abroad. We anticipate writing in some industrial/business partners.

Our philosophy is not to seek to maximise the input of resources to UEA, or to the consortium, in the short term, but to build a Centre which has the credibility and the authority to identify, initiate, orchestrate research programmes, and to include the best people available. We see this as the likeliest way to attract long-term funding & to ensure the long-term future of the CCC.

We have a fairly clear idea of the "science framework" of the CCC and, together with our partners, are now agreeing the "research challenges". At the moment the research challenges look something like this:

1. DEVELOPING THE TOOLKIT

Given that the Centre's starting point is to take advantage of the best research internationally (extant, on-going, and planned), it will be necessary to apply, refine, and develop methods of 'integration'. Much science and engineering research is focused on specific disciplinary issues. This has to be brought together with critical analyses of social and economic factors, to design more adaptive and effective policies, and more effective and appropriate engineering/technology. The best aspects of 'integrated assessments' will be applied with a UK focus. An important part of such assessments will be isolate emerging opportunities (for business/industry) afforded by climate change - in order to identify competitive opportunities it will be necessary to consider global pressure points. Existing models need to be linked. Reduced complexity modelling has a significant role.

The Toolkit can also be developed and tested via geographically-focused studies. For example, integrated coastal (incl. estuaries) management which will include: risk analysis; valuation of coastal environments; effects of adaptation (soft/hard engineering solutions) on coastline; ecological/economic models; etc.

Methods to characterise/measure vulnerability and adaptive capacity.

The Toolkit will also include some of the consultation/inclusion techniques outlined in UEA's JIF bid for ICER.

2. ABRUPT CHANGES AND EXTREMES

'Climate' research on abrupt/non-linear changes (in 'underlying' climate) and on changes in extreme event frequency (some of the Tools will need to be applied - or adapted for - this Challenge: for example, vulnerability/adaptation, risk analysis, reduced complexity modelling). Of particular importance is how the possibility of abupt/non-linear change should be assimilated into decision-making frameworks (perception/risk analyses, etc.).

It will be necessary to consider the implications of non-climate 'shocks' - political and economic shocks; or combinations, for example, climate/weather extremes influencing perceptions (amongst business community and politicians) leading to sudden shifts of policy, investments, etc.

3 CARBON MANAGEMENT AND TECHNOLOGY

Adoption of clean technology (includes 'alternative' energy sources, and removal of C from emissions). In particular, clean technologies and solutions for developing countries link into identifying business opportunities. The impacts of clean technologies - landscape/lifestyle valuation. Incorporation (technological) into existing infrastructure/supply networks.

a. Carbon 'sequestration' - options, waste C recycling, use in building materials, long-term storage, etc. Oceans. Ambitious bio-engineering? (discussions with Norwich's John Innes Centre on-going).

b. Energy efficiency (technological), including control systems, especially when concentrated on one of the scale 'foci' (e.g. the household).

4. MANAGEMENT OF SOCIAL AND TECHNOLOGICAL CHANGE.

Factor 4 and beyond 1. We will need to go well beyond Factor 4 to stabilise the climate system. This programme would analyse and assess different emission trajectories, and look at how we would in practice achieve Factor 4+. It would include assessment of tools such as: C trading, domestic tradeable carbon quotas, regulation and taxation, voluntary agreements, opportunities for win-win scenarios through resource use minimisation, etc. Also, it would look at changes to a low-C economy at different scales: households, SMEs, large firms, MNCs; local to regional to national to global, etc., to sectoral: transport, energy supply, heavy & light manufacturing, services & finance, etc. Techhnology uptake. This includes reducing transport emissions and exploring low-consumption (water, energy, carbon) households. What about air traffic?

The research challenges above are not intended to be all-inclusive. We intend to use Research Challenges such as these 4, as "examplars" of the sort of thingw we will expand upon in the final bid.

The research councils have emphasised the importance of attracting a top-rate international scientist as Research Director. They also wish us to name the Executive Director at this point. We believe it should be someone with a reputation in climate research in their own right, good links etc with the "impacts" people and with funders, as well as being a good manager/organiser. We anticipate naming Mike Hulme. From what we have heard, that will give us an additional advantage over other bids.

At this point, we will welcome your comment, input, suggestions.

Trevor

++++++++++++++++++++++++++
Professor Trevor D. Davies
Dean, School of Environmental Sciences
University of East Anglia
Norwich NR4 7TJ
United Kingdom

Tel. +44 1603 592836
Fax. +44 1603 507719
++++++++++++++++++++++++++

135. 0937952556.txt
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##########

From: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>
To: k.briffa@uea.ac.uk
Subject: Additional material for final report and proposal
Date: Tue, 21 Sep 1999 18:22:36 +0500
Reply-to: "Stepan G. Shiyatov" <stepan@ipae.uran.ru>

Dear Keith,
I am sending you an additional material which can be useful for writing the final report and the next proposal.

Sincerely yours,

Dr. Stepan G. Shiyatov

Lab. of Dendrochronology
Institute of Plant and Animal Ecology
8 Marta St., 202
Ekaterinburg, 620144, Russia
e-mail: stepan@ipae.uran.ru
Fax: +7 (3432) 29 41 61
Phone: +7 (3432) 29 40 92
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##
##########

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, "Folland, Chris" <ckfolland@meto.gov.uk>, 'Phil Jones' <p.jones@uea.ac.uk>
Subject: RE: IPCC revisions
Date: Wed, 22 Sep 1999 12:35:24 -0400
Cc: tkarl@ncdc.noaa.gov, mann@virginia.edu

Thanks for your response Keith,

For all:

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

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

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

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

The recent Crowley and Lowery multiproxy estimate is an important additional piece of information which I have indeed incorporated into the

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

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

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

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

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

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

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

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

Looking forward to hearing back w/ comments,

mike

At 04:19 PM 9/22/99 +0100, Keith Briffa wrote:
>
>Hi everyone
>   Let me say that I don't mind what you put in the policy makers
>summary if there is a general concensus. However some general discussion
>would be valuable . First , like Phil , I think that the supposed
>separation of the tree-ring reconstruction from the others on the grounds
>that it is not a true "multi-proxy" series is hard to justify. What is true
>is that these particular tree-ring data best represent SUMMER temperatures
>mostly at the northern boreal forest regions. By virtue of this , they also
>definately share significant variance with Northern Hemisphere land and
>land and marine ANNUAL temperatures - but at decadal and multidecadal
>timescales - simply by virtue of the fact that these series correlated with
>the former at these timescales. The multi proxy series (Mann et al . Jones

>et al) supposedly represent annual and summer seasons respectively, and
>both contain large proportions of tree-ring input. The latest tree-ring
>density curve (i.e. our data that have been processed to retain low
>frequency information) shows more similarity to the other two series- as do
>a number of other lower resolution data (Bradley et al, Peck et al ., and
>new Crowley series - see our recent Science piece) whether this represents
>'TRUTH' however is a difficult problem. I know Mike thinks his series is
>the 'best' and he might be right - but he may also be too dismissive of
>other data and possibly over confident in his (or should I say his use of
>other's). After all, the early (pre-instrumental) data are much less
>reliable as indicators of global temperature than is apparent in modern
>calibrations that include them and when we don't know the precise role of
>particular proxies in the earlier portions of reconstruction it remains
>problematic to assign genuine confidence limits at multidecadal and longer
>timescales. I still contend that multiple regression against the recent
>very trendy global mean series is potentially dangerous. You could
>calibrate the proxies to any number of seasons , regardless of their true
>optimum response . Not for a moment am I saying that the tree-ring , or any
>other proxy data, are better than Mike's series - indeed I am saying that
>the various reconstructions are not independent but that they likely
>contribute more information about reality together than they do alone. I do
>believe   , that it should not be taken as read that Mike's series (or
>Jone's et al. for that matter) is THE CORRECT ONE. I prefer a Figure that
>shows a multitude of reconstructions (e.g similar to that in my Science
>piece). Incidently, arguing that any particular series is probably better
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>through high CO2 or nitrate input) . I know there is pressure to present a
>nice tidy story as regards 'apparent unprecedented warming in a thousand
>years or more in the proxy data' but in reality the situation is not quite
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>those that do (at least a significant number of tree proxies) some
>unexpected changes in response that do not match the recent warming. I do
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>   cheers to all
>   Keith
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>At 01:07 PM 9/22/99 +0100, Folland, Chris wrote:

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>>
>>Chris
>>
>>> -----Original Message-----
>>> From: Phil Jones [SMTP:p.jones@uea.ac.uk]
>>> Sent: 22 September 1999 12:58
>>> To: Michael E. Mann; k.briffa@uea.ac.uk
>>> Cc: ckfolland@meto.gov.uk; tkarl@ncdc.noaa.gov
>>> Subject: Re: IPCC revisions
>>>
>>>
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>>> Cheers
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>>>
>>>
>>> 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
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>>>
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>>
>--
>Dr. Keith Briffa, Climatic Research Unit, University of East Anglia,
>Norwich, NR4 7TJ, United Kingdom
>Phone: +44-1603-592090   Fax: +44-1603-507784
>
>
>

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

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

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From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>,k.briffa@uea.ac.uk
Subject: Re: IPCC revisions
Date: Wed, 22 Sep 1999 12:58:14 +0100
Cc: ckfolland@meto.gov.uk,tkarl@ncdc.noaa.gov

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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

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Folland, Chris" <ckfolland@meto.gov.uk>, 'Phil Jones' <p.jones@uea.ac.uk>, "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
Subject: RE: IPCC revisions
Date: Wed Sep 22 16:19:06 1999
Cc: tkarl@ncdc.noaa.gov

Hi everyone

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

There is still a potential problem with non-linear responses in the very recent period of some biological proxies (or perhaps a fertilisation through high CO2 or nitrate input) . I know there is pressure to present a nice tidy story as regards 'apparent unprecedented warming in a thousand years or more in the proxy data' but in reality the situation is not quite so simple. We don't have a lot of proxies that come right up to date and those that do (at least a significant number of tree proxies) some unexpected changes in response that do not match the recent warming. I do not think it wise that this issue be ignored in the chapter.

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

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

cheers to all

Keith

At 01:07 PM 9/22/99 +0100, Folland, Chris wrote:
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>> character to other tree-ring work as it is as 'hemispheric in scale' as
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>>
>> --
>> --
>>
>>
>

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From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Phil Jones <p.jones@uea.ac.uk>, "Folland, Chris" <ckfolland@meto.gov.uk>, Keith
Briffa <k.briffa@uea.ac.uk>, "Folland, Chris" <ckfolland@meto.gov.uk>
Subject: RE: IPCC revisions
Date: Thu, 23 Sep 1999 13:34:14 -0400
Cc: tkarl@ncdc.noaa.gov, mann@virginia.edu

Thanks for your comments Phil,

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

Some important specifics:

1) I am definitely using the version of the Briffa et al series you sent
in which Keith had restandardized to retain *more* low-frequency variability
relative to the one shown by Briffa et al (1998). So already, the
reconstruction I'm using is one-step removed from the published series
(as far as I know!) and that makes our use of even this series a bit
tenuous in my mind, but I'm happy to do it and let the reviewers tell us if
they see any problem. If I understand you correctly, there is yet a new
version of this series that is two steps removed from Briffa et al (1998)?
Frankly, at this stage I think we have to go w/ what we have (please see
Ian Macadam's plot
when it is available--I think the story it tells isn't all that bad,
actually) for the time being. Things as you say will change following
review anyways.

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

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

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

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

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

mike

At 05:20 PM 9/23/99 +0100, Phil Jones wrote:
>
> Mike,
>   Here are my thoughts on the text you sent. Keith will be sending some
> as well hopefully later today. One important aspect Keith will address is
> whether you're using the latest Briffa et al curve. We know you're not but
>the
> one with the greater low frequency and therefore much better chance of
> looking much better with the other two series, isn't yet published. We know
> it looks better in plots we have here.
>
> Specifics :
>
> p1 line 10 - say mid-19th century rather than the 20th century
>
>   lines 18-20 - seems a bit too much here with three refs on laminated
>   sediments.
>
>   line 46 Add Briffa et al (1998b) to Cook(1995).
>
> p2 line 59 - I would suggest changing 'a particularly' to 'the most' .
>
>   line 64 - I would add a reference here to the paper by Crowley and
>   Kim (1999) in GRL (July) where this aspect is also discussed.
>

> p3 line 101 - I would add Argentina as well as Chile adding a ref to
>   Villalba (1990) in QR.
>
>   line 108 change 'key' to 'vital'
>
>   line 119 'have providing' to 'provide' . There are several instances
>   where the text doesn't read that well. I suspect as there are several
>   iterations to go it is not that important yet !
>
>   The coral section is just about the right size now and is justly
>   devoid of references !
>
> p4 line 151 I would add a reference here to Morgan and van Ommen (1997)
>   'Seasonality in late-Holocene climate from ice core records',
>   The Holocene 7, 351-4. This is the Law Dome core which is the best
>   available with regards to dating in either hemisphere. It should be
>there.
>
>   As with the coral section the ice core section expresses some
>   cautionary notes with regard to dating etc which I think are justified.
>   I suspect teh contrast with the tree-ring section will draw some
>   criticism. Just a warning !
>
>   As none of the multiproxy reconstuctions use any sediment information
>   this section seems overlarge and could be reduced.
>
> p189 century-scale add in the 'y'
>
> p5 The borehole section is also a bit overlong. I don't know whether the
>   map really adds something. Not that vehement on this.
>
>   With respect to comapring high and low frequency aspects the diagram
>   comparing CET with the UK boreholes is now out. I've sent a copy to
> Chris. It is in :
>
>   Jones PD, 1999 : Classics in physical geography revisited - Manley's
>   CET series. Progress in Physical Geography 23, 425-428.
>
> line 245 the 'is' is not needed.
>
> p6 I still think that a reference to Raper et al (1996) would be good
> here. This models a glacier in northern Sweden using the northern
> Fennoscandian temperature reconstructions since AD 500. Again it shows
> how a low frequency estimate (the glacial snout position) can be compared
> with a high-frquency temperature reconstruction from trees.
>
> Raper, SCB, Briffa KR and Wigley TML, 1996: Glacial change in northern
> Sweden from AD 500: a simple geometric model of Storglaciaren. Journal
> of Glaciology 42, 341-351.
>
> line 268 IPCC(1996) earlier - is it 95 or 96
>
> p 7 line 295 I would like to add my paper in Reviews of Geophysics in 1999
>   as that also says that 1998 was likely to be the warmest year of the
>   millennium.
>
> line 334 I would like to see Bradley (1999). I must get a copy from
>   Ray in Venice.
>
> p7-9 All need a careful read through for English and the arguments.
>
>   At the bottom of p8 I think you make too much of the differences in the
> ranking of the centuries. The boreholes would agree with my series with

> the 17th being colder than the 19th, although they may not be able to
> resolve the timescales then.
>
>   Is the Crowley and Lowery (1999) the paper Tom's submitted to Ambio ?
>
>   I've not commented much on this final section as again I suspect there
> are many things you will have to justify in the next two sets of reviews.
> On the whole I think most is OK and I support the final paragraph. I
> don't believe the astronomical argument as an explaination over the
> last 1000 years but we can differ on that.
>
>   I know I would have written this final section 2.3.3 somehat differently
> with different emphases and slants but the basic final conclusion would
> have been the same.
>
> Cheers
> Phil
>
>
>
>
>Prof. Phil Jones
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From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>, "Folland, Chris" <ckfolland@meto.gov.uk>,
'Phil Jones' <p.jones@uea.ac.uk>
Subject: RE: IPCC revisions
Date: Thu, 23 Sep 1999 13:47:22 -0400
Cc: tkarl@ncdc.noaa.gov, mann@virginia.edu

Thanks alot Keith,

Your comments and suggestions sound good on all counts.

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

Figure??

Please check out the data here ASAP:

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

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

One other thing, I think you misinterpreted my statement:

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

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

At 06:29 PM 9/23/99 +0100, Keith Briffa wrote:
>
>Dear Mike (and all)
>
>Some remarks in response to your recent message
>
>I believe strongly that the strength in our discussion
>>will be the fact that certain key features of past climate estimates are
>>robust among a number of quasi-independent and truly independent estimates,
>>each
>>of which is not without its own limitations and potential biases
>
>Mike , I agree very much with the above sentiment. My concern was motivated
>by the possibility of expressing an impression of more concensus than might
>actually exist . I suppose the earlier talk implying that we should not
>'muddy the waters' by including contradictory evidence worried me . IPCC is
>supposed to represent concensus but also areas of uncertainty in the
>evidence. Of course where there are good reasons for the differences in
>series (such as different seasonal responses or geographic bias) it is
>equally important not to overstress the discrepancies or suggest
>contradiction where it does not exist.
>
>
> And I
>>certainly don't want to abuse my lead authorship by advocating my own work.
>>
>
>I sincerely hope this was not implied in anything I wrote - It was not
>intended

>
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>>We have shown that this is not the case: (see here:
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>>and specifically, the plot and discussion here:
>>http://www.ngdc.noaa.gov/paleo/ei/ei_nodendro.html
>>Ironically, you'll note that there is more low-frequency variability when
>>the tree ring data *are* used, then when only other proxy and
>>historical/instrumental data are used!
>>
>
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>This is certainly relevant and sounds really interesting. I need to look at
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>reconstructions (relative coefficient magnitudes). There is certainly some
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>role of instrumental predictors) in the various approaches.
>The key here is knowing much more about the role of specific predictors
>through time and their associated strengths and weaknesses.

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>undoubtedly must contain more information than our set . I have never
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>discussion is only about how close our and your data likely represent what
>they are calibrated against , back in time. Let's not imagine a
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>>One last point: We will (like it or not) have SUBSTANTIAL
>>opportunity/requirement to revise much of this discussion after review, so
>>we don't have to resolve everything now. Just the big picture and the
>>important details...
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>>I'm sure we can can up with an arrangement that is amenable to all, and I'm
>>looking forward to hearing back from Keith, Phil, and Chris in particular
>>about the above, so we can quickly move towards finalizing a first draft.
>>
>>
>
>Yes indeed. The reviewing will lead to much comment and likely disagreement
>by the masses. This is the way of these things. It is always a thankless
>task undertaking these drafting jobs and I think you are doing a good job.
>Tommorrow I'll send some very minor comments on typos and the like if you
>want them - or have you picked many of them up? Anyway , keep up the good
>work .
>
>   best wishes
>   Keith
>
>--
>Dr. Keith Briffa, Climatic Research Unit, University of East Anglia,
>Norwich, NR4 7TJ, United Kingdom
>Phone: +44-1603-592090   Fax: +44-1603-507784
>
>
>

_____
Professor Michael E. Mann
Department of Environmental Sciences, Clark Hall
University of Virginia
Charlottesville, VA 22903
_____
e-mail: mann@virginia.edu   Phone: (804) 924-7770   FAX: (804) 982-2137
http://www.evsc.virginia.edu/faculty/people/mann.html

141. 0938121656.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>, "Folland, Chris"
<ckfolland@meto.gov.uk>, Keith Briffa <k.briffa@uea.ac.uk>, "Folland, Chris"

<ckfolland@meto.gov.uk>
Subject: RE: IPCC revisions
Date: Thu, 23 Sep 1999 17:20:56 +0100
Cc: tkarl@ncdc.noaa.gov

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

Specifics :

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

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

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

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

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

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

line 108 change 'key' to 'vital'

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

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

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

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

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

p189 century-scale add in the 'y'

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

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

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

line 245 the 'is' is not needed.

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

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

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

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

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

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

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

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

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

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

Cheers
Phil

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

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142. 0938125745.txt
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>, "Folland, Chris" <ckfolland@meto.gov.uk>, 'Phil Jones' <p.jones@uea.ac.uk>
Subject: RE: IPCC revisions
Date: Thu Sep 23 18:29:05 1999
Cc: tkarl@ncdc.noaa.gov, mann@virginia.edu

Dear Mike (and all)

Some remarks in response to your recent message

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

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

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

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

>I am perfectly amenable to keeping Keith's series in the plot, and can ask
>Ian Macadam (Chris?) to add it to the plot he has been preparing (nobody
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>The key thing is making sure the series are vertically aligned in a reasonable
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Yes indeed. The reviewing will lead to much comment and likely disagreement by the masses. This is the way of these things. It is always a thankless task undertaking these drafting jobs and I think you are doing a good job. Tommorrow I'll send some very minor comments on typos and the like if you want them - or have you picked many of them up? Anyway , keep up the good work .

best wishes
Keith

143. 0938712073.txt
##########

From: Jim Fairchild-Parks <jparks@LTRR.ARIZONA.EDU>
To: ITRDBFOR@LISTSERV.ARIZONA.EDU
Subject: crossdating difficult tree-ring series
Date: Thu, 30 Sep 1999 13:21:13 -0700
Reply-to: grissino@VALDOSTA.EDU

Forumites,

Ouch, my hackles are rising so high, it hurts. (Just what exactly are hackles, anyway?).

Yes, computer crossdating ring series with special problems is always dangerous. But this is where good old skeleton-plot dating with intensive and thorough visual examination of the WOOD becomes the way to go.

I don't know about Thuja, but with the Juniperus species in the U.S. I've worked with, rings piching in and out can be a problem. You can lose 50-100 rings that way, sometimes. However, a different radius of the sample may possess all those absent rings. It's nice to have a cross-section of the subject tree, though I know this isn't always possible.

I don't understand physiologically what's going on with the Canadian cedars, but dendrochronologically speaking, absent rings are absent rings, no matter what the reason for the rings not forming on any given portion of the tree. I'll leave the reasons to scientists like Frank Telewski.

I do know that with some dying trees -- like the pinyons from New Mexico that died in the Great 1950s Drought -- the ring series on the outside became so suppressed that individual rings were indiscernable. Fortunately, other trees growing in more favorable spots had distinguishable -- though still suppressed -- rings. Traditional skeleton-plot croosdating -- along with its concomitant intensive visual analysis -- made it possible to sort though these problems.

I am not, however, an America-centrist skeleton-plot-dating bigot! I have a true appreciation for computer crossdating where it is appropriate and indeed necessary. I myself was recently involved dating high-elevation bristlecone pine from northern Arizona, U.S.A. The multi-millenial length of the chronology -- as well as the freedom from absent rings and the presence of frost-year marker rings -- made computer crossdating advisable. Of course every significant computer dating correlation was thoroughly checked out on the WOOD, and if the visual characteristics of the tree rings themselves did not support the computer dating, we threw out the date -- right out the window. Discarded computer dates collected on the parking lot beneath our offices and needed to be hauled off to the dump everyday.

I apologize for the aggressive (though sincere) tone of this message, but every few years I feel the need to rant and rave about the importance of WOOD and "pure" forms of crossdating.

Best Regards,

Jim Parks
Laboratory of Tree-Ring Research
jparks@ltrr.arizona.edu

144. 0939003588.txt
##

From: "Mike Hulme" <m.hulme@uea.ac.uk>
To: <t.d.davies@uea.ac.uk>, <c.bentham@uea.ac.uk>, <p.jones@uea.ac.uk>, <j.palutikof@uea.ac.uk>, <p.liss@uea.ac.uk>, <r.k.turner@uea.ac.uk>, <j.darch@uea.ac.uk>, <a.watkinson@uea.ac.uk>, <k.brown@uea.ac.uk>, <parryml@aol.com>
Subject: national climate change centre meeting - documents
Date: Sun, 3 Oct 1999 22:19:48 +0100
Cc: <m.hulme@uea.ac.uk>

Dear All,

Here are some notes and suggestions for our national climate centre meeting on Monday morning (1000hrs). A suggested agenda of the main points we need to cover is in this email. The attached document has three components (also appended as text to the email):

A suggested Outline Bid structure with some comments/questions
A draft of a possible 600-word opening statement
A draft of the six (from original four) research challenges (ca. 2,400 words)

We really need to aim to get a first full draft of the bid out to our Partners by late Wednesday this week, thus allowing 7 days for iterations.

Mike

NCCC: UEA Working Group Meeting, 4 October

Suggested Agenda

1. The research challenges (draft attached)

2. RD and Schneider (?)

3. The Assessment Panel; key issues for Schellnhuber

4. The structure of the outline proposal (see attached suggestion)

5. The name of the Centre

6. Timetable for submission (8 working days left)

****
**

Outline Proposal
Suggested Contents – cf. invitation to bid

Opening Statement (500 words)

Who are the co-applicants?
Hulme, Davies, Jones, Liss, Palutikof, Parry, Turner, Watkinson, Brown?
Allen, Arnell, Berkhout, Bristow, Cannell, Choularton, Halliday, Jenkins, Kohler, Launder, Markvard, Reynard, Shepherd, Shackley? – is this too many?

The strengths of the UEA-led Team (1000 words)
- being drafted by UMIST

Research Director 100 words

Management team, structure, strategy (500 words)
Advisory Board - Hasselmann, Rotmans, McQuaid, Mary Archer (Chair of National Energy Foundation), Basil Butler (RAE), Wigley, and named others?
Management Team, Programme Leaders,
What building do we use? – and a suggested physical presence at Southampton and UMIST

Initial research plan/agenda - the Challenges (2000 words)
0-order draft (attached)

How will we achieve - integration, collaboration, exploit results, attract funding? (500 words) (this might be folded into the discussion of the strengths of the UEA Team)

integrated research
formal or informal integration; IAMs are one way, but I'm not so keen on them. Some research themes may develop their own limited IAMs, e.g. optimal policy. Overall informal integration may be achieved through a common scenario approach/framework
collaboration in UK and abroad
establish MoUs with parallel centres abroad – RIVM (Neth.), PIK (Germany), ICIS (Neth.), MIT (US), Batelle (US), TERI (India), CICERO (Norway), etc. …….. Host an international conference early on to 'position' the UK NCCC in the wider field.
relevant and strategic research results and knowledge-transfer
establish regular policy briefings, both written and verbal, targeted at the business community; CBI link; UKCIP. Have a strong media presence, with a p-t communications person.
attract additional funding
may be not so easy, cf. UKCIP on impacts research have only been able to mobilise small amounts of money. Need some big corporate sponsors – what do we say about this in the outline bid?. Appoint a p-t 'marketing' person (maybe the other half of communications).

Training strategy (250 words)
Ring-fence money for training/workshops/fellowships - how much?

Training not just for researchers, but also for managers in public/private sectors. These could be 1-day sessions, as well as longer 1-week courses (cf. the Harvard course), and also longer-term secondments.

Should also maximise our links with the B.Council and DfID to bring international scientists and policy-advisors into the loop. These people

can act to facilitate the two-way flow and testing of ideas between UK and developing countries. Some of our research themes would have global dimensions – optimal policy, C sequestration, ……

UNESCO Southampton

Financial plan - salaries, equipment, sub-contracted research, collaboration expenses
- estimates from Trevor

Operations timetable - phases, etc.
- what ideas do we have for this?

Other contacts
institutions involved, but outside the bid
BRE, BAS, NRI, POL, LSHTM, AEA, Hadley Centre, UKCIP, etc.

other academic/user bodies who are relevant
RIVM, ICIS, TERI, RDBs, BP, Fuji, PowerGen, BP Solarex, ETSU/DTI photovoltaic test facility, Severn-Trent,

Appendix

1 page CVs for co-applicants
signed statement from institution(s)

[extraction of purpose from the RC's document …….. the integration of scientific research that will shape and underpin sustainable solutions to the climate change challenge].

Possible Opening Statement

The prospect of human-induced global climate change initially emerged as a research challenge for the natural sciences. Since the causes of climate change are profoundly rooted in society and the consequences of climate change for society can only be understood through social and cultural insight, the social sciences have become increasingly engaged in the research effort. With attention now turning to 'solutions' to climate change, new climate change management strategies need identifying and promoting, need to be targeted at both mitigation and adaptation objectives, and need to embrace a full array of emerging policy instruments and engineering technologies. The participation of the engineering and technological sciences, alongside the environmental and social sciences, has therefore become critical to meet this rapidly evolving research agenda.

But climate change is not just intellectually embracing challenge. It is also an experiential one. Climate change is unique in that it poses questions on space and time scales over which individual humans (especially space) and governments (especially time) are not used to thinking or do not find it easy to think. In this sense climate change is a problem of ultimate penetration and of ultimate connectivity; penetration, because we will all experience and react to climate change in some way, and connectivity, because emissions are driven by a global economy, because the response of the physical system is planetary, and because these social and natural systems are intimately co-evolving.

The intellectual and experiential challenges of climate change create a new

and distinctive lens through which we can envision the future. These insights into the future - often termed scenarios - suggest to us various tools and instruments that may allow us to fashion and shape the future. This sets us out on a course of climate change management, an active and considered pursual of desirable long-term objectives. Establishing such objectives is essential in order to adequately define the 'problem' of climate change, and even more essential if 'solutions' to this problem are going to be designed. The prospect of climate change, at the very least therefore, forces us to think about what sort of future we regard as desirable.

The UEA-led Consortium sees the new national climate change centre as an exciting opportunity to build connected research structures and outputs that exploit partnerships between science and business, between the household and government, and between the UK and emerging parallel initiatives around the world. With a strong foundation of inter-disciplinary research, and through the engaging of both public and private organisations and of both governments and individuals, there is a real prospect that we can implement emerging 'solutions' to climate change and create new ones. These 'solutions' need to engage with both mitigation and adaptation objectives and, most importantly, need to recognise and function on a hierarchy of scales ranging from the household to the global. The UK climate change centre will be built around three key principles:

The deployment of practised, inter-disciplinary research teams, who have already pioneered new insights and approaches into the questions raised by climate change, but releasing them to explore novel approaches for thinking laterally across natural, social and engineering sciences.

The practising of an inclusionary process of research in which we explore - with their developers - ways of mobilising many of the new technologies, lifestyles, regulatory mechanisms that are emerging from our technological, social and political cultures to allow us to manage climate change in the twenty-first century.

The establishment of a focal point in the UK and abroad for the open and constructive exchange of insights concerning climate change solutions across cultural divides - public-private, households-corporations, North-South.

These three key characteristics - a research programme, an engagement with stakeholders, and an educational/opinion-shaping role - are the three central elements of the new centre as proposed by the UEA-led Consortium. [Given the essential need for integration in all three of these elements, we propose the centre by called the "UK Centre for Integrated Climate Change Studies" (UK CICCS)]. The rest of this outline proposal will demonstrate, in an indicative rather than an exhaustive way, how we would operationalise these principles in terms of both management and research ideas. [refer to our conceptual schematic here or later?]

Proposed Challenges to be included in the Outline Bid

Draft, Mike Hulme, 2 October

[It may be worth including some examples of key stakeholder/client interests under each of these. These six research challenges are

exemplars, for the outline proposal, of the thinking behind our bid. Each of them may potentially involve all of the Centre's Partners - and numerous organisations beyond - and each of them are therefore integrating activities. Each of these Challenges, if developed into Research Programmes, would have a Programme Leader, appointed from within the Consortium, and accountable to the Centre's Management Team. Each of the Challenges should be able to be contextualised by our (revised) conceptual schematic of the process of integration - if we are still going to show this.]

Challenge 1: Carbon Management

Carbon management poses two fundamental questions. Given a continuing pre-dominance of fossil carbon fuels how can we combust less (the energy efficiency question) and given that a proportion of this combusted carbon will enter the atmosphere how can we sequester larger volumes within the biosphere and oceans (the carbon sequestration question)? In thinking about improving our management of carbon, the Centre will address both these questions.

Combined heat and power plants and decentralised energy generation for energy intensive industries are areas where technology can make a considerable contribution to emissions reduction. Locations and markets where investment in these technologies is both politically and economically feasible need to be identified. For LDCs, the provisions of the Kyoto Protocol for Joint Implementation are relevant here. Supplementary engineering challenges in this area include energy storage systems, fuel cell and novel transportation technologies.

Research should also be directed to the identification of business opportunities in the mitigation of climate change. This would involve a process of identifying 'climate change markets' where UK products and technologies could be supplied. One potential growth area is that of the use of modern, cheap control technology to optimise the performance of household energy management systems. Where growth markets are identified, suitable technology and service products can be developed. Business could be approached for ideas through the DTI-funded liaison officer. This work would also inform development and aid policy within the UK government. We would also draw upon the extensive experiences of UK agencies involved in delivering 'win-win' energy and waste minimisation programmes (such as Energy Efficiency and Environmental Technology Best Practice Programmes, Ground Work Trust, Business Links, and so on). Other country experiences would also be useful input, for example the highly effective programmes of boosting company productivity by reducing greenhouse gas emissions developed in the USA.

The introduction of the climate change levy in March 2001 will be analysed by the Centre in terms of its effectiveness at delivering emission reductions and its costs/benefits to a range of units (firms, sectors, regions, nationally). In addition, the introduction of voluntary agreements for some companies in return for a reduction in the levy charged will be analysed along similar lines. The DTI-ACBE led initiative on voluntary use of tradeable emission schemes will provide important empirical evidence on the relative costs of achieving given emission reductions by a taxation scheme compared to emissions trading.

While conventional carbon sequestration technologies are not considered a long-term solution to climate change, there is nevertheless a need to research the most efficient ways of implementing such technologies and also a need to research new, longer-term sequestration technologies through bio-engineering and deep ocean sinks. The Centre will explore the

feasibility of both these latter two technologies, in collaboration with the John Innes Centre for the bio-engineering. [We may only have 30 years to get some 'emergency' carbon sequestration techniques sorted out under the scenario that we don't manage to get enough CO2 emissions reduction.] A mixture of methods and tools will be required to evaluate sequestration options - life-cycle costing and LCA, environmental impact analysis, technological assessment, public acceptance, etc. Some work on biomass sequestration may also be needed to feed into the global assessments/evaluation of this option. Given the sensitivity of this issue under the terms of the Kyoto Protocol, the UK government needs excellent advice on methods, assumptions, pitfalls, etc.

[Links outside the Centre to: JIC, many others …….

Challenge 2: The Renewables Challenge

A parallel challenge to that of carbon management is how to stimulate and release the full potential for zero- or low-carbon renewable energies? This therefore is the third strand of the strategy to meet and surpass the carbon emissions reduction obligations placed on developed nations by Kyoto. There are a number of research questions related to this Challenge that again require engagement by the engineering, environmental and social science communities within an integrated framework. Too much work to date has compartmentalised the three perspectives.

The EU has a target of 12 per cent of primary energy to be met from renewable energy by the year 2010. Meeting such a target, let alone moving beyond it, has major implications for the electricity delivery systems in the UK. How to get this much renewable energy - from intermittent sources – linked, delivered and purchased by customers? Engineers and economists need an opportunity to explore the long-term implications of such policy objectives. Related questions concern the landscape and infrastructural implications of an expanded uptake of biofuels in the UK.

Many renewable technologies appear in various EPSRC research programmes, but they need to be brought together to produce scenarios whose emissions and life-cycle costs can be assessed in a common framework, thus enabling more practical advice and comment on energy policy debates. Some of these scenarios could be taken further in the form of pilot-demonstration projects.

There needs to be mechanisms established for the better integration of architectural design with renewable energies, e.g. solar and wind. The design of these new technologies needs explicitly to consider the architectural consequences for domestic, commercial and industrial structures. Partners who are directly involved in delivering design solutions in this area will be invited by the Centre to establish 'demonstration' projects to explore how successful such solutions are in practice. [can we give some specific examples of Partners and projects here?]

One of the obstacles to the more rapid exploitation of wind energy in the UK relates to landscape value and aesthetics. This is an issue that needs the interaction of design technologists and social scientists - including psychologists - to explore cultural and behavioural limits to new renewable technology uptake. We propose that the visualisation facility of the Centre be exploited to research these issues through involving the wider community.

[Links outside the Centre to: ……

Challenge 3: Singularities, Non-Linear Changes and Extreme Events

The climate system is generally assumed to be 'well-behaved'. Certainly, much of the scenario and impacts work assessed by the IPCC (and that has therefore fed through into climate policy) has assumed conditions of relative regularity in future climate. However, not only does the climate system possess the potential for rapid, singular changes (i.e., a complex, non-linear system being rapidly forced), but recent research has shown using theoretical models and palaeo-evidence that such potential changes can be and have been realised. Elsewhere, thresholds and sensitivities of natural/social systems to changing frequencies of extreme weather events induce additional non-linearities in the environmental responses to climate change. There are also singularities and non-linear processes operating in the social/political drivers of climate change - for example, political or economic 'shocks' that may fundamentally and rapidly re-direct our technological/economic futures away from 'conventional' pathways.

A particular Challenge to be addressed by the Centre will therefore be how such potential for non-linear behaviour - in both climate and non-climate systems - can be both modelled and introduced into scenario exercises. Recent work with reduced-complexity models has shown the potential to model such non-linear behaviour in a quasi-stochastic manner and such modelling work will be developed by the Centre. A corollary of this is to better understand how such abrupt changes should be assimilated into decision-making frameworks and policy analysis. This requires the involvement of risk theoreticians and risk analytic tools. The possible interactions between these two complex non-linear systems - the climate and the social - is of particular importance. For example, an abrupt climate change or a string of short-term weather extremes can radically influence perceptions amongst the business community and politicians and lead to sudden shifts in policy, investment flows, etc. The implications of such singular behaviour for vulnerability and adaptation strategies have not been well explored. This kind of analysis would be important to many commercial sectors, which are highly concerned about the unexpected and about extremes. This is an inter-disciplinary Challenge the Centre will be uniquely well-placed to address.

[Links outside the Centre to: POL, Hadley Centre, PIK, ……..

Challenge 4: Managing the Coastal Zone

There are many geographic domains where climate change poses particular problems for the management of natural and social assets - coasts, uplands, cities, river basins, etc. We propose that the Centre should pay particular attention to one such domain, since these provide physical entities within which many of the issues of climate change vulnerability and adaptation play out in a given context of local/regional governance. We suggest that the coastal zone best epitomises this challenge of integrating our social, environmental and engineering knowledge. A unique feature of the interaction between climate change and the coastal zone is the very long time-scales over which sea-level rise impacts will materialise - of all the impacts of climate change these are least amenable to mitigation and therefore where appropriately designed adaptation strategies are most needed.

Research is first needed to improve our understanding of the threats posed

by climate change, most notably changing storm-surge frequencies along the UK coast and changes in estuarine hydrology and ecology. This will involve coupled high resolution ocean-atmosphere modelling, estuarine economic/ecological modelling, and the assimilation of such modelling results into a risk analysis framework.

Designing an array of possible management options for the coastal zone needs to involve economists, ecologists, marine scientists, and coastal engineers. A range of options from 'hard' engineering solutions to managed retreat need identifying. The desirability of any one or combination of these management options for the coastal zone can then only be evaluated following an understanding of the value of the coastal environment and the services it delivers. Such valuation needs to be a fully participatory process involving local communities, local government, landowners, NGOs, and national regulatory bodies. We propose the Centre plays an active role in bringing together insights from integrated modelling exercises and from stakeholder participatory exercises, thus enabling better public participation in the policy-forming process (see integration methodologies - Challenge 6). This role would involve novel visualisation techniques of coastal environments to exploit both modelling results and individual perceptions of coastal landscape value.

[Links outside the Centre to: EA/MAFF, NGOs/Conservation, LAs, Railtrack, construction companies, ……

Challenge 5: Beyond Factor 4

There is a growing body of opinion that in order to mitigate climate change, or even to adapt to it, significant changes in current patterns of consumption, and therefore lifestyle, are necessary. This raises the question of how to direct consumption of goods and services towards more sustainable paths. The scale of the Challenge here suggests that we need to go well beyond Factor 4 - doubling wealth, halving resource use.

One unsolved dilemma is that of expanding car use for personal transportation. The psychology, behavioural sociology and economics of people's use of cars is reasonably well understood. What needs to be researched are methods to manage the ever-increasing demand for travel, especially car and air travel, that ranges from taking the children to school, to car-based salespeople, to international business and holiday travel. Research will also be needed into managing the overturning of the vehicle stock and transport infrastructure under conditions of novel transportation technologies - infrastructural inertia is an obstacle to new technology uptake.

The concept of a low consumption household is a further desirable objective which is easy to state and not straightfoward to achieve. This way of analysing human activity is inherently interdisciplinary and looks at the activities of a household - housing, domestic appliances and services, transport needs, consumption, work and leisure time use, waste generation and recycling - in terms of the interactions between them. For example, housing choice is partly determined by the work/leisure split, which then determines the demand for transport; consumption generates waste and also contributes to energy demand. Another important example is that of home insulation. The UK has a poorly insulated housing stock and even new housing could be built to much higher standards of energy efficiency. Research, in conjunction with the construction industry, is needed into the adoption of new building standards and (politically acceptable) economic incentives for low-energy housing is needed. This is especially relevant given the current debate about the millions of new households predicted for

the UK in the next 20-30 years and the greenfield/brownfield land use argument.

Partners who are directly involved in delivering sustainable solutions will be involved in setting up 'demonstration' projects to explore how successful such methodologies are in practice. For example, the Centre will explore whether 'climate-friendly' households can be demonstrated in practice. Partners could include Going for Green, National Centre for Business and Ecology, Forum for the Future, Sustainability Northwest, United Utilities, Eastern Group, Anglian Water and other water companies, etc.

[Links outside the Centre to: …….

Challenge 6: Integration Methodologies

An important methodology which provides insights into the dynamics of climate and social change, but which has not yet been fully developed for the UK is that of integrated assessment. Integrated assessment encompasses formal modelling approaches and more participatory and qualitative explorations of the future. Integrated modelling includes both reduced-form models and complex systems models. All integrated assessment is built around the concept of scenarios, used either in the more traditional role of 'what-if' or in a 'back-casting' role. While integrated assessments of climate change have developed substantially over the last decade, few have embraced the engineering community to explore the feasibility of pathways with rapid uptake of new technologies. The Challenge for the Centre will be to develop further existing modelling and participatory approaches for integrated assessment and apply them to the five research Challenges identified above.

The integrated modelling framework that is required to address these concerns is obviously extremely difficult to imagine. Recent advances based on complex systems modelling do, however, suggest how such frameworks may be achieved (e.g. NEXSUS, ESRC Priority Network). These are constituted of a spatial hierarchy of nested models representing the possible behaviours of complex social, economic, ecological, and technological systems at different spatial and temporal scales of resolution. They can explore the possibility of emergent behaviour at larger scales, as well as the effects of micro-responses and adaptations at smaller ones. In order to address the issues raised by climate change and its associated impacts and responses, considerable development of this framework would be necessary. However, without it, there seems little prospect of providing a rational basis for the assessment of possible climate policies or actions.

The Centre will also develop parallel research into participatory approaches for the development of integrated scenarios of the future. This will include the public perception of environmental risks caused by climate change; peoples actions in response to these perceptions is also important. Identification of suitable scenarios for presentation in participative experiments on public/corporate response would involve the physical sciences in co-operation with engineers, political scientists, psychologists and economists. Methods include surveys, focus groups, citizens juries and stakeholder workshops. [CSERGE/UMIST developing these ideas; use the ICER Visualisation Laboratory]. More in-depth empirical research could be undertaken to understand better individual and organisational decision-making on climate change related issues, such as energy consumption, transport choices, and so on. This activity would have the objective of developing methodologies for assessing the public response to the particular problems identified in the Carbon Management, Renewables and Factor 4+ Challenges above. Through interactions with business it may

also open up the possibility of 'design-oriented scenarios', i.e., in which the scenario identifies a need for a new kind of product/process design in response to a prospective future socio-political change.

[Links outside the Centre to: other process modelling centres, ULYSSES, …….

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145. 0939141116.txt
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From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>, imacadam@meto.gov.uk
Subject: Re: Briffa et al. series for IPCC figure
Date: Tue, 05 Oct 1999 12:31:56 -0400
Cc: k.briffa@uea, p.jones@uea, ckfolland@meto.gov.uk, tkarl@ncdc.noaa.gov

Dear Tim,

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

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

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

mike

At 04:18 PM 10/5/99 +0100, Tim Osborn wrote:
>Dear Mike and Ian
>

>Keith has asked me to send you a timeseries for the IPCC multi-proxy
>reconstruction figure, to replace the one you currently have. The data are
>attached to this e-mail. They go from 1402 to 1995, although we usually
>stop the series in 1960 because of the recent non-temperature signal that
>is superimposed on the tree-ring data that we use. I haven't put a 40-yr
>smoothing through them - I thought it best if you were to do this to ensure
>the same filter was used for all curves.
>
>The raw data are the same as used in Briffa et al. (1998), the Nature paper
>that I think you have the reference for already. They are analysed in a
>different way, to retain the low-frequency variations. In this sense, it
>is one-step removed from Briffa et al. (1998). It is not two-steps removed
>from Briffa et al. (1998), since the new series is simply a *replacement*
>for the one that you have been using, rather than being one-step further.
>
>A new manuscript is in preparation describing this alternative analysis
>method, the calibration of the resulting series, and their comparison with
>other reconstructions. We are consdering submitting this manuscript to J.
>Geophys. Res. when it is ready, but for now it is best cited as:
>Briffa KR, Osborn TJ, Schweingruber FH, Harris IC and Jones PD (1999)
>Extracting low-frequency temperature variations from a northern tree-ring
>density network. In preparation.
>Keith will be sending you a copy of the manuscript when it is nearer to
>completion.
>
>I have also attached a PS file showing the original Briffa et al. (1998)
>curve, with annotation of cold years associated with known volcanic
>eruptions. Overlain on this, you will see a green curve. This is the new
>series with a 40-yr filter through it. This is just so that you can see
>what it should look like (***ignore the temperature scale on this
>figure***, since the baseline is non-standard).
>
>With regard to the baseline, the data I've sent are calibrated over the
>period 1881-1960 against the instrumental Apr-Sep tempratures averaged over
>all land grid boxes with observed data that are north of 20N. As such, the
>mean of our reconstruction over 1881-1960 matches the mean of the observed
>target series over the same period. Since the observed series consists of
>degrees C anomalies wrt to 1961-90, we say that the reconstructed series
>also represents degrees C anomalies wrt to 1961-90. One could, of course,
>shift the mean of our reconstruction so that it matched the observed series
>over a different period - say 1931-60 - but I don't see that this improves
>things. Indeed, if the non-temperature signal that causes the decline in
>tree-ring density begins before 1960, then a short 1931-60 period might
>yield a more biased result than using a longer 1881-1960 period.
>
>If you have any queries regarding this replacement data, then please e-mail
>me and/or Keith.
>
>Best regards
>
>Tim
>
>Calibrated against observed Apr-Sep temperature over 1881-1960
>averaged over all land grid boxes north of 20N
>
>
>Year Reconstructed temperature anomaly (degrees C wrt 1961-90)
>1402   -0.283
>1403   -0.334
>1404   -0.286
>1405   -0.350
>1406   -0.152
>1407   -0.124

>1408	-0.220
>1409	-0.175
>1410	-0.100
>1411	-0.129
>1412	-0.226
>1413	-0.115
>1414	-0.386
>1415	-0.319
>1416	-0.277
>1417	-0.136
>1418	-0.172
>1419	-0.294
>1420	-0.280
>1421	-0.335
>1422	-0.406
>1423	-0.312
>1424	-0.207
>1425	-0.136
>1426	-0.354
>1427	-0.222
>1428	-0.305
>1429	-0.322
>1430	-0.282
>1431	-0.143
>1432	-0.212
>1433	-0.234
>1434	-0.076
>1435	-0.309
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>1438	-0.272
>1439	-0.159
>1440	-0.330
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>1442	-0.105
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>1445	-0.138
>1446	-0.317
>1447	-0.270
>1448	-0.301
>1449	-0.357
>1450	-0.137
>1451	-0.183
>1452	-0.207
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>1460	-0.252
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>1472	-0.122
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>1477	-0.075
>1478	0.037
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>1481	-0.513
>1482	-0.286
>1483	-0.354
>1484	-0.327
>1485	-0.208
>1486	-0.125
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>1493	-0.278
>1494	-0.394
>1495	-0.526
>1496	-0.275
>1497	-0.264
>1498	-0.233
>1499	-0.169
>1500	-0.128
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>1509	-0.312
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>1555	-0.116
>1556	-0.141
>1557	-0.419
>1558	-0.174
>1559	-0.465
>1560	-0.287
>1561	-0.169
>1562	-0.231
>1563	-0.270
>1564	-0.347
>1565	-0.116
>1566	-0.202
>1567	-0.278
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>1569	-0.488
>1570	-0.465
>1571	-0.434
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>1573	-0.324
>1574	-0.493
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>1587	-0.580
>1588	-0.530
>1589	-0.534
>1590	-0.354
>1591	-0.377
>1592	-0.407
>1593	-0.337
>1594	-0.591
>1595	-0.459
>1596	-0.436

>1597	-0.475
>1598	-0.152
>1599	-0.134
>1600	-0.381
>1601	-1.169
>1602	-0.403
>1603	-0.414
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>1655	-0.400
>1656	-0.372
>1657	-0.332
>1658	-0.359
>1659	-0.182

>1660	-0.260
>1661	-0.258
>1662	-0.433
>1663	-0.433
>1664	-0.353
>1665	-0.440
>1666	-0.837
>1667	-0.857
>1668	-0.816
>1669	-0.779
>1670	-0.871
>1671	-0.463
>1672	-0.434
>1673	-0.631
>1674	-0.663
>1675	-0.870
>1676	-0.523
>1677	-0.670
>1678	-0.794
>1679	-0.768
>1680	-0.701
>1681	-0.380
>1682	-0.518
>1683	-0.364
>1684	-0.369
>1685	-0.688
>1686	-0.178
>1687	-0.481
>1688	-0.351
>1689	-0.229
>1690	-0.254
>1691	-0.221
>1692	-0.545
>1693	-0.263
>1694	-0.316
>1695	-0.955
>1696	-0.816
>1697	-0.687
>1698	-1.054
>1699	-1.005
>1700	-0.630
>1701	-0.818
>1702	-0.510
>1703	-0.377
>1704	-0.420
>1705	-0.527
>1706	-0.328
>1707	-0.257
>1708	-0.465
>1709	-0.493
>1710	-0.288
>1711	-0.344
>1712	-0.345
>1713	-0.242
>1714	-0.390
>1715	-0.305
>1716	-0.390
>1717	-0.309
>1718	-0.270
>1719	-0.194
>1720	-0.110
>1721	-0.427
>1722	0.005

>1723	-0.193
>1724	-0.249
>1725	-0.497
>1726	-0.381
>1727	-0.241
>1728	-0.133
>1729	-0.261
>1730	-0.633
>1731	-0.723
>1732	-0.426
>1733	-0.371
>1734	-0.104
>1735	-0.373
>1736	-0.330
>1737	-0.206
>1738	-0.557
>1739	-0.291
>1740	-0.734
>1741	-0.594
>1742	-0.808
>1743	-0.378
>1744	-0.372
>1745	-0.418
>1746	-0.501
>1747	-0.150
>1748	-0.389
>1749	-0.328
>1750	-0.168
>1751	-0.343
>1752	-0.227
>1753	-0.218
>1754	-0.377
>1755	-0.328
>1756	-0.221
>1757	-0.259
>1758	-0.431
>1759	-0.340
>1760	-0.335
>1761	-0.261
>1762	-0.466
>1763	-0.291
>1764	-0.473
>1765	-0.378
>1766	-0.212
>1767	-0.429
>1768	-0.544
>1769	-0.343
>1770	-0.341
>1771	-0.265
>1772	-0.547
>1773	-0.421
>1774	-0.048
>1775	-0.289
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>1777	-0.288
>1778	-0.178
>1779	-0.550
>1780	-0.339
>1781	-0.251
>1782	-0.164
>1783	-0.757
>1784	-0.142
>1785	-0.141

>1786	-0.179
>1787	-0.432
>1788	-0.207
>1789	-0.235
>1790	-0.612
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>1792	-0.086
>1793	-0.023
>1794	-0.030
>1795	-0.243
>1796	-0.028
>1797	-0.565
>1798	-0.049
>1799	-0.228
>1800	-0.287
>1801	-0.413
>1802	-0.117
>1803	0.020
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>1806	-0.251
>1807	-0.089
>1808	-0.241
>1809	-0.460
>1810	-0.582
>1811	-0.353
>1812	-0.459
>1813	-0.545
>1814	-0.458
>1815	-0.588
>1816	-0.855
>1817	-0.861
>1818	-0.629
>1819	-0.680
>1820	-0.289
>1821	-0.351
>1822	-0.159
>1823	-0.246
>1824	-0.276
>1825	-0.263
>1826	-0.140
>1827	-0.293
>1828	-0.033
>1829	-0.087
>1830	-0.173
>1831	-0.045
>1832	-0.621
>1833	-0.660
>1834	-0.141
>1835	-0.647
>1836	-0.775
>1837	-0.771
>1838	-0.359
>1839	-0.267
>1840	-0.144
>1841	-0.077
>1842	-0.337
>1843	-0.435
>1844	-0.101
>1845	-0.412
>1846	0.106
>1847	-0.079
>1848	-0.346

>1849	-0.393
>1850	-0.261
>1851	-0.165
>1852	-0.100
>1853	-0.174
>1854	-0.138
>1855	-0.418
>1856	-0.250
>1857	-0.538
>1858	-0.126
>1859	-0.195
>1860	-0.231
>1861	-0.029
>1862	-0.555
>1863	-0.303
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>1865	-0.256
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>1867	-0.413
>1868	-0.119
>1869	-0.321
>1870	-0.213
>1871	-0.352
>1872	-0.163
>1873	-0.183
>1874	-0.372
>1875	-0.247
>1876	-0.487
>1877	-0.192
>1878	0.120
>1879	-0.152
>1880	-0.346
>1881	-0.184
>1882	-0.200
>1883	-0.183
>1884	-0.717
>1885	-0.534
>1886	-0.485
>1887	-0.281
>1888	-0.261
>1889	-0.153
>1890	-0.341
>1891	-0.313
>1892	-0.138
>1893	-0.301
>1894	-0.134
>1895	-0.128
>1896	-0.241
>1897	-0.016
>1898	0.065
>1899	-0.574
>1900	-0.218
>1901	-0.049
>1902	-0.287
>1903	-0.142
>1904	-0.205
>1905	-0.308
>1906	-0.034
>1907	-0.412
>1908	-0.048
>1909	-0.214
>1910	-0.147
>1911	-0.194

>1912	-0.631
>1913	-0.161
>1914	-0.294
>1915	-0.074
>1916	-0.277
>1917	-0.297
>1918	-0.460
>1919	-0.013
>1920	-0.272
>1921	-0.114
>1922	-0.036
>1923	-0.305
>1924	-0.141
>1925	-0.258
>1926	-0.115
>1927	-0.198
>1928	-0.018
>1929	-0.161
>1930	0.086
>1931	0.104
>1932	0.081
>1933	-0.057
>1934	0.007
>1935	-0.037
>1936	-0.019
>1937	0.060
>1938	0.163
>1939	-0.075
>1940	0.113
>1941	-0.200
>1942	0.128
>1943	0.053
>1944	-0.080
>1945	0.059
>1946	-0.016
>1947	-0.188
>1948	-0.038
>1949	-0.107
>1950	-0.269
>1951	-0.100
>1952	-0.118
>1953	0.161
>1954	-0.235
>1955	-0.127
>1956	-0.308
>1957	-0.194
>1958	-0.308
>1959	-0.224
>1960	0.076
>1961	-0.104
>1962	-0.289
>1963	-0.173
>1964	-0.479
>1965	-0.474
>1966	-0.171
>1967	-0.200
>1968	-0.599
>1969	-0.355
>1970	-0.353
>1971	-0.328
>1972	-0.563
>1973	-0.262
>1974	-0.336

>1975 -0.507
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>1981 -0.195
>1982 -0.522
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>1984 -0.335
>1985 -0.423
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>1993 -0.341
>1994 -0.320
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>Attachment Converted: "c:\eudora\attach\Briffa et al.ps"
>
>Dr Timothy J Osborn | phone: +44 1603 592089
>Senior Research Associate | fax: +44 1603 507784
>Climatic Research Unit | e-mail: t.osborn@uea.ac.uk
>School of Environmental Sciences | web-site:
>University of East Anglia _____| http://www.cru.uea.ac.uk/~timo/
>Norwich NR4 7TJ | sunclock:
>UK | http://www.cru.uea.ac.uk/~timo/sunclock.htm
>

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

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

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

Dear Mike and Ian

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

The raw data are the same as used in Briffa et al. (1998), the Nature paper

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

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

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

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

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

Best regards

Tim

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

Year	Reconstructed temperature anomaly (degrees C wrt 1961-90)
1402	-0.283
1403	-0.334
1404	-0.286
1405	-0.350
1406	-0.152
1407	-0.124
1408	-0.220
1409	-0.175
1410	-0.100
1411	-0.129
1412	-0.226
1413	-0.115
1414	-0.386
1415	-0.319
1416	-0.277

1417	-0.136
1418	-0.172
1419	-0.294
1420	-0.280
1421	-0.335
1422	-0.406
1423	-0.312
1424	-0.207
1425	-0.136
1426	-0.354
1427	-0.222
1428	-0.305
1429	-0.322
1430	-0.282
1431	-0.143
1432	-0.212
1433	-0.234
1434	-0.076
1435	-0.309
1436	-0.411
1437	-0.122
1438	-0.272
1439	-0.159
1440	-0.330
1441	-0.160
1442	-0.105
1443	-0.080
1444	-0.308
1445	-0.138
1446	-0.317
1447	-0.270
1448	-0.301
1449	-0.357
1450	-0.137
1451	-0.183
1452	-0.207
1453	-0.485
1454	-0.265
1455	-0.358
1456	-0.241
1457	-0.199
1458	-0.366
1459	-0.397
1460	-0.252
1461	-0.230
1462	-0.252
1463	-0.209
1464	-0.174
1465	-0.174
1466	-0.280
1467	-0.256
1468	-0.256
1469	-0.222
1470	-0.237
1471	-0.094
1472	-0.122
1473	-0.056
1474	-0.320
1475	-0.376
1476	-0.133
1477	-0.075
1478	0.037
1479	-0.161

1480	-0.379
1481	-0.513
1482	-0.286
1483	-0.354
1484	-0.327
1485	-0.208
1486	-0.125
1487	-0.380
1488	-0.193
1489	-0.245
1490	-0.466
1491	-0.244
1492	-0.146
1493	-0.278
1494	-0.394
1495	-0.526
1496	-0.275
1497	-0.264
1498	-0.233
1499	-0.169
1500	-0.128
1501	-0.415
1502	-0.306
1503	0.011
1504	-0.013
1505	-0.378
1506	-0.226
1507	-0.428
1508	-0.192
1509	-0.312
1510	-0.157
1511	-0.162
1512	-0.188
1513	-0.135
1514	-0.418
1515	-0.258
1516	-0.381
1517	-0.134
1518	-0.180
1519	-0.166
1520	-0.035
1521	-0.384
1522	-0.302
1523	-0.541
1524	-0.371
1525	-0.183
1526	-0.289
1527	-0.224
1528	-0.247
1529	-0.432
1530	-0.291
1531	-0.467
1532	-0.343
1533	-0.586
1534	-0.183
1535	-0.417
1536	-0.350
1537	-0.257
1538	-0.451
1539	-0.398
1540	-0.497
1541	-0.406
1542	-0.584

1543	-0.448
1544	-0.317
1545	-0.312
1546	-0.289
1547	-0.114
1548	-0.459
1549	-0.335
1550	-0.009
1551	-0.074
1552	-0.047
1553	-0.207
1554	-0.285
1555	-0.116
1556	-0.141
1557	-0.419
1558	-0.174
1559	-0.465
1560	-0.287
1561	-0.169
1562	-0.231
1563	-0.270
1564	-0.347
1565	-0.116
1566	-0.202
1567	-0.278
1568	-0.445
1569	-0.488
1570	-0.465
1571	-0.434
1572	-0.674
1573	-0.324
1574	-0.493
1575	-0.273
1576	-0.623
1577	-0.483
1578	-0.521
1579	-0.551
1580	-0.473
1581	-0.436
1582	-0.382
1583	-0.345
1584	-0.280
1585	-0.565
1586	-0.409
1587	-0.580
1588	-0.530
1589	-0.534
1590	-0.354
1591	-0.377
1592	-0.407
1593	-0.337
1594	-0.591
1595	-0.459
1596	-0.436
1597	-0.475
1598	-0.152
1599	-0.134
1600	-0.381
1601	-1.169
1602	-0.403
1603	-0.414
1604	-0.472
1605	-0.393

1606	-0.564
1607	-0.529
1608	-0.822
1609	-0.789
1610	-0.617
1611	-0.681
1612	-0.670
1613	-0.364
1614	-0.733
1615	-0.428
1616	-0.698
1617	-0.479
1618	-0.485
1619	-0.524
1620	-0.706
1621	-0.671
1622	-0.714
1623	-0.662
1624	-0.387
1625	-0.566
1626	-0.671
1627	-0.665
1628	-0.759
1629	-0.654
1630	-0.379
1631	-0.466
1632	-0.330
1633	-0.377
1634	-0.521
1635	-0.222
1636	-0.265
1637	-0.252
1638	-0.396
1639	-0.382
1640	-0.400
1641	-1.152
1642	-1.067
1643	-1.092
1644	-0.649
1645	-0.588
1646	-0.632
1647	-0.554
1648	-0.368
1649	-0.572
1650	-0.215
1651	-0.317
1652	-0.529
1653	-0.268
1654	-0.343
1655	-0.400
1656	-0.372
1657	-0.332
1658	-0.359
1659	-0.182
1660	-0.260
1661	-0.258
1662	-0.433
1663	-0.433
1664	-0.353
1665	-0.440
1666	-0.837
1667	-0.857
1668	-0.816

1669	-0.779
1670	-0.871
1671	-0.463
1672	-0.434
1673	-0.631
1674	-0.663
1675	-0.870
1676	-0.523
1677	-0.670
1678	-0.794
1679	-0.768
1680	-0.701
1681	-0.380
1682	-0.518
1683	-0.364
1684	-0.369
1685	-0.688
1686	-0.178
1687	-0.481
1688	-0.351
1689	-0.229
1690	-0.254
1691	-0.221
1692	-0.545
1693	-0.263
1694	-0.316
1695	-0.955
1696	-0.816
1697	-0.687
1698	-1.054
1699	-1.005
1700	-0.630
1701	-0.818
1702	-0.510
1703	-0.377
1704	-0.420
1705	-0.527
1706	-0.328
1707	-0.257
1708	-0.465
1709	-0.493
1710	-0.288
1711	-0.344
1712	-0.345
1713	-0.242
1714	-0.390
1715	-0.305
1716	-0.390
1717	-0.309
1718	-0.270
1719	-0.194
1720	-0.110
1721	-0.427
1722	0.005
1723	-0.193
1724	-0.249
1725	-0.497
1726	-0.381
1727	-0.241
1728	-0.133
1729	-0.261
1730	-0.633
1731	-0.723

1732	-0.426
1733	-0.371
1734	-0.104
1735	-0.373
1736	-0.330
1737	-0.206
1738	-0.557
1739	-0.291
1740	-0.734
1741	-0.594
1742	-0.808
1743	-0.378
1744	-0.372
1745	-0.418
1746	-0.501
1747	-0.150
1748	-0.389
1749	-0.328
1750	-0.168
1751	-0.343
1752	-0.227
1753	-0.218
1754	-0.377
1755	-0.328
1756	-0.221
1757	-0.259
1758	-0.431
1759	-0.340
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1761	-0.261
1762	-0.466
1763	-0.291
1764	-0.473
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1766	-0.212
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1768	-0.544
1769	-0.343
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1771	-0.265
1772	-0.547
1773	-0.421
1774	-0.048
1775	-0.289
1776	-0.186
1777	-0.288
1778	-0.178
1779	-0.550
1780	-0.339
1781	-0.251
1782	-0.164
1783	-0.757
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1785	-0.141
1786	-0.179
1787	-0.432
1788	-0.207
1789	-0.235
1790	-0.612
1791	-0.163
1792	-0.086
1793	-0.023
1794	-0.030

1795	-0.243
1796	-0.028
1797	-0.565
1798	-0.049
1799	-0.228
1800	-0.287
1801	-0.413
1802	-0.117
1803	0.020
1804	0.036
1805	-0.094
1806	-0.251
1807	-0.089
1808	-0.241
1809	-0.460
1810	-0.582
1811	-0.353
1812	-0.459
1813	-0.545
1814	-0.458
1815	-0.588
1816	-0.855
1817	-0.861
1818	-0.629
1819	-0.680
1820	-0.289
1821	-0.351
1822	-0.159
1823	-0.246
1824	-0.276
1825	-0.263
1826	-0.140
1827	-0.293
1828	-0.033
1829	-0.087
1830	-0.173
1831	-0.045
1832	-0.621
1833	-0.660
1834	-0.141
1835	-0.647
1836	-0.775
1837	-0.771
1838	-0.359
1839	-0.267
1840	-0.144
1841	-0.077
1842	-0.337
1843	-0.435
1844	-0.101
1845	-0.412
1846	0.106
1847	-0.079
1848	-0.346
1849	-0.393
1850	-0.261
1851	-0.165
1852	-0.100
1853	-0.174
1854	-0.138
1855	-0.418
1856	-0.250
1857	-0.538

1858	-0.126
1859	-0.195
1860	-0.231
1861	-0.029
1862	-0.555
1863	-0.303
1864	-0.407
1865	-0.256
1866	-0.437
1867	-0.413
1868	-0.119
1869	-0.321
1870	-0.213
1871	-0.352
1872	-0.163
1873	-0.183
1874	-0.372
1875	-0.247
1876	-0.487
1877	-0.192
1878	0.120
1879	-0.152
1880	-0.346
1881	-0.184
1882	-0.200
1883	-0.183
1884	-0.717
1885	-0.534
1886	-0.485
1887	-0.281
1888	-0.261
1889	-0.153
1890	-0.341
1891	-0.313
1892	-0.138
1893	-0.301
1894	-0.134
1895	-0.128
1896	-0.241
1897	-0.016
1898	0.065
1899	-0.574
1900	-0.218
1901	-0.049
1902	-0.287
1903	-0.142
1904	-0.205
1905	-0.308
1906	-0.034
1907	-0.412
1908	-0.048
1909	-0.214
1910	-0.147
1911	-0.194
1912	-0.631
1913	-0.161
1914	-0.294
1915	-0.074
1916	-0.277
1917	-0.297
1918	-0.460
1919	-0.013
1920	-0.272

1921	-0.114
1922	-0.036
1923	-0.305
1924	-0.141
1925	-0.258
1926	-0.115
1927	-0.198
1928	-0.018
1929	-0.161
1930	0.086
1931	0.104
1932	0.081
1933	-0.057
1934	0.007
1935	-0.037
1936	-0.019
1937	0.060
1938	0.163
1939	-0.075
1940	0.113
1941	-0.200
1942	0.128
1943	0.053
1944	-0.080
1945	0.059
1946	-0.016
1947	-0.188
1948	-0.038
1949	-0.107
1950	-0.269
1951	-0.100
1952	-0.118
1953	0.161
1954	-0.235
1955	-0.127
1956	-0.308
1957	-0.194
1958	-0.308
1959	-0.224
1960	0.076
1961	-0.104
1962	-0.289
1963	-0.173
1964	-0.479
1965	-0.474
1966	-0.171
1967	-0.200
1968	-0.599
1969	-0.355
1970	-0.353
1971	-0.328
1972	-0.563
1973	-0.262
1974	-0.336
1975	-0.507
1976	-0.558
1977	-0.363
1978	-0.698
1979	-0.289
1980	-0.612
1981	-0.195
1982	-0.522
1983	-0.234

1984 -0.335
1985 -0.423
1986 -0.430
1987 -0.424
1988 -0.161
1989 -0.286
1990 -0.275
1991 -0.169
1992 -0.175
1993 -0.341
1994 -0.320

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

147. 0939165392.txt
##########

From: "Sujata Gupta" <sujatag@teri.res.in>
To: <m.hulme@uea.ac.uk>
Subject: Re: UK National Climate Change Centre
Date: Tue, 05 Oct 1999 19:16:32 +0530
Cc: <t.d.davies@uea.ac.uk>

Dear Mike,

I was on travel and hence the delay in responding to your email. TERI will be interested in being one of the International Supporting Institutes for the Centre. I will fax a letter to you tomorrow and send the original by post.

I have not heard on the DETR proposal as yet.

Best wishes

Sujata

Sujata Gupta, Ph.D.
Fellow and Dean
Policy Analysis Division
TERI

>>> Mike Hulme <m.hulme@uea.ac.uk> 09/28/99 02:34AM >>>
Dear Sujata,

This may well not be news to you, but the UK government has recently requested bids from UK universities to house a new 'National Climate Change Centre'. The Centre would receive funds of 2 million pounds sterling per year for (at least initially) five years. The role of the Centre would be to compliment existing work on climate modelling and data analysis (IPCC WGI areas) by focussing on 'solutions' (mitigation and adaptation options and their implementation), specifically for the UK government and business community, but within a global context. The emphasis appears to be on IPCC WG3 area with a strong commitment to integrated research, but with some overlap with WG2. The Centre would carry out independent research, but

would also be expected to make use of, and to integrate, exisiting UK research and expertise. It would be expected to contribute to and to foster interdisciplinary research that underpins sustainable solutions to the climate change problem.

UEA is making a bid for this Centre. Applications are due by mid-October. UEA is well-known for CRU, but it also has strengths in data distribution to the climate impacts community, in impacts research, and in environmental economics (CSERGE). While these areas are fundamental foundation stones for the science that the Centre is expected to develop, the Centre would need to expand significantly beyond these areas. We have a Consortium in place as follows

- 6-7 Senior Partners - (UEA, UMIST, U.Southamton, Dept. Economics at U.Cambridge, Cranfield, Leeds Institute of Transport Studies, IH and ITE)
- Affiliated UK Organisations - (we have 6-8 of these)
- Supporting Business Links
- Supporting International Organisations

If UEA were to succeed in its bid for the Centre, then it would seek to develop strong links with other institutions abroad in order to strengthen its own intellectual base and, through such links, to contribute to the development and implementation of the science. We would see TERI as one of these Supporting International Organisations.

To this end, we would like a short letter of support from yourself - on behalf of the Policy Analysis Division, or a wider TERI grouping if you feel able to represent them - indicating that you fully support the UEA bid and would exclusively lend your backing to this Consortium and be keen to interact closely with us at a research level were the Centre to come to UEA. This interaction may take the form of exchanging scientists, testing out new methodologies, developing/advising on workshops, providing entry-points into international policy initiatives, etc., etc.

Nothing too formal or lengthy at this stage, but we would like to provide the Council's with a flavour of the breadth of our existing and future colloboration in the field and our ability to mobilise support in our favour.

Many thanks. Please send to Prof. Trevor Davies, Dean, Environmental Sciences, UEA, Norwich, NR4 7TJ, before the 12th October.

Feel free to ask me for more details, etc. Our written text is beginning to take shape and we will circulate a draft of this to you before the bid goes in.

Regards,

Mike

p.s. I have not yet heard anything about the DETR India Programme. Have you?

****
Dr Mike Hulme
Reader in Climatology tel: +44 1603 593162
Climatic Research Unit fax: +44 1603 507784
School of Environmental Science email: m.hulme@uea.ac.uk
University of East Anglia web site: http://www.cru.uea.ac.uk/~mikeh/
Norwich NR4 7TJ
****
Annual mean temperature in Central England for 1999
is currently about +1.4 deg C above the 1961-90 average
****

The global-mean surface air temperature anomaly for 1998
was +0.57 deg C above the 1961-90 average, the warmest year yet recorded
****

148. 0939235897.txt
##########

From: Tom Wigley <wigley@meeker.ucar.edu>
To: Mike Hulme <m.hulme@uea.ac.uk>
Subject: Re: outline bid for Centre
Date: Wed, 6 Oct 1999 14:51:37 -0600 (MDT)
Cc: j.Rotmans@icis.unimaas.nl, hasslemann@uea.ac.uk, "Stephen H. Schneider"
<shs@leland.stanford.edu>

Dear Mike,

I've not yet looked at your Tyndall biography, but I see your logic in
suggesting his name. His 1861 papers in Phil. Mag. Ser. 4, 22, 169-194
and 273-285 were arguable the first reasonable descriptions of the CO2
(or, in his words, "carbonic acid") greenhouse effect. However, it is
generally believed that Fourier, in 1827, was the first person to allude
to a greenhouse effect and to suggest that human activities might affect
the climate (see, e.g., Ramanathan, Science 240, 293-299, 1988).

In my view, however, neither Tyndall nor Fourier would be appropriate for
naming a climate centre devoted to human-induced change. Tyndall is not
appropriate because he did not consider (or even dream of) the human
influence; while Fourier is not appropriate because it would not be P.C.
to name a UK centre after a Frenchman. Furthermore, both Tyndall and
Fourier are well-known and well-recognized for their contributions in
other areas.

The person who really deserves the credit is Callendar who, in 1938, not
only suggested that human influences were causing CO2 to increase, but
also that this was causing global warming. Furthermore, he did an amazing
job documenting both the CO2 build up *and* the warming. Essentially, it
was Callendar who, more than 60 years ago, really exposed the problem that
is our current concern. His work was a quantum leap above anything done
previously; and, one could argue, was not really improved upon until
Manabe and Wetherald's seminal 1967 (JAS 24, 241-259) paper. I doubt
whether there is an intellectual milestone in *any* field that compares
with this.

Best wishes,

Tom

On Tue, 5 Oct 1999, Mike Hulme wrote:

> Dear 'Advisory Board member',
>
> As tentative nominees for the 'Advisory Board' for the UEA-led bid for the
> new UK National Climate Change Centre, I am sending you a first full draft
> of our outline bid. This is due with the Council's on the 15th October.
> Needless to say, please regard this document as confidential and do *not*
> circulate it to third parties.
>
> I would like to invite your comments in the next few days on the draft. I
> can accept comments until Tuesday 12th October, but earlier comments will

> prove most useful. Appended below is the communication sent out to our
> co-applicants with this draft. Please bear in mind that this is the first
> full draft we have put together and it is very rough and ready.
>
> You may find it easier to download from the named web site.
>
> Thank you for your time. Please direct any comments to the Consortium via me.
>
> Regards,
>
> Mike

****
*Tom M.L. Wigley *
*Senior Scientist *
*National Center for Atmospheric Research *
*P.O. Box 3000 *
*Boulder, CO 80307-3000 *
*USA *
*Phone: 303-497-2690 *
*Fax: 303-497-2699 *
*E-mail: wigley@ucar.edu *
****

149. 0939437868.txt
##########

From: Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>
To: Mike Hulme <m.hulme@uea.ac.uk>
Subject: Re: apologies
Date: Fri, 8 Oct 1999 22:57:48 +0200
Reply-to: Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>

Dear Mike,

I can understand you very well. I would have been more nervous about
this, hadn't the preparations AND registrations been going as well as
they have done: just now, I feel pretty comfortable about the meeting.
Sure, it's a pity not having you around, but I guess you are taking
the appropriate decision under your particular circumstances.

Perhaps I shouldn't be doing this, but let me add a VERY CONFIDENTIAL
piece of information for you. It won't make your life less stressful
during the next few days, and I really MUST ask you to keep this
confidential at your end (since I am effectively breaking a
confidentiality here, and I wouldn't want Edinburgh to know that), but
I received the following e-mail on October 6:

Dear Dr Cramer,

I am contacting you on behalf of Prof Paul Jarvis to check whether you
are willing to have your name mentioned in association with a project
he is hoping to undertake. The project is part of a much larger package
of projects which forms the nucleus of a bid being made by the
University of Edinburgh and other partners to host a new Climate Change
Centre, to be funded by the UK Research Councils at 10 million GBP over
5 years (for further details of this opportunity see:
http://www.nerc.ac.uk/press/aooclim.html). I work in a small unit of
the University of Edinburgh that has responsibility for co-ordinating

multi-disciplinary environmental research bids. Currently we are preparing the Outline Bid (deadline 15 October), so nothing should be regarded as firm, and details will be open to modification in the Full Bid, which we will prepare if the Outline Bid is successful.

Below I reproduce the text we are proposing to include in the Outline Bid. Please confirm whether or not you are willing to have your name included.

Please treat this email as confidential.

Best regards,

Simon Allen.

==========
Dr S J Allen, Research Co-ordinator
Centre for the study of Environmental Change and Sustainability (CECS)
University of Edinburgh
John Muir Building, King's Buildings, Mayfield Road, Edinburgh EH9 3JK

Tel: 0131 650 7215 Email: simon.allen@ed.ac.uk
Fax: 0131 650 7214 http://www.cecs.ed.ac.uk
==========

Issue: Will terrestrial carbon sinks saturate?

It has been proposed that the assimilation of CO2 by vegetation will reach saturation within the foreseeable future as atmospheric CO2 concentrations continue to rise and that, conversely, increase in temperature will lead to open-ended increase in respiration by soil heterotrophs, so that at some point in the not too distant future, CO2 efflux will come to exceed CO2 influx.

This far-reaching assumption derives from global models that lack a consideration of acclimation, feed backs and biological constraints acting on these processes. This proposition will be critically evaluated using Dynamic Global Vegetation Models (DGVM's) that include appropriate feed backs derived from new data that are becoming available from on-going experiments in the UK and elsewhere. This core project will be executed over two years by a research fellow at the University of Edinburgh, under the supervision of Professor Paul Jarvis, FRS. The project will involve close collaboration with: the Max Planck Institut fur Biogeochemie (Prof I Colin Prentice) and the Potsdam Institute for Climate Impacts Research (Dr Wolfgang Cramer) where fully operational DGVMs are in use; the Dept of Production Ecology, University of Uppsala (Prof Sune Linder), currently conducting soil warming experiments in northern Sweden.

Costs (GBP):	Yr 1	Yr 2
Research fellow	50 k	52 k
Travel/interaction	4 k	4 k
Total project cost:	54 k	56 k

-----end of Edinburgh mail-----

To me, this comes at a very strange moment, since I am, with Bert Bolin, in a very strange situation with the completion of our second draft of the IPCC Special Report on Sinks due Land Use and Forestry. The very issue they propose to collaborate with Colin and myself about was the most contentious one of all, and Paul on one side, and several

others including myself on the other side, had diametrically opposing opinions. In fact, I simply believe Jarvis either wasn't able or not wasn't willing to understand what the real issue was.

Anyway, I don't know whether, and if, in which way, this may or may not affect your completion of the UEA bid, but I thought I'd better let you know. Obviously I discussed this with Colin, and his response is that he a) would place his bet on your rather than the Edinburgh bid in terms of potential success, and b) that he nevertheless thinks Edinburgh is proposing the appropriate thing to do here, and that he therefore will reply positive to their request for collaboration. Unless you see a strong reason for recommending me to NOT do the same (we can talk about this in Brussels of course), I shall probably reply in the same positive way.

Take care,

Wolfgang

PS: I am really uncertain whether I do something terribly bad in sending this to you, after the explicit request for confidentiality - so please keep this among the two of us...

On Freitag, 8. Oktober 1999, you wrote:

> Wolfgang,

> I shall have to apologise, but I will not be able to make the ECLAT meeting
> at all. The pressures of getting our UK National Climate Change Centre
> outline bid together for the 15th October are now such that I have to be
> here on the 13th and 14th (being in Brussels in the 12th is not very
> helpful either, but I can at least get back to UEA for Wednesday/Thursday
> to wrap up the bid). I have the lead responsibility now at UEA for
> co-ordinating our proposal - 8 institutions, 24-co-applicants, so you can
> imagine the headaches involved. But we want to make sure Hans-Joachim has
> a good proposal tabled from UEA when he meets with the Assessment Panel
> later in November!

> I really regret not being there - you have done a great job in pulling the
> programme and people together amidst IPCC activities. I have asked Tim
> Carter to present the IPCC/ACACIA speech and I am sure he will!

> Tim Carter and David Viner will co-ordinate over what needs doing for the
> proceedings which I insist will be a Cramer et al. (ed) (1999/2000)
> publication. David and Ruth will bring several dozen copies of the
> Helsinki book for distribution. It is important to get the breakout groups
> to get text together on their deliberations while at the meeting. You will
> see what we have done to the Helsinki material. For the Green Workshop we
> should not exceed 100pp. (cf. 128pp. for Helsink) and colour should be
> avoided where possible. CRU will take over the sub-editing and desk-top
> publishing role again.

> I guess I will see you in Brussels anyway.

> Gabi ..... please cancel my hotel reservation and travel pick-up.
> Thank you for your efficiency in organising all this.

> Best regards,

> Mike

mailto:Wolfgang.Cramer@pik-potsdam.de

150. 0939844657.txt
##

From: "R K Pachauri" <pachauri@teri.res.in>
To: <m.hulme@uea.ac.uk>
Subject: Workshop on "North-South Strategies for Sustainable Development", November 1, 1999
Date: Wed, 13 Oct 1999 15:57:37 +0530

Workshop on "North-South Strategies for Sustainable Development", November 1, 1999

Dear Dr Hulme,

TERI is hosting an event at the Fifth Conference of the Parties on "North-South Strategies for Sustainable Development". At this event we intend to generate a discussion on the impetus for furthering the objectives of the UN Framework Convention on Climate Change. Not only is there a need to review the provisions in the Kyoto Protocol but also to develop a framework for operationalizing it. In particular, the workshop will focus on the Clean Development Mechanism. The workshop also aims to identify drivers that could maintain the momentum, which was achieved at Kyoto, ratification of the Protocol notwithstanding.

Hoping you were already at Bonn, I would like to invite you to provide your valuable viewpoint as a discussant at our event scheduled for November 1, 1999 at Hotel Maritim from 1800 - 1930 hours. A brief background note highlighting the issues intended for discussions during the Workshop as well as the Workshop agenda is attached herewith for your perusal. In case you have not planned for Bonn, I would deeply appreciate it if you could forward this mail to prospective participants to COP 5.

Thanking you and looking forward to meeting you at Bonn.

With warm regards,

R K Pachauri

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151. 0939923089.txt
##

From: Eric Steig <steig@igl.geol.upenn.edu>
To: domraynaud@glaciog.ujf-grenoble.fr
Subject: No Subject
Date: Thu, 14 Oct 1999 13:44:49 -0400 (EDT)
Cc: jto@ngdc.noaa.gov, k.briffa@uea.ac.uk, icdc@igl.geol.upenn.edu

Dear Dominique,

Jonathon Overpeck forwarded your email to me some time ago, regarding Holocene ice core data. I apologize for the delay in responding.

Frist, regarding US contacts for ice core data. I am happy to work on this as you suggest, and it certaintly makes sense to have me involved

since I have been working on ice core data management for some time. I can probably do a good job representing the US Arctic/Antarctic community, but Lonnie Thompson should also be contacted, since there is so much data from tropical glaciers that is not yet publicly available. In any case, I look forward to working with you on this.

Second, regarding ice core relevant for Holocene studies:

It would be ideal to include all of the Antarctic cores drilled so far: Dome B, Dome C, Vostok, Komsomolskaya, Byrd, etc. Much of the stable isotope data for these cores is already available at our "Ice Core Data Cooperative" web site. Valerie Masson, Jean Jouzel, myself and others recently submitted a paper comparing isotope data from all of these cores, and I should be able to get the data from her. Also at the Data Co-op site are data from the Canadian ice caps (we do not yet have Penny Ice Cap, but I can talk with David Fisher about this), Mount Logan, and from some temperate ice cores including Fremont Glacier. These data are better than commonly believed and may be useful.

I think that any Holocene climate compilation really needs chemistry and gas data as well as isotope data. Although chemical concentrations have not been measured on many of the cores, a very important data set that is missing from our current archive is the chemistry data from the Antarctic cores. All of the Taylor Dome chemistry data is available at www.sas.upenn.edu/~esteig/taylor.html but as far as I am aware there is no other chemistry data out there. It would be wonderful if you could convince Michel Legrand and colleagues to send these data to me, for inclusion on the Ice Core Data site, for both the Holocene the glacial periods.

All of the data that I currently have are available via the NOAA web server "International Ice Core Data Cooperative". The site also lists cores which exist but for which data are still needed. The direct link is:

http://www.ngdc.noaa.gov/paleo/icecore/iicdc.html

I apologize that the pages are not in very good order; most of my time when I had hoped to be working on this was devoted to the production of the GISP2/GRIP CDROM, which took considerably more effort than expected. I plan to begin improving those pages soon. Let me know if you have additional questions.

Warm regards,

Eric Steig

152. 0941483736.txt
##########

From: Tom Wigley <wigley@meeker.ucar.edu>
To: Mike Hulme <m.hulme@uea.ac.uk>
Subject: Re: CONFIDENTIAL: CRU scenarios
Date: Mon, 1 Nov 1999 14:15:36 -0700 (MST)
Cc: rwatson@worldbank.org

Dear Mike,

Thanks for your detailed response about your use of the SRES scenarios. I'm sure it will be useful to Bob Watson. I wish I could explain better what Bob's problem entails -- it is intensely political. My judgement is that, if I tell you more, then this will indirectly help Bob in answering the questions posed of him by Sensenbrenner; particularly should Bob need to get back to you. Please note that this is confidential information. Please note, too, that I am making my own judgement on this in the interest of clarifying a complex issue. I have not been authorized by Bob, or anyone associated with IPCC, to divulge this information.

The stated concern of Sensenbrenner is that the use of the SRES scenarios prior to their ratification might, in some way, jeopardize IPCC's "independence and objectivity". Sensenbrenner apparently uses as guidelines in making his judgement "IPCC's 'Principles' (as) approved in Vienna, Austria in October 1998" together with "June 11 and 28, 1999 letters" giving "Appendix A to the Principles, which is entitled 'Procedures for the Preparation, Review, Acceptance, Approval and Publication of IPCC Reports' (which was) approved ... in April 1999". Sensenbrenner implies that these documents "raise concerns about the use of preliminary IPCC material by Dr. Wigley and the Pew Center on Global Climate Change for non-IPCC purposes, apparently without IPCC sanction". He considers that "these issues (are) significant because they relate directly to the integrity of the IPCC process".

In my case, I bypassed the "IPCC process" by obtaining permission, in writing, from the 4 groups who produced the marker scenarios. I did not acknowledge the CIESIN web site. In your case, apparently, you did. The problem here is that this site stated very clearly that the data were "not for citation or quotation". Did you take notice of this?

My view is, and has always been, that contributors to such data sets or distribution sites do not give up the intellectual property rights to their own data. They could do so, of course, by signing appropriate legal/copyright documents; but I have never done this, nor, as far as I know, has anyone who contributed to the CIESIN site. This is why I went to the individual authors in order to obtain permission to use their data in my Pew report. I hope you can see that there is an important difference between what you did and what I did. At face value, it would appear that you have ignored the clearly-stated message that the CIESIN site data were "not for citation or quotation". (More on this point below.)

You refer back to the July 1998 Bureau meeting agreeing that the preliminary SRES scenarios (in your words) "could, and should, be used by scientists". From my reading of the background material, this is subtly wrong -- the Bureau only agreed that the data could be used by "the GCM modeling community". As it happens, I am part of that community, and I acted as the interface between the scenarios and the rest of the NCAR GCM team, providing SRES data to them in a form that could be used for our GCM runs. I do not think you can claim to have filled this particular and quite specific role in your work.

However, there are some interesting subtleties here that, I think, vindicate your position. The issue is what is meant by the "GCM modeling community". In my view, anyone who uses GCM data either to provide data sets to the impacts community or to carry out diagnostic studies directly to improve GCMs is part of this community. (Note that this does *not* allow one to include the impacts modelers as part of the GCM community.) The two stated aspects are precisely what you do. Furthermore, SCENGEN (which I presume you have used in your work) makes direct use of GCMs in order to produce spatially-specific climate results based on any given emissions scenarios (including the SRES scenarios). The SCENGEN method is

simply an alternative way of translating emissions scenarios into GCM-based and GCM-type output. In my view, anyone using the SRES scenarios in the development of SCENGEN, or applying SCENGEN to produce spatially-specific climate results for dissemination to others, must be included as part of the "GCM modeling community" referred to in the Bureau's agreement regarding use of the SRES scenarios. You may have interpreted the Bureau's statements even more broadly than this -- but this is of no consequence, since what you have done also falls squarely within the more restricted interpretation that I have given above.

Nevertheless, I think it would have been wiser for you to have done things the way I did, rather than to have acknowledged the CIESIN site as your source.

The next issue, raised in your email, concerns the DDC. I have not looked at this site, but I presume it duplicates what was on the CIESIN site. If so, then its use (and the use of the preliminary SRES data) must be controlled by the rules under which the DDC was set up and operates. The key questions, therefore, are:

(1) Do these rules allow the use of these data by anyone?
(2) Do the SRES data, as it appears on this site, include the statement "not for citation or quotation"?
(3) Does this make moot the whole issue of the use of the SRES scenarios?

In other words, if these data are available to all and sundry, with no restrictions, through DDC, then no one can complain about their use. (Although, in your case, since you acknowledged CIESIN rather than DDC, you may still be subject to criticism.)

What this could amount to is a loophole in the IPCC rules of procedure. Sensenbrenner might then argue that this loophole should be closed by clarifying and tightening the rules for the DDC.

The bottom line is that I think you have done things in a perfectly legitimate way. Even acknowledging the CIESIN site is legitimate, since your primary application was in the production of climate change scenarios as a member of the "GCM modeling community" as I believe this community should be defined. You have then distributed these results to the global climate impacts community who, in turn, will be feeding their results back into the IPCC process through WGII. Your chosen method of distribution (especially the WWF pathway) might be judged as less than ideal; but I cannot see anything that you have done that goes explicitly or implicitly against IPCC regulations.

Below the bottom line is the concern expressed by Sensenbrenner that these actions (yours and mine) might, in some way, have undermined the "integrity of the IPCC process". It would be interesting to hear from Sensenbrenner just how he thinks that might have happened. All we have done is distribute credible and defensible scientific information. If this information were to be in conflict with the currently best-available science, this might be an issue of concern -- but it is not. The more such credible scientific information is distributed to the community, particularly when it is presented in an easily-read, non-technical yet authoritative way, the better. I can see no way that this can distort the IPCC process. Some people, however, appear to think that it might. (A less kind interpretation might be that they are just trying to slow down the process by tying it up in legal and procedural knots -- but I have no evidence that this is what they are trying to do.)

I hope you can see from the above quotes and somewhat convoluted arguments what a legal and political minefield this is. These sorts of issues do not seem to arise outside of the USA; but here they take on an enormous

importance. One must tread very cautiously.

Cheers,

Tom

On Sat, 30 Oct 1999, Mike Hulme wrote:

> Bob,
>
> You will have seen Tom Wigley's email asking me about the climate scenarios
> I prepared for WWF and which were distributed 2 weeks ago. I have just got
> back from a trip away and am concerned that *you* are concerned, hence my
> immediate reply.
>
> These CRU/WWF regional/national scenarios *do* use the preliminary SRES98
> emissions scenarios that are posted on the CIESIN and IPCC DDC web sites.
> The CRU/WWF reports state that preliminary emissions scenarios sre used,
> they acknowledge the CIESIN source of these emissions, and they make it
> clear that the derived climate scenarios are the work and responsibility of
> the authors alone.
>
> Maybe some background would help explain why I do not think that from my
> perspective there is cause for concern (although I am aware of the
> criticism the SRES report has increasingly been receiving and that the
> issues are bigger than I may realise):
>
> _____
>
> July '98: IPCC Bureau meeting agreed that the preliminary SRES emissions
> scenarios could, and should, be used by scientists in their unapproved
> form.
>
> Dec '98: the above was reiterated to WGI scientists at the Paris LA
> meeting. In particular, it was recognised that SAR science would have to
> be used in the interim (i.e., next 12-18 months) to generate the climatic
> (and consequently impact) implications of the SRES emissions.
>
> Jan '99: the SRES Open Process ended. The IPCC DDC placed the preliminary
> SRES98 emissions scenarios on the open DDC web site as requested by the
> IPCC Task Group on Climate Scenarios for Impact Assessment (Chair Martin
> Parry). The objective of the DDC right from its original 1997 commission
> was to provide timely access to emissions scenarios, observed climate
> datasets and new GCM experiments (all of which would be assessed in the
> IPCC TAR), thus enabling impact scientists worldwide to construct and apply
> consistent climate scenarios (this information has already been used by
> several 100 scientists, including many in developing countries). Only in
> this way would it be at all possible for WGII to have access to
> impact/adaptation science that was in any way consistent with the WGIII
> (SRES emissions) and WGI (climate modelling) material. The placing of the
> SRES98 emissions on the DDC web site was widely discussed in the TGCIA and
> was publicised at the time to the research community using the DDC,
> including through the A4-flier advertising the DDC that was sent to the WGI
> (and WGII?) mailing list.
>
> Feb '99: Hulme&Carter used the preliminary SRES98 emissions (and other DDC
> products) to develop climate scenarios

> for the European Union as part of the EU-funded ACACIA assessment
> (unrelated to Tom's US-based ACACIA). The approach
> I took in using the SRES98 emissions for the ACACIA climate scenarios was
> *my* decision and was not part of any IPCC activity. The ACACIA climate
> scenarios, and indeed entire EU ACACIA impacts assessment, have been widely
> reviewed within Europe, and are part of the draft report presented to
> Brussels last month. They will published in their final form in June 2000.
> This EU-ACACIA activity has done in my view *exactly* what the DDC was
> intended to do, namely allow impact scientists to generate results using
> consistent scenarios and assumptions; these results provide the raw
> material for IPCC LAs to assess in their TAR chapters!
>
> My approach for converting the preliminary SRES98 emissions into climate
> scenarios is also being used in many other EU and UK-funded impact research
> programmes and is generating a variety of scientific reports and papers -
> several of the latter are under peer-review at the moment and may be
> citeable in time for the 2nd-order WGII drafts.
>
> ***Is an apology needed for this activity? If so, then I and others on the
> IPCC TGCIA totally misunderstood the brief of the DDC and the intent of the
> July 98 and Dec. 98 IPCC decisions.***
>
> May '99: WWF commissioned me to prepare a set of national/regional climate
> scenarios for them to launch in October 1999. It seemed entirely
> appropriate and legitimate to me to use the same method I had adopted for
> EU-ACACIA to generate these WWF scenarios.
>
> June '99: Tom's Pew Report was published using SRES98 emissions is a not
> dissimilar way to me (i.e., using them to drive a simple climate model
> based on SAR science).
>
> July '99: following some controversy over the Pew Report, there was an
> email circular from WGI TSU (Griggs) reminding LAs that there was 'active
> encouragement' from IPCC for scientists to use the preliminary SRES98
> emissions in modelling work. The conditions were that it should be stated
> that they were unapproved by IPCC (i.e, preliminary) and that work using
> them should ideally be peer-reviewed and published. Tom Wigley followed-up
> on this circular by explaining *his* use of SRES98 in the Pew Report, the
> conditions he met and his justification for using them. I noted this
> correspondence at the time and did not feel that my use of SRES98 emissions
> in my WWF work was out of order.
>
> Oct '99: the 15 sets of CRU/WWF regional/national scenarios were published
> and widely distributed by WWF. These leaflets state that 'preliminary IPCC
> emissions scenarios' are used, acknowledge the source of these emissions as
> the CIESIN site, and make clear that the climate scenarios are the work of
> the authors alone and no other organisation. Furthermore, the approach I
> have taken (which I originally designed back in December 1998) has been
> subject to a diversity of peer-review activities, and will shortly be
> published.
> _____
>
> Sorry for making this a lengthy reply, but it seems best to spell out the
> history and my thinking to avoid any room for misunderstanding. In
> summary, the only two grounds on which I think I could be criticised for
> using the SRES98 emissions in my CRU/WWF climate scenarios are if:
>
> 1) the IPCC DDC was wrong to put the SRES98 emissions on its web site back
> in January 1999 and to publicise its purpose in doing so. If we *were*
> wrong, then this error goes back to January 1999 and the TGCIA
> fundamentally misunderstood its brief.
> 2) the pronouncements of the IPCC in July 1998 and December 1998 were
> intended to apply *only* to scientists who had a formal role in the IPCC

> and that the SRES98 emissions could only be used for 'official' IPCC
> scientific activities whatever these may be. This would draw a very
> dubious line between science done for IPCC and science done 'not for IPCC'.
> IPCC's brief is to assess *all*, done by no matter whom or for what
> purpose.
>
> Best wishes,
>
> Mike
>
> Dr Mike Hulme
> Reader, Climatic Research Unit
> School of Environmental Sciences
> University of East Anglia
> Norwich NR4 7TJ
> (tel: +44 1603 593162; fax: +44 1603 507784)
> (email: m.hulme@uea.ac.uk)
> (web: http//www.cru.uea.ac.uk/~mikeh)
>
> ----------
> > From: Tom Wigley <wigley@meeker.ucar.edu>
> > To: Mike Hulme <m.hulme@uea.ac.uk>
> > Cc: Robert Watson <rwatson@worldbank.org>
> > Subject: CONFIDENTIAL: CRU scenarios
> > Date: 27 October 1999 19:02
> >
> > ****In strictest confidence****
> >
> > Dear Mike,
> >
> > Bob Watson contacted me last week asking about some climate results that
> > he apparently saw on the CRU and/or WWF web pages. The CRU web site
> > states that you have produced (and already distributed) a set of regional
> > scenario leaflets based on "new ghg emissions scenarios", which I think
> is
> > what Bob may be concerned about.
> >
> > I hope that "new" does not refer to the SRES scenarios. You may recall
> > that, when I was in CRU, I showed you, in confidence, a letter from F.
> > James Sensenbrenner, chairman of the U.S. House of Representatives
> > Committee on Science, criticizing IPCC for "allowing" me to use these
> > scenarios in my Pew Report.
> >
> > Unfortunately, this issue is not going away, and any further perceived
> > "misuse" of the SRES scenarios prior to their IPCC ratification would
> > exacerbate the problem considerably.
> >
> > I do hope, therefore, that you have *not* used the SRES scenarios. I
> > expect not, since I explained the potential problems to you in July.
> > Please reassure me -- and Bob.
> >
> > If, by chance, you *have* used the SRES scenarios, but not yet
> distributed
> > the WWF leaflets, I urge you to hold fire until you have contacted Bob.
> >
> > Best wishes,
> >
> >
> > Tom
> >
> >
> >
> >   **

> > *Tom M.L. Wigley *
> > *Senior Scientist *
> > *National Center for Atmospheric Research *
> > *P.O. Box 3000 *
> > *Boulder, CO 80307-3000 *
> > *USA *
> > *Phone: 303-497-2690 *
> > *Fax: 303-497-2699 *
> > *E-mail: wigley@ucar.edu *
> > **********
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> >
>

****
Tom M.L. Wigley
Senior Scientist
National Center for Atmospheric Research
P.O. Box 3000
Boulder, CO 80307-3000
USA
Phone: 303-497-2690
Fax: 303-497-2699
E-mail: wigley@ucar.edu
Web: http://www.acacia.ucar.edu
****

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From: Mike Hulme <m.hulme@uea.ac.uk>
To: wigley
Subject: MAGICC/SCENGEN
Date: Fri Nov 12 18:19:52 1999
Cc: s.raper,m.salmon,m.hulme,barrow

Tom,

Sorry I couldn't say goodbye - I was actually on the phone to Bo Lim at the time. I also wanted to ask you about your views on the UK national climate change centre, but this can wait until later.

Anyway, about MAGICC/SCENGEN Workbook I think we agreed the following things for this UNDP version ....

- a select number of emissions scenarios, IS92, SRES98, 550 and 750 stabilisation cases, some Kyoto variants (perhaps from IS92a,e,d reference), and 1-2 others you may recommend. I would be keen to use your *.gas files if that's OK, even though I have some of my own. You may have done the SO2 into regions, which I haven't. Could you send me a selection?

- you would think about how to handle the CH4 adjustment to ensure SAR replicability across the emissions scenarios. This may require a tweak in the MAGICC code which Mike will have to recompile.

- we should aim to reproduce the SAR results as closely as possible in this version, e.g. use 6.37Wm-2 rather than 5.5, and not use Prather's methane concentrations (an Annex in the Workbook will explain this).

- the variable upwelling rate will be hard-wired. Choices will remain for the Dn80s, climate sensitivity and aerosol forcing.

- SCENGEN will have the new DDC patterns included and we will switch off the buttons for the older 2xCO2 patterns.

- SCENGEN will output values over land and ocean.

- the Help Screens will need updating. I will attempt this and then check them all with you to make sure you agree.

The only problem I can forsee is that the 2.32 version that Mike and you produced in the summer corrected the aerosol calculations and also used Prather's methane concentrations. If we now want a version with correct aerosol concentrations and IPCC SAR Chapter 6 CH4 concentrations, *plus* a CH4 tweak to handle the ad hoc adjustment, then Mike Salmon will need a new and unique FORTRAN version of MAGICC. Am I right?

I have agreed with Bo Lim to get a first draft of the Workbook by 17 December, but the final version and all the CDs will not be agreed until February 2000.

Have I missed anything?

Regards,

Mike

154. 0942777075.txt
##########

From: Phil Jones <p.jones@uea.ac.uk>
To: ray bradley <rbradley@geo.umass.edu>,mann@virginia.edu, mhughes@ltrr.arizona.edu
Subject: Diagram for WMO Statement
Date: Tue, 16 Nov 1999 13:31:15 +0000
Cc: k.briffa@uea.ac.uk,t.osborn@uea.ac.uk

Dear Ray, Mike and Malcolm,
Once Tim's got a diagram here we'll send that either later today or first thing tomorrow.
I've just completed Mike's Nature trick of adding in the real temps to each series for the last 20 years (ie from 1981 onwards) amd from 1961 for Keith's to hide the decline. Mike's series got the annual land and marine values while the other two got April-Sept for NH land N of 20N. The latter two are real for 1999, while the estimate for 1999 for NH combined is +0.44C wrt 61-90. The Global estimate for 1999 with data through Oct is +0.35C cf. 0.57 for 1998.
Thanks for the comments, Ray.

Cheers
Phil

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

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From: Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>
To: "F. Ian Woodward" <F.I.Woodward@Sheffield.ac.uk>, "Nigel W. Arnell" <N.W.Arnell@soton.ac.uk>, Alberte Bondeau <Alberte.Bondeau@pik-potsdam.de>, Ben Smith <Ben.Smith@planteco.lu.se>, Colin Prentice <Colin.Prentice@bgc-jena.mpg.de>, Harald Bugmann <bugmann@waho.ethz.ch>, José Manuel Moreno9yZW5v <jmmoreno@greco.cc-to.uclm.es>, Mark Rounsevell <rounsevell@geog.ucl.ac.be>, Martin Sykes <vxt_masy@luecology.ecol.lu.se>, Mike Hulme <m.hulme@uea.ac.uk>, Pete Smith <pete.smith@bbsrc.ac.uk>, Pierre Friedlingstein <pierre@lsce.saclay.cea.fr>, Riccardo Valentini <rik@unitus.it>, Rik Leemans <Rik.Leemans@rivm.nl>, Sandra Lavorel <lavorel@cefe.cnrs-mop.fr>, Sergey Venevski <Sergey.Venevski@pik-potsdam.de>, Stephen Sitch <Stephen.Sitch@pik-potsdam.de>, Torben Christensen <torben.christensen@planteco.lu.se>, Wolfgang Knorr <knorr@dkrz.de>, Wolfgang Lucht <Wolfgang.Lucht@pik-potsdam.de>
Subject: A-TEAM Call is out
Date: Thu, 18 Nov 1999 14:33:21 +0100
Reply-to: Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>

Dear colleagues,

you may already know it: the EU FP5 second call for proposals is out since today (http://www.cordis.lu/eesd/calls/calls.htm), as expected, and the deadline for submission is Feb 15.

The new call does indeed answer a question I have been wondering about when I heard from many first-call projects that they were asked to re-submit. The present call is only for the slots that were, last year, declared to be opened at this stage (not for the previous slots). Probably the re-invited proposals then still bid for the old money (or at least, I hope so).

There is however one important exception: "2.3.1 Mitigation and adaptation to global change". About this, the official document says "re-open ... because of the quality of proposal received in reply to the call of 20 March 1999". Further down, they point out that Kyoto really is tremendously important for the commission ("primary objective"), and then comes the following far-reaching sentence: "If one takes into account the time lag between the research results, the political decisions and the actual emission reduction it is evident that the year 2000 is the last opportunity for research to cover the remaining analytical gaps of priority." (da_pg2_en_199902.pdf, page 6). Tough!

This mail goes to all people I have currently listed as "likely participants in A-TEAM", although the group may either grow or shrink as the remaining time passes by (depending, among other things, on your inputs!).

The present state of development is that I have recently circulated another draft of our basic document among just a few of you, hoping for input to it REALLY SOON. On the basis of this, I intend to

a) develop a better draft that will then be circulated to all of you,

b) organize a small brainstorming meeting, hopefully before christmas, but again only with a small core group,

c) by christmas, provide you with a roadmap for the remaining things to be done.

Best regards,

Wolfgang

Wolfgang Cramer
Department of Global Change and Natural Systems
Potsdam Institute for Climate Impact Research
PO Box 60 12 03, D-14412 Potsdam, Germany
Tel.: +49-331-288-2521, Fax: +49-331-288-2600
mailto:Wolfgang.Cramer@pik-potsdam.de

NOTE: IF YOU NEED TO SEND ATTACHMENTS TO ME, PLEASE:
1) avoid sending MS-Word *.doc files (send rtf instead)
2) if the attachments exceed 500kB, contact me before sending anything

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