Alleged CRU Emails - 25 results below

The below are part of a series of alleged emails from the Climate Research Unit at the University of East Anglia, released on 20 November 2009.

Browse by **10** | **25** | **50 100**

Enter keywords to search (no need for quote marks)

[   ] [Search]

Original Filename: **826209667.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: "Tatiana M. Dedkova" <tatm@xxxxxxxxx.xxx>
To: K.Briffa@xxxxxxxxx.xxx
Subject: schijatov
Date: Thu, 7 Mar 96 09:41:07 +0500

Dear Keith, March 6, 1996

I and Eugene received your E-mail of 04.03.1996. This day I talked
over the telephone with Eugene and he asked me to send an answer from
both of us.
Thank you for the information concerning proposals to the
INCO/COPERNICUS. We agree with your strategy used and we hope
that this proposal will not be rejected.
The results of INTAS-RFBR proposal will be known at the beginning
of May. We know that they received many proposals and a competition
is high (only 1 in 10 proposals might get money). Of course, you
included in as a participant. Fritz is a coordinator from the INTAS
countries.
This year our laboratory received two small grants (approximately
8,000-10,000 USD per year) from the Russian Foundation of Basic
Researches (RFBR) for the next three years: the first one for
developing the Yamal supra-long chronology and the second one for
developing tree-ring chronologies from living trees growing at the
polar timberline in Siberia (together with Vaganov's laboratory).
These money are very important for us as they will allow to maintain
the staff of our laboratories.
I and Valery Mazepa were in Krasnoyarsk during one month and
together with E.Vaganov wrote the manuscript of book "Dendroclimatic
Studies in the Ural-Siberian Subarctic". The problem now is to find
money for its publication. If we find enough money soon (20 million
roubles), the book will be published this autumn. We analysed 61 mean
ring-width and 6 cell chronologies which we intend to publish in form
of tables in the Appendix. We can send to you all raw measurements
which were used for developing these chronologies.
Of course, we are in need of additional money, especially for
collecting wood samples at high latitudes and in remote regions.
The cost of field works in these areas is increased many times
during the last some years. That is why it is important for us
to get money from additional sources, in particular from the ADVANCE
and INTAS ones. Also, it is important for us if you can transfer
the ADVANCE money on the personal accounts which we gave you earlier
and the sum for one occasion transfer (for example, during one day)
will not be more than 10,000 USD. Only in this case we can avoid
big taxes and use money for our work as much as possible. Please,
inform us what kind of documents and financial reports we must
represent you and your administration for these money.
I and Eugene have a possibility to participate in the Cambridge
meeteng in July, but we need extra many and special invitations.
If you do not have enough money to invite both of us, Eugene does
not insist upon this visit.
The best wishes to you and Phil.

Yours sincerely Stepan Shiyatov

What are others looking for?

- **idiot**
- **bush**
- **revkin**
- **gavin**
- **watts**
- **false**
- **daly**
- **rahmstorf**
- **money**
- **foia**
- **flannery**
- **greenpeace**
- **trenberth**
- **briffa**
- **bolt**
- **delete**
- **hide**
- **australia**
- **lie**
- **manipulate**

Original Filename: **835015638.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: "Tatiana M. Dedkova" <tatm@xxxxxxxxx.xxx>
To: k.briffa@xxxxxxxxx.xxx

Subject: schiyatov
Date: Mon, 17 Jun 96 08:47:18 +0500

Dear Keith,
I have bought the tickets from Moscow to London and back. My arrival
to London (Heathrow Airport) is by flight SU 245 (Aeroflot Company) on
July 19. Departure from Moscow is at 20.10 (local time), arrival to London
is approximately at the same local time. As I know, Evgeny Vaganov did not
bay tickets until now, but he informed of my dates and can bay tickets the
same flights. My depature from London to Moscow is on August 1 by the
Aeroflot Company flight SU 244 at 09.00 of local time.
Please, inform me how can I arrive at Cambridge from London? Is
there the program of this meeting? We must be ready to do some reports?
For example, I can prepare a report about the progress in developing the
Yamal supra-long chronology and together with Evgeny about dendroclimatic
investigation in the Ural-Siberian subarctic.
Rashit Hantemirov and Alexander Surkov will go soon to the Yamal
peninsula (June 24). This summer they want to collect subfossil material
from areas which are much more remote and situated at higher latitudes.
We hoped to use some money of the ADVANCE project. But we have not received
this money until now and the program of collecting during this summer will
be reduced.
Some days ago I received an information that the INTAS-RFBR project
was rejected. The competition was very high.

Sincerely yours Stepan Shiyatov

Original Filename: **835819980.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: km_king@xxxxxxxxx.xxx
To: F028@xxxxxxxxx.xxx
Subject: URGENT RESPONSE NEEDED - Early Detection Work
Date: Wed, 26 Jun 1996 16:xxx xxxx xxxx(PDT)

Dr. Jones,

I am contacting you on behalf of Dave Bader and Tim Barnett regarding a couple
action items in support of early detection on climate change. Based upon the
anticipated award for NOAA support during fiscal year 1997 on climate change
data and detection, DOE has authorized the Pacific Northwest National
Laboratory (PNNL) to utilize existing funding through 9/30/96 to conduct a
meeting of the experts, and to begin preliminary investigations.

PNNL would like to place a contract with you as soon as possible to provide
support through 9/30/96. In order to place a contract with you, I need to
submit a statement of work and signed cost proposal to our Contracts
Department. If you could please fax this to me as soon as possible on (509)
xxx xxxx xxxx, it would be greatly appreciated.

I thought your activity my look something like the following (feel free to
change/edit):

Scope of Work

Dr. Phillip Jones shall begin initial work in support of the pilot project
identified in the Early Detection of Climate Trends report. He shall prepare
for and participate in a meeting on greenhouse signal detection, to be held in
Washington, DC on September 17-18, 1996. In addition, Dr. Jones shall conduct
a preliminary analysis ?????? (please provide input)

Deliverables

Prepare for and participate in 9/17-18, 1996 meetings on greenhouse signal.

Provide a summary report on the preliminary analysis of ?????? on or
before September 30, 1996.

Also, for your information the current plan for the meeting is for September 17-
18, 1996 at the Courtyard by Marriott - Greenbelt, 6301 Golden Triangle Drive,
Greenbelt, MD. (3xxx xxxx xxxx, fax: (3xxx xxxx xxxx. Government room rate is
$89/day.

When you provide your cost estimate, it would be appreciated if you could
provide your hourly rate, in addition to travel estimates for the September
meeting. To expedite the process, it is very helpful if can include
documentation to support your hourly rate.

Please feel free to contact me with any questions. My phone number is xxx xxxx xxxx

2861, fax is xxx xxxx xxxx.

Thank you,

Karen

Original Filename: **837094033.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: Alan Robock <alan@xxxxxxxxx.xxx>
To: Phil Jones <p.jones@xxxxxxxxx.xxx>
Subject: Re: your mail
Date: Thu, 11 Jul 1996 10:07:xxx xxxx xxxx(EDT)

Dear Phil,

It looks like you have found Baitoushan. Vol. 2 lists Kuwae as VEI 6 in
1452 +/- 10 AD. How accurate are your dates? By the way, Chris Newhall
thinks 1600 is the Parker volcano on Mindanao in the Philippines. He
hasn't published that so far, as I know.

Could you please define "utter prat" for me? Sometimes I think we speak
the same language, and sometimes I'm not so sure.

I'm doing fine. We have a new building with nice new offices. I'm going
to Australia next week with Sherri and Danny, and after the meeting, will
visit Cairns, Adelaide, and New Zealand. I'm looking forward to skiing
on a volcano, if it stops erupting.

Alan

Prof. Alan Robock Phone: (3xxx xxxx xxxx
Department of Meteorology Fax: (3xxx xxxx xxxx
University of Maryland Email: alan@xxxxxxxxx.xxx
College Park, MD 20xxx xxxx xxxx http://www.meto.umd.edu/~alan

On Thu, 11 Jul 1996, Phil Jones wrote:

> Alan,
> Thanks for the quick response. We'll expect something from Melissa
> in the next few weeks. I also hope our copy of the 2cnd edition arrives
> soon. In our maximum latewood density reconstruction from the polar Urals
> to AD 914, the most anomalous summer is AD 1032. A lot of other volcano
> years are there with summers of -3 to -4 sigma such as 1816,1601,1783 and
> 1453 (I think this later one is Kuwae that is being found in the Ice Cores
> in the Antarctic. However 1032 is 6 sigma and it may be the Baitoushan
> event which you say is 1010 +/- 50 years or the Billy Mitchell event.
>
> I hope all's well with you.
>
> Cheers
> Phil
>
> PS Britain seems to have found it's Pat Michaels/Fred Singer/Bob Balling/
> Dick Lindzen. Our population is only 25 % of yours so we only get 1 for
> every 4 you have. His name in case you should come across him is
> Piers Corbyn. He is nowhere near as good as a couple of yours and he's
> an utter prat but he's getting a lot of air time at the moment. For his
> day job he teaches physics and astronomy at a University and he predicts
> the weather from solar phenomena. He bets on his predictions months
> ahead for what will happen in Britain. He now believes he knows all
> there is to know about the global warming issue. He's not all bad as
> he doesn't have much confidence in nuclear-power safety. Always says
> that at the begining of his interviews to show he's not all bad !
>
> Cheers Again
>
> Phil
> Dr Phil Jones
> Climatic Research Unit Telephone +44 xxx xxxx xxxx
> School of Environmental Sciences Fax +44 xxx xxxx xxxx
> Norwich Email p.jones@xxxxxxxxx.xxx
> NR4 7TJ
> UK
> ---
>
>

Original Filename: **837197800.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: Keith Briffa <k.briffa@xxxxxxxxx.xxx>
To: Neil Loader
Subject: Cambridge details

Date: Fri Jul 12 14:56:xxx xxxx xxxx

>Date: Fri, 12 Jul 1996 12:05:15 +0100
>To: "Tatiana M. Dedkova" <tatm@xxxxxxxxx.xxx>
>From: Keith Briffa <k.briffa@xxxxxxxxx.xxx>
>Subject: Cambridge details
>Cc: Neil Loader
>
>At 08:47 17/06/96 +0500, you wrote:
>>Dear Keith,
>> I have bought the tickets from Moscow to London and back. My arrival
>>to London (Heathrow Airport) is by flight SU 245 (Aeroflot Company) on
>>July 19. Departure from Moscow is at 20.10 (local time), arrival to London
>>is approximately at the same local time. As I know, Evgeny Vaganov did not
>>bay tickets until now, but he informed of my dates and can bay tickets the
>>same flights. My depature from London to Moscow is on August 1 by the
>>Aeroflot Company flight SU 244 at 09.00 of local time.
>> Please, inform me how can I arrive at Cambridge from London? Is
>>there the program of this meeting? We must be ready to do some reports?
>>For example, I can prepare a report about the progress in developing the
>>Yamal supra-long chronology and together with Evgeny about dendroclimatic
>>investigation in the Ural-Siberian subarctic.
>> Rashit Hantemirov and Alexander Surkov will go soon to the Yamal
>>peninsula (June 24). This summer they want to collect subfossil material
>>from areas which are much more remote and situated at higher latitudes.
>>We hoped to use some money of the ADVANCE project. But we have not received
>>this money until now and the program of collecting during this summer will
>>be reduced.
>> Some days ago I received an information that the INTAS-RFBR project
>>was rejected. The competition was very high.
>>
>> Sincerely yours Stepan Shiyatov
>>
>>
>>
>>
>>
> Dear Stepan ,
> I have sent your message on to Neil Loader who is organising
>the logistics for the Cambridge meeting. By the time you arrive you could
>still get the underground to London and take a train to Cambridge. This will take about 3 to 4 hours and so you will not arrive until very late. You may
>wish to stay in a hotel near Heathrow - for the night and take a train in
>the morning. It will not be advisable to go into London and search for a
>reasonable hotel at that time . If you go to information at the airport they
>will arrange for a hotel and courier service to and from the hotel. It is
>best to ask when you arrive. You could also phone me and/or Niel to let us
>know your situation. My home phone number is (01xxx xxxx xxxx). Niel will
>probably give you a contact number in Cambridge. You will need money only for
>your travel and hotel expenses until you get to Cambridge. I will refund this
>and give you additional funds when I arrive on Saturday evening. If you need
>to, you will be able to change money in Heathrow when you arrive.
> Please let me know if any of this is not feasible. Perhaps Neil or
>someone here can book you a hotel room if you decide whether or not to go
>to Cambridge the same night you arrive.
> I will send this message to Neil and he may contact you seperately.
>Let me know your thoughts on this .
> As for the meeting - if you wish to give a presentation on the Urals
>and Taimyr work that would be good. The main reason you are coming is to meet
>everyone and to discuss further work plans - so do not worry about a talk.
> It's up to you. After the meeting I thought you might like to come back
>to my house near Norwich for a day or two or have a holiday in and around
>Cambridge. We can discuss this later. Fritz Schweingruber will not now
>come to Cambridge.
> Thats all for now - I look forward to hearing from you
> best wishes
> Keith
>

Original Filename: **839635440.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: John Daly <daly@xxxxxxxxx.xxx>
To: n.nicholls@xxxxxxxxx.xxx
Subject: Re: Climatic warming in Tasmania
Date: Fri, 09 Aug 1996 20:04:00 +1100
Cc: Ed Cook <drdendro@xxxxxxxxx.xxx>, NNU-NB@xxxxxxxxx.xxx, k.briffa@xxxxxxxxx.xxx, Mike Barbetti
<mikeb@xxxxxxxxx.xxx>, zetterberg@xxxxxxxxx.xxx, rjf@xxxxxxxxx.xxx

Dear Neville,

You mentioned to me some time ago that in your view, the 11-year solar cycle
did not influence temperature. There have been numerous attempts by
academics to establish a correlation, but each has been shot down on some
ground or other. I remember Barrie Pittock was especially dismissive of
attempts to correlate solar cycle with temperature.

Have you tried this approach?

Load "Mathematica" into your PC and run the following set of instructions -

```
data = ReadList[ "c:sydney.txt", Number]
dataElements = Length[data]
X = ListPlot[ data, PlotJoined-> True];
fourierTrans = Fourier[data];
ListPlot[Abs[fourierTrans], PlotJoined -> True];

fitfun1 = Fit[data,{1,x,x^2,x^3,Sin[11 2 Pi x/dataElements],
Cos[11 2 Pi x/dataElements]},x];
fittable = Table[N[fitfun1], {x, dataElements}];
Y = ListPlot[fittable, PlotJoined -> True];
Show[X, Y]
```

The reference to "c:sydney.txt" is a suggested pathname for the following
set of data - which is Sydney's annual mean temperature.

16.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx
xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.4
17.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx
xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.1
16.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx
xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.4
17.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx
xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.5
17.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx
xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.4
17.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx
xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.8
18.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx
xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.4
17.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx
xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.xxx xxxx xxxx.1
18.6

So Far so good.

"Mathematica" first plots out the data itself (see Atachment 1)

The first part of the instruction set lets "mathematica" do a Fourier Transform
on the data, ie. searching out the periodicities, if there are any. The result is
shown on Attachment 2.

The transform result shows a sharp spike at the 11 year point (I wonder
what is significant about 11 years?). The second part of the instructions
now acts upon this observed spike (the Cos 11 bit), to extract it's
waveform from the rest of the noise. The result is shown as a waveform
in attachment 3, the waves having an 11-year period, with the long-term
Sydney warming easily evident.

Attachment 4 shows the original Sydney data overlaid against the 11-year
periodicity.

It would appear that the solar cycle does indeed affect temperature.

(I tried the same run on the CRU global temperature set. Even though CRU
must be highly smoothed by the time all the averages are worked out, the
11-year pulse is still there, albeit about half the size of Sydneys).

Stay cool.

John Daly http://www.vision.net.au/~daly

Attachment Converted: c:eudoraattachSydney.gif

Attachment Converted: c:eudoraattachFourier.gif

Attachment Converted: c:eudoraattachSolar1.gif

Attachment Converted: c:eudoraattachSolar2.gif

Original Filename: **839858862.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: Tom Wigley <wigley@xxxxxxxxx.xxx>
To: dgm@xxxxxxxxx.xxx
Subject: Re: Your help, please?
Date: Mon, 12 Aug 1996 10:07:xxx xxxx xxxx(MDT)
Cc: trenbert@xxxxxxxxxx.xxx, boville@xxxxxxxxx.xxx, branst@xxxxxxxxx.xxx, kiehl@xxxxxxxxx.xxx, francisb@xxxxxxxxx.xxx,
rjcicero@xxxxxxxxx.xxx, covey@xxxxxxxxx.xxx, tom@xxxxxxxxx.xxx, curry@xxxxxxxxx.xxx, pdadd@xxxxxxxxx.xxx,
gates5@xxxxxxxxx.xxx, graumlich@xxxxxxxxx.xxx, dennis@xxxxxxxxx.xxx, barafu@xxxxxxxxx.xxx, tkarl@xxxxxxxxx.xxx,
lindzen@xxxxxxxxx.xxx, liu@xxxxxxxxx.xxx, sloman@xxxxxxxxx.xxx, jm@xxxxxxxxx.xxx, rcm@xxxxxxxxx.xxx,
meehl@xxxxxxxxx.xxx, berrien@xxxxxxxxx.xxx, dickm@xxxxxxxxx.xxx, neelin@xxxxxxxxx.xxx, newell@xxxxxxxxx.xxx,

north@xxxxxxxxx.xxx, obrien@xxxxxxxxx.xxx, peltier@xxxxxxxxx.xxx, rtp1@xxxxxxxxx.xxx, ram@xxxxxxxxx.xxx,
randall@xxxxxxxxx.xxx, erasmu@xxxxxxxxx.xxx, cddhr@xxxxxxxxx.xxx, alan@xxxxxxxxx.xxx, njrosenberg@xxxxxxxxx.xxx,
sarachik@xxxxxxxxx.xxx, schlesin@xxxxxxxxx.xxx, schneide@xxxxxxxxx.xxx, shukla@xxxxxxxxx.xxx, esmith@xxxxxxxxx.xxx,
rsomervi@xxxxxxxxx.xxx, turco@xxxxxxxxx.xxx, waliser@xxxxxxxxx.xxx, wallace@xxxxxxxxx.xxx, walsh@xxxxxxxxx.xxx,
wang@xxxxxxxxx.xxx, "P.D. Jones" <p.jones@xxxxxxxxx.xxx>, drdendro@xxxxxxxxx.xxx, k.briffa@xxxxxxxxx.xxx,
mhughes@xxxxxxxxx.xxx, rbradley@xxxxxxxxx.xxx, Tim Barnett <tbarnett@xxxxxxxxx.xxx>, jfein@xxxxxxxxx.xxx, Ben Santer
<bsanter@xxxxxxxxx.xxx>, dgm@xxxxxxxxx.xxx

Dear Doug,

In response to Jay Fein's e-mail re den-cen, here are some points (which
may merely echo where you are already).

(1) Why study den-cen? Reason is: improve understanding of climate
system to aid in detection and prediction. You should read Ch. 8
(detection) of IPCC WGI SAR in this regard.

(2) How to study den-cen? Models and observed data are equally
important. Models (coupled O/AGCMs) can only give the internal component
of variability, instrumental and paleodata give internal-plus-external.

(3) How useful are paleodata? I support the continued collection of
such data, but I am disturbed by how some people in the paleo community
try to oversell their product. A specific example is the ice core
isotope record, which correlates very poorly with temperature on the
annual to decadal timescale (and possibly also on the century
timescale)---question, how do we ever demonstrate the usefulness or
otherwise of ice core isotopes on this timescale?

There are other well known proxy data issues that need careful thought...

(a) Sedimentary records---dating. Are 14C-dated records of any value at
all (unless wiggle matched)?

(b) Seasonal specificity---how useful is a proxy record that tells us
about a single season (or only part of the year)?

(c) Climate variance explained by the proxy variable--close to zero for
ice core isotopes, up to 50% for tree rings, somewhere in between for
most other indicators. How valuable are such partially explained records
in helping explain the past?

(d) Signal-to-noise problems---a key issue is, what role has external
forcing had on climate over the past 10,000 years. There is a tendency
to interpret observed changes as evidence of external forcing---usually
unjustifiably. Few workers in the area realize that paleo interpretation
has a detection aspect, just like interpreting the past 100+ years---only
much more difficult. More work is needed on this.

(e) Frequency dependence of explained variance---the classic example
here is tree rings, where it is exceedingly difficult to get out a
credible low frequency (50+ year time scale) message. Work in this area
could reap useful rewards.

(f) Coverage---what about den-cen data from the oceans? We need much
more of this, especially in regions that might provide insights into
mechanisms (like NADW changes).

(4) Causes. Here, ice cores are more valuable (CO2, CH4 and volcanic
aerosol changes). But the main external candidate is solar, and more
work is required to improve the "paleo" solar forcing record and to
understand how the climate system responds both globally and regionally
to solar forcing.

I hope these very hasty ramblings are helpful

Cheers,
Tom

P.S. I've added Ben Santer, Tim Barnett, Ed Cook, Keith Briffa, Malcolm
Hughes, Ray Bradley and Phil Jones to your mailing list.

On Thu, 8 Aug 1996, it was written:

> Dear Colleague:
>
> Doug Martinson is the Chair of the NAS, Climate Research
> Committee's Dec-Cen panel. He and his Panelists are drafting a
> Decadal-Century Climate Variability Science Plan (a US CLIVAR
> contribution). Doug and his Panel are trying to get the broadest
> possible scientific input for this Plan. Doug's approach is one
> that I strongly endorse. In this reagrd he asked me to solicite
> your comments on highest priority science questions and asks also
> for some help regarding examples of published work that would be
> useful for the Plan.

>
> I know you are busy, but urge you to think about this and comment.
> Doug's committee meet in mid-September, so to be of most use to
> him, your comments should be received by the end of August.
>
> Please email to Doug with a cc to me.
>
> Doug Martinson: dgm@xxxxxxxxx.xxx
> Jay Fein: jfein@xxxxxxxxx.xxx
>
> Thanks very much. Jay
>

Original Filename: **841293339.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: T.Osborn@xxxxxxxxx.xxx (by way of Tim Osborn <T.Osborn@xxxxxxxxx.xxx>)
To: t.osborn@xxxxxxxxx.xxx
Subject: No Subject
Date: Thu, 29 Aug 1996 00:35:39 +0100

From: CPCMB::Fxxx xxxx xxxxJUL-1995 10:53:56.46
To: MX%"pierce@xxxxxxxxx.xxx"
CC: F055
Subj: Re: Hi and questions

Dave

You're right, smoothing the P-E field is a much bigger change than adding
a bit of noise, or the statistical model feedback. But some papers give the
indication that the strong instability/variability of the thermohaline
circulation under traditional mixed boundary conditions cannot possibly
occur when a more realistic SST condition is used. Yet that's not true
of some current models - e.g.:

- some LSG/EBM configurations still oscillate,
- the Manabe & Stouffer 1988 coupled model had two stable states,
- Mikolajewicz and Maier-Reimer 1994 still could collapse NADW even with a
reduced coupling of 16 W/m**2/K (I note your caveat about the lack of scale
dependence though),
- the Stocker et al 1992 zonally averaged coupled model had multiple
equilibria,
- the OPYC/ECHAM2 coupled run (Lunkeit et al) shows what appears to be a
temporary collapse of NADW.

The answer is that the stability depends on the relative buoyancy forcing of
heat and fresh-water, as you've pointed in both you're papers. Freeing up
the SST increases the stabilising (not static stability, but stability of the
model's state) effect of the heat flux - but doesn't GUARANTEE that it will
be stronger than the fresh-water flux effect. To be realistic, the fresh-water
flux used should ideally be the observed flux - I agree that a diagnosed field
hides model errors. Its similar to the flux correction or no flux correction
dilemna of coupled models - do you want a realistic state with unrealistic
processes, or a possibly unrealistic state with realistic processes. Either
way, the response of the model to perturbations cannot be guaranteed to be
realistic. The best current way is to do both. Then, with luck, the real
world will lie between the two answers obtained.

The SALFLU_EBM file is not readable yet, although it is there.

You have some interesting papers on your WWW page - the Marginal Sea model
looks very innovative. Also, the LSG/EBM experiment with the open Panama
Isthmus shows good results. What P-E forcing field did you use for that run,
and what small-scale coupling coefficient?

Cheers,

Tim Tim Osborn, CRU, UEA, UK

Original Filename: **841418825.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: Keith Briffa <k.briffa@xxxxxxxxx.xxx>
To: Jean-Claude.Duplessy@xxxxxxxxx.xxx (Jean-Claude Duplessy)
Subject: Re:
Date: Fri Aug 30 11:27:xxx xxxx xxxx

At 13:52 27/08/xxx xxxx xxxx, you wrote:
>Dear Keith,
>
> I have been asked to write a white paper on the possibility for the
>paleo community to interact with CLIVAR.
>

> Evidently part of the jow has been made during the Venice meeting,
>but I would like to know if you have somme recent recent work of yours that
>I could include in this paper.
>
> Any suggestion woulmd be welcome.
>
>Best regards
>
>jean claude
>
Dear Jean-Claude

It is good news indeed that these initiatives are now meaningfully
underway to join the palaeo , pure climate , and modelling communities.
I will join the short CLIVAR/PAGES meeting (24/25 Oct.) and a colleague -
Tim Osborn will attend the larger meeting from Oct.28-Nov.1. As for question
about new results , Ed Cook and I have a paper in press describing an
initial attempt to reconstruct a North Atlantic Oscillation index back into
the 1700s using tree-ring chronologies in Europe and North America. I will
have a copy sent to you. Otherwise we have a paper soon to come out in an
American book describing our early analyses of the growing Russian data.
This work, developing the density network is progressing well and we have
some very good reconstructions of growing season degree days- excellent
spatial maps over western siberia going back several hundred years.
We recently published a paper in Nature describing a 1000-year summer
temperature reconstruction in the northern Urals and a brief but interesting
paper demonstrating a strong volcanic influence in the tree-ring density
data when they show extreme low density over large areas. We have very
interesting developments from these areas of work but they are only now
being written up.
The usefull thing to stress is that these researches are in progress
and the development of the tree-ring network is continuing well and is already
providing patterns of past climate variability in northern Europe/Russia
and at a number of special locations- nortern Sweden/Finland, Yamal, and
Taimyr we have already got continuous 2000-year chronologies and have the
potential (indeed we already are) to build xxx xxxx xxxxyear series at ech location.
I will send you some reprints/preprints and an overhead that shows
the present state of the northern chronology network. Any stress on the
importance of future collaboration btween us and the Russians would be wellcome.
I have just heard that a proposal I submitted to Copernicus to do just this
was to my amazement ruled not relevant to the programme!

I look forward to seeing you in October. Very best wishes. Needless to say, if I can offer any help with drafting the white
paper or similar
I am happy to oblige.
cheers
Keith

Original Filename: **842992948.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: Keith Briffa <k.briffa@xxxxxxxxx.xxx>
To: Gary Funkhouser <gary@xxxxxxxxx.xxx>
Subject: Re: russian data
Date: Tue Sep 17 16:42:xxx xxxx xxxx

Gary,
it's great to hear from you. The stuff you are doing is very interesting
to say the least. From the details you give the precip. stuff looks the more
relevant for the Holocene though I note that you don't have a manuscript
yet. The other stuff is of course interesting but I would have to see it
and the board would want the larger implications of the stats clearly
phrased in general and widely understandable (by the ignorant masses) terms
before they would consider it not too specialised. I suspect that this
might not be straight forward. Are you not being (in the time honoured Don
Graybill fashion) too demanding of the response function results when you
say deriving a transfer function is not justified? We all strive for
perfection but does it exist? Seriously , it would be easier as regards
publication policy to get the Editor to accept a reconstruction/reconstruction
based paper than one describing chronology inferences.
I don't know whether this is any use but I hope you'll send us something.
I also hope life going O.K. for you these days. I can't see me getting to
Tucson for some considerable time and I don't suppose you have any plans
for cruising this way so I'll see you when I see you.
keep in touch and let me know what you you decide.
the best to you
Keith

At 16:44 11/09/xxx xxxx xxxx, you wrote:
>Keith,
>How's it going?
>

>I've been working on some of the data that Don collected with
>Shiyatov, Mazepa and Vaganov in the late 80's and I was wondering
>if you thought any of it might be appropriate for The Holocene - or
>if you have any ideas about where we could go with it.
>
>I already have a fair draft dealing with the Kyrgyzstan juniper
>chronologies. Although I wasn't able to get any climatic
>reconstructions out of it, the material has some interesting
>properties similar to some of our long-lived trees in the southwest
>US. For example, autocorrelation in the series increases as a direct
>function of stand elevation, there is a shift from high to low
>frequency variation with increasing elevation, and the
>intercorrelation among the highest elevation stands is greater
>than that for the lower stands.
>
>Maybe this means that the lower altitude sites are responding
>to more local conditions (precipitation), while the higher stands
>are responding to a more regional (temperature) signal. Response
>function analyses with the indices may suggest this, but again,
>it's not strong enough to justify developing a transfer function.
>
>The draft is about 2500 words plus figures and tables. Stepan hasn't
>seen it yet, but I can't imagine that he will change it very much -
>I know that Valeri didn't find any great climate responses either.
>
>There are also 12 chronologies from central and southern Siberia, some
>which are pretty close to Jacoby's Mongolian sites. I was able to
>build 3 precipitation reconstructions - one has about 50% explained
>variance for a May - June season. I haven't composed a draft yet and
>although Gordon's dealing with temperature, a couple of the
>chronologies are of comparable length and I want to look at our
>low frequency variation relative to his.
>
>Jeff Dean and I are headed to the White Mountains this Friday for
>a little 5-day collection trip. Thanks for your time, Keith.
>
>Cheers, Gary
>Gary Funkhouser
>Lab. of Tree-Ring Research
>The University of Arizona
>Tucson, Arizona 85721 USA
>phone: (5xxx xxxx xxxx
>fax: (5xxx xxxx xxxx
>e-mail: gary@xxxxxxxxx.xxx
>

Original Filename: **842996314.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: Keith Briffa <k.briffa@xxxxxxxxx.xxx>
To: Mike Salmon <m.salmon@xxxxxxxxx.xxx>
Subject: Re: shiyatov
Date: Tue Sep 17 17:38:xxx xxxx xxxx

Dear Stepan
I have received the receipts. Thankyou.
Unfortunately I have also heard that our request to COPERNICUS was not
successfull. I am very disappointed about this. The letter I recieved
said that the proposal " was not considered relevant" so you can imagine
that I am seriously exploring what this is all about. I have just returned
from a PEP3 meeting in Paris . I tried to emphasise how important the Russian
work is and , of course , our collaboration. I am relly angry that our proposal
was not considered by referees - just rejected by the committee.
Thanks for the piece for the Web page - It is already on. It is now
more important than ever that we publish some papers over the next few months
on the different aspects of the network reconstructions and the long series.
Have you considered my suggestion to think about a long,detailed paper on
the Yamal work for submission to The Holocene? I am happy to help as much
as possible with such an effort. I am glad you are safely home and I send
my best wishes to you all.
Keith

Original Filename: **843161829.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: Gary Funkhouser <gary@xxxxxxxxx.xxx>
To: k.briffa@xxxxxxxxx.xxx
Subject: kyrgyzstan and siberian data
Date: Thu, 19 Sep 1996 15:37:xxx xxxx xxxx

Keith,

Thanks for your consideration. Once I get a draft of the central
and southern siberian data and talk to Stepan and Eugene I'll send
it to you.

I really wish I could be more positive about the Kyrgyzstan material,
but I swear I pulled every trick out of my sleeve trying to milk
something out of that. It was pretty funny though - I told Malcolm
what you said about my possibly being too Graybill-like in evaluating
the response functions - he laughed and said that's what he thought
at first also. The data's tempting but there's too much variation
even within stands. I don't think it'd be productive to try and juggle
the chronology statistics any more than I already have - they just
are what they are (that does sound Graybillian). I think I'll have
to look for an option where I can let this little story go as it is.

Not having seen the sites I can only speculate, but I'd be
optimistic if someone could get back there and spend more time
collecting samples, particularly at the upper elevations.

Yeah, I doubt I'll be over your way anytime soon. Too bad, I'd like
to get together with you and Ed for a beer or two. Probably
someday though.

Cheers, Gary
Gary Funkhouser
Lab. of Tree-Ring Research
The University of Arizona
Tucson, Arizona 85721 USA
phone: (5xxx xxxx xxxx
fax: (5xxx xxxx xxxx
e-mail: gary@xxxxxxxxx.xxx

Original Filename: **844968241.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: "Tatiana M. Dedkova" <tatm@xxxxxxxxx.xxx>
To: k.briffa@xxxxxxxxx.xxx
Subject: Rashit
Date: Thu, 10 Oct 96 13:24:01 +0500

Dear Keith,
enclosed are data concerning Yamal chronology.
1 - list of samples: 139 subfossil samples (checked only),
covered time span from about 350 BC and 18 samples from living
trees (jah- from Yada river, m- and x- Hadyta river, por- from
Portsa river);
2 - general chronology (1248 BC - 1994 AD). I have some little
doubt about 360 BC - may be it is false. It was found that
in chronology I sent you before 155 BC was false ring;
3 - ring widths of living trees from Yada and Hadyta;
4 - ring widths of living trees from Portsa. Some of them didn't
include in chronology, because were not measured at that time;
5 - ring widths of subfossil trees. Zero means that ring didn't
find on sample.
I don't send description of collection sites, deposits and etc.
for the present. Some details you can find in our article
(Shiyatov,...., Loosli). By the way, do you know something about
its fate?
Please, inform me if you have any questions about these data.
Sincerely yours,
Rashit Hantemirov

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M2-DY.;&XX@^*3R2'YFWPC[YHY4W.+.6@K_133AH"H$$$I$R4WH;6F;;OID[>V@`[J0YFRP
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M1SX>W'[KP%]O>$U'+^L^G5&)Z2]W)O&`8^IQ?6'`$&G(P7GC]^>F[?J^G:_G$

Original Filename: **845217169.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: Fred Pearce <100713.1311@xxxxxxxxxx.xxx>
To: keith briffa <k.briffa@xxxxxxxxxx.xxx>
Subject: new sciwentist feature
Date: 13 Oct 96 10:32:49 EDT

Keith,

This is my first draft of the dendrochronology feature. I wonder if you have time to go through look. I hope you recognise the quotes, but please makes changes if they think they misrepresent you. And if you can answer any of the questions in square brackets that would be most useful.

Ideally, can you not change the full text but make notes, remarks, answers referring to it.

As ever, haste is of the essence.

Regards

--Fred Pearce

It was one of the largest volcanic eruptions of the past xxx xxxx xxxxyears. Mount Changbai [correct?] in China blasted 50 cubic kilometres of rock into the air and deluged much of the far east with hot pumice. Radiocarbon dated the explosion at early in the 11th century. But it took Keith Briffa, sitting in his office in Norwich and juggling data from tree rings round the world, to pinpoint the precise year: 1032.

Volcanoes scatter the atmosphere with dust that deflects sunlight and cools the world beneath for a year or more. And when

the world cools, trees grow less. That year's growth rings are smaller and less dense.

By analysing those rings, Briffa and his colleagues at the Climatic Research Unit in the University of East Anglia have charted these sudden and dramatic shocks to the climate system, from Changbai to Pinatubo in 1991. Larches in the forests of the northern Urals, for instance, have revealed that 1032 was the coldest summer there in a thousand years, more than 6 degrees cooler than the long-term average. Four of the five coldest summers in Europe and North America during the past four centuries (1601, 1641, 1669, and 1912) coincided with known major volcanic events. "We are pretty certain the fifth one, in 1699, did too," says Briffa. "But the geologists haven't found the volcano yet."

It is clever work. But the science of tree-ring analysis, dendrochronology, is more than just a party piece for botanists. Every ring in every tree round the world contains a memory of the climate the year it was formed. Reading these rings holds the potential, Briffa believes, to answer one of the most vital questions of our time: has human activity started to warm the planet?

With colleagues in laboratories and field stations from Dublin to eastern Siberia, he has within the past year [correct?] begun an attempt to construct a history, year by year, of temperatures across northern Europe and Asia over the past xxx xxxx xxxxyears, right back to the waning of the last ice age. The tam, funded by the European Union, hope to help show whether the warming seen across the planet in the past century, and especially since around 1980, is within the limits of normal natural variability, or the start of man-made global warming.

For climatologists, the search for an irrefutable "sign" of anthropogenic warming has assumed an almost Biblical intensity. The leading figures of the UN's Intergovernmental Panel on Climate Change (IPCC), claim that, in all probability, they have seen it. Last summer [ed: 1996], the IPCC's scientific working group, chaired by former UK Meteorological Office boss Sir John Houghton, concluded that "the balance of evidence suggests a discernible human influence on global climate". But it is like the "balance of evidence" suggesting BSE causes CJD. The judgment is far from "beyond reasonable doubt". The case remains "not proven".

Many researchers most intimately involved in the search are still far from sure how the probabilities balance. And some of the sharpest concerns are coming from the places where the original early warnings of global warming emerged in the mid-1980s. Places such as Briffa's base at the Climatic Research Unit in Norwich, and the Scripps Institution of Oceanography in California.

Few investigators doubt that the world has warmed recently. Nor that the enhanced "greenhouse effect" of pollution from gases such as carbon dioxide, will warm the planet. But in the past five years, climate researchers have growing increasingly aware of how little they really know about the natural variability from which they must pick out the "signal" of human influence.

One prominent IPCC researcher concerned about this gap in knowledge is Simon Tett from the Hadley Centre for climate modelling at the Meteorological Office, home to one of the world's five leading global circulation models, capable of recreating a mathematical version of how the atmosphere works and of running simulations of climatic changes over decades or even centuries. He says that "in the past, our estimates of natural variability have been based on climate models." But this autumn [date?], he says, those estimates have been thrown into turmoil by a paper published in the journal The Holocene. In it, Tim Barnett of the Scripps Institution of Oceanography, part of the University of California at San Diego, compared model estimates of natural temperature fluctuations over the past 400 years with the best evidence from the real world -- from instruments in the past century and "proxy data", such as Briffa's tree rings, from before that.

The result was bad news for the modellers. The two models examined -- one German, the other American -- generated a natural variability of around 0.1 degree C per century. This was less than half that revealed in the proxy data. "Of course we don't have to believe the proxy data. They certainly have problems attached to them. But my belief is that they both models, and proxy data too, underestimate real variability," says Barnett

The models' error was not, perhaps, too surprising. As Barnett points out, they do not include vital "forcing" mechanisms that alter temperature, such as solar cycles and volcanic eruptions. Nor can they yet mimic the strength of the largest year-on-year variability in the natural system, the El Nino oscillation in the Pacific Ocean, which has a global impact on climate.

Nonetheless, the findings should serve as a warning, Barnett says, that "the current models cannot be used in rigorous tests for anthropogenic signals in the real world". If they are they "might lead us to believe that an anthropogenic signal had been found when, in fact, that may not be the case."

Barnett knows how easily this can happen. He was a lead author for a critical chapter in the last IPCC scientific assessment, which investigated "the detection of climate change and attribution of causes". It formulated the IPCC case that the evidence points towards a human influence on climate, but it warned repeatedly that great uncertainties remained. "We wrote a long list of caveats in that chapter. We got a lot of static from within IPCC, from people who wanted to water down and delete some of those caveats. We had to work very hard to keep them all in." Even so, when the findings were first leaked to the New York Times, it was under the headline "Scientists finally confirm human role in global warming".

Suggestive though the evidence may be, Barnett and his co-authors insist that the uncertainties, especially concerning natural variability, have to be answered. And so, suddenly, the modellers are queuing at Briffa's door to find out what his tree-ring data shows about the real world beyond the computer simulations. "Five years ago, climate modellers wanted nothing to do with the palaeo community," says Briffa with a grin. "But now they realise that they need our data. We can help them to define natural variability." He has already collaborated with Barnett. Tett paid his first visit to the dendrochronology lab in November [1996].

And so to the forests of Europe and Asia where, over the next [how many?] years Briffa will coordinate the work of colleagues in a dozen countries who hope to dramatically increase the available proxy data on past climate change. Much of the best data so far has come from the forests round Lake Tornetrask, on the northern border of Sweden, deep inside the Arctic Circle. This is near the northern limit for Scots pine, a place where their growth rate of the trees can be massively altered by small perturbations in summer temperatures. The result is dramatic differences in the thickness and density of tree rings.

The head of this work is Professor W [full first name?] Karlen [ed: acute on e], a geographer at the University of Stockholm, who over many years has taken cores from living trees and from logs and stumps hauled from old peat bogs. Despite the harsh climate, there are living trees here up to 600 years old. And the chronology can be extended ever further by analysing the dead trees. So far the climate reconstruction is complete for more than 1400 years before the present; the aim now is to extent it up to 8000 years.

The best data, says Briffa, comes from analysing both ring width and the maximum density of wood in each ring. By firing X-rays through the wood, researchers can now analyse the density of rings as little as 30 microns across -- the equivalent of a

tree's girth growing by a centimetre every century. The growth of cell walls late in the growing season creates the densest wood and, says Briffa, "appears to depend directly on the average mean temperature".

Even so, ring growth is a product of many factors, including the genetics of the tree, past climate, the age of the tree and soil moisture. The relationships between ring growth and summer temperature are not a precise. But comparisons between the recent rings and known climatic data show that the rings can capture at least half of the summer temperature variability.

The temperature graphs produced at Tornetrask show "pronounced variability on all timescales, from year-on-year variations right up to century-on-century," says Briffa. On the longer timescales, for instance, they show 20 major cooling periods during the past two millenia, including long spells between 500 and 850, between 1100 and 1350 and between 1580 and 1750, the little ice age. There were also long warm spells between 900 and 1100, known as the medieval warm period, and 1360 to 1560. [ed: show graph from NERC paper].

Further back, early results suggest a strong warm era from 4000 to 3300 BC, and a cool period ending around 5070 BC. But there are intriguing gaps, for which no tree rings can be found. These, says Briffa, "suggest some major calamity that destroyed trees. Volcanoes, perhaps, or a rapid rise in the water tables." A 19-year gap between 1130 and 1111 BC, for instance, coincides with volcanic ash showing up in Greenland ice.

"What all this means," says Briffa, "is that the old image of the xxx xxxx xxxxyears since the end of the last ice age -- the Holocene era -- as climatically tranquil looks increasingly inaccurate." Hence the intense interest in the EU project, which will attempt to reconstruct those xxx xxxx xxxxyears of climate right across northern Europe and Asia, from Ireland to the Sea of Okhotsk, from the borders of Mongolia to shores of the Arctic Ocean.

During the past summer, helicopters flying low over the tundra have spotted logs in hundreds of small lakes in the Tornetrask region of northern Sweden. Karlen has donned his diving suit to help remove samples of timber from the freezing waters [did he?]. In northern Finland, local diving clubs picked some 3000 samples from lakes.

In the Arctic wastes of northern Siberia, a major survey is being conducted on the Taimyr peninsula, the largest stretch of frozen tundra in Eurasia and far north of today's tree line. There are well-preserved logs buried in river sediments here that grew between 5000 and 8000 years ago. On the Yamal Peninsula, just east of the Ural mountains on the shores of the Arctic Ocean, wood dug from the permafrost grew in conditions so cold that some summers temperatures never exceeded the threshold for growth of about 5 degrees C, so no growth rings formed. Nonetheless Yamal is the only site so far found that yields tree rings right through a gap at 300 BC. "Interestingly, the Yamal rings show this to have been the coldest period in the entire run," says Briffa.

Other, less detailed, surveys are being carried out across the whole of the north of the two continents. And this winter the timber is being analysed at laboratories in Copenhagen and Birmensdorf -- the Swiss home of Fritz Schweingruber, one of the world's top tree-ring analysts. The project will also carry out new analysis on the large numbers of samples of ancient oak already stored in laboratories in Ireland, Britain, Germany, Poland, the Netherlands and Sweden. The oak has been dragged from bogs and river beds, or liberated from archaeological sites and even the beams of old houses over the past 30 years.

"There is a massive amount of data on existing European oak rings. But much of it was done in the 1970s, and then not updated," says Briffa. One of Britain's biggest collections, at Sotterley Park near Lowestoft in Suffolk [Keith: who runs this?], has ring data going back to 1580. "But it stops in the 1980s, missing the recent major droughts. We have got to update that information."

Already, the first long data sets are starting to emerge from Siberia. Last summer [ed: 13 July 1995], Briffa, Schweingruber and Stepan Shiyatov of the Institute of Plant and Animal Ecology at Ekaterinburg in the Russian Urals published a paper on "unusual 20th-century summer warmth in a 1000-year temperature record from Siberia". A complete tree-ring chronology from AD 914, pieced together from larches on the Yamal peninsula, suggested that average summer temperatures since 1901 have been higher than for any similar length of time during the chronology. It estimated that from 1600, the depth of the little ice age, to the present day there has been a 1.14 degrees C warming. The first eight decades of the 20th century were 0.13 degrees C warmer than the next warmest period, nine centuries before in1202-91.

The chronology also showed that Europe's "little ice age" extended east of the Urals, but that the medieval warm period did not. But these long trends disguise sharp short-term anomalies. The 11th century seems to have been a particularly turbulent time in the Urals. 1032, the year of the Changbai eruption, yielded the coldest summer in a thousand years. But the following year was the second warmest of the millenium, at 2.11 degrees above the mean.

Tree rings are not the only source of proxy temperature data. Layers of ice laid down annually in permanent ice sheets, such as those in Greenland and Antarctica, carry a temperature record in the isotopic composition of the ice. Corals also have a temperature imprint, and even sediments on continental shelves can be mined for climate information. The most work, so far, has been done on ice sheets. American and European researchers in the Greenland Ice Sheet Project (GISP), for instance, have drilled for 3 kilometres into the ice pack, going back more than xxx xxxx xxxxyears. Besides plotting the course of the last ice age, they have found evidence of constant climate shifts during the past xxx xxxx xxxxyears.

Briffa says tree rings and ice cores "complement each other, focusing best at different timescales." Tree rings show annual and decade-to-decade variations very clearly. But they do not go back so far, and are not so good at spotting change from millenium to millenium. Ring analysis seems to smooth out long-term trends, probably because trees slowly adapt to these changes, disguising them." On the other hand, ice-core data shows up long-term trends very clearly, but is poor at showing single-year changes. The melting and refreezing of ice in the surface of ice packs means that the ice from individual years tends to mingle together.

The patterns of temperature change revealed by these different methods will probably always remain too fragmented to reveal unambiguous trends in global average temperatures. But this may not matter. "Frankly, global averages are not central to the issue of attributing climate change," says Barnett. "What will ultimately prove whether or not we are altering the climate will be the patterns of temperature change -- geographical patterns, seasonal patterns and vertical patterns." It is not how much it warms, but where, that will be vital.

Under the IPCC umbrella, Barnett and Phil Jones of the CRU have formed a small "detections group", to look for these tell-tale patterns. "We are systematically looking at the patterns, past and present, of all the main forcings on climate," Barnett says. They will investigate how the world's climate systems respond to volcanoes, to changes in the ocean circulation, to solar cycles and so on. "Then we will compare those patterns with what we are seeing today. What we hope is that the current patterns of temperature change prove distinctive, quite different from the patterns of natural variability in the past." And if that turns out to be the case, he says, "we will be able to close down this issue of attribution, perhaps within three to five years."

Here, the climate models will again come into play. If current climate change also accords with what the models predict from global warming, then the "hand of man" will indeed look to be on the planet's thermostat.

The models all suggest that anthropogenic global warming will show a very distinctive pattern. For instance, they predict that anthropogenic warming will be greatest in the northern latitudes of the great continental land masses, such as Eurasia. And that makes the finding of Briffa's team that summer temperatures in northern Siberia are higher than for a millenium potentially extremely important. And the prospect of further data from this region to confirm that finding so intriguing.

Briffa grins at the prospect. "The trend seems to be accelerating. We are getting reports back from Stepan, our man in the Urals, that it was warmer this spring on the Yamal peninsula there than ever before, and tree growth has been absolutely fantastic. It is a major warming, like nothing seen there for a thousand years -- and it is what the climate models predict." Caution prevails, but the elusive pattern of man-made global warming may just be emerging amid the larch groves on the sunny hills of northern Siberia.

ends

Original Filename: **846715553.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: Keith Briffa <k.briffa@xxxxxxxxx.xxx>
To: tatm@xxxxxxxxx.xxx
Subject: the Yamal data
Date: Wed Oct 30 17:45:xxx xxxx xxxx

Dear Rashit,
As always I seem to have been away bullshiting and politiking in
various meetings for weeks! I try to convince myself that this is of use to us as a dendrochronological community but I am not so sure how much that is really
true these days. I have the data you sent and I had to get someone here to
decode it for me . That is fine now so I would like to try and reformat and RCS it . I will be back in touch soon. Your paper is in review for Denrochronologia.
I am very keen to get a much more detailed paper in The Holocene dealing with
this stuff and I hope you and Stepan will consider this - perhaps for some time in spring next year. Sorry I wasn't in touch sooner. Please give my regards
to Stepan and Valerie.
very best wishes
Keith

Original Filename: **846781264.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: Keith Briffa <k.briffa@xxxxxxxxx.xxx>
To: tatm@xxxxxxxxx.xxx
Subject: the Yamal data
Date: Thu Oct 31 12:01:xxx xxxx xxxx

Dear Rashit,
In looking at the data I now see that you have only sent data from abot 350bc onwards. What is the situation with the earlier data. I am very interested in the details of the 1st millennium B.C. and especially this period from about
500 to 100 B.C. We still have a gap in the Tornetrask data at about 350 B.C.
I was of the opinion that this period was very low growth in the chronology of yours shown by Stepan in Cambridge - but it does not seem so low in the
chronology he gave me. What are your thoughts on this and is it possible to get the earlier data when you are happy with them?Thanks very best wishes
Keith

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From: Keith Briffa <k.briffa@xxxxxxxxx.xxx>
To: Eugene Vaganov <evag@xxxxxxxxx.xxx>
Subject: Re: message from Vaganov
Date: Tue Nov 12 17:36:xxx xxxx xxxx
Cc: tatm@xxxxxxxxx.xxx

Eugene
I have not received my copy of the book. A message to Malcolm is
the best idea. I have been experimenting with the Yamal data mostly trying
to fit RCS curves - and am finding problems with recent local chronologies
behaving oddly - i.e. too much growth in recent years makes it difficult to
derive a valid age/growth curve. I have produced a rcs standardised curve
for taimyr and will fax a copy to you. I will send comments to you and stepan
on the two papers reviewed for dendrocronologia on the development of the yamal
and taimyr chronologies. I have made major changes to the tracheid paper and need to type and send the new version to you
- also there are problems understanding some bits - I will ask specific questions. How goes the organisation of the
Krasnoyarsk meeting?
Stepan /Rashit I have had some comments on the Yamal paper that I will try to
email tommorow.
best wishes
Keith

At 13:41 12/11/96 +0000, you wrote:

>Dear Keith
> How are you? Did you receive the material
>(chronologies on Siberian subarctic) from
>Stepan? Several days later I'll send to you
>some additional data (several samples) on
>Taymir supra-long chronology, which make
>more deep in sampling the interval around
>xxx xxxx xxxxyear.
> There are a few questions to you.
>1. The volume of "Radiocarbon" with proccedings
> reach Krasnoyarsk with some months delay, so
> can you send me by fax (007)(3912xxx xxxx xxxx
> the content of volume (only for references)?
>2. What about the draft of paper which I gave
> you in Germany (paper concerning the compa-
> rison of tracheid dimension, cell wall thickness
> and density)?
>Best wishes,Gene.
>

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From: gjjenkins@xxxxxxxxx.xxx
To: p.jones@xxxxxxxxx.xxx, deparker@xxxxxxxxx.xxx
Subject: 1996 global temperatures
Date: Fri, 22 Nov 1996 11:23 +0000 (GMT)
Cc: llivingston@xxxxxxxxx.xxx, djcarson@xxxxxxxxx.xxx, ckfolland@xxxxxxxxx.xxx

Phil

Remember all the fun we had last year over 1995 global temperatures,
with early release of information (via Oz), "inventing" the December
monthly value, letters to Nature etc etc?

I think we should have a cunning plan about what to do this year,
simply to avoid a lot of wasted time.

I have been discussing with David P and suggest the following:

1. By 20 Dec we will have land and sea data up to Nov

2. David (?) computes the December land anomaly based on 500hPa
heights up to 20 Dec.

3. We assume that Dec SST anomaly is the same as Nov

4. We can therefore give a good estimate of 1996 global temps by 20
Dec

5. We feed this selectively to Nick Nuttall (who has had this in the
past and seems now to expect special treatment) so that he can write
an article for the silly season. We could also give this to Neville
Nicholls??

6. We explain that data is provisional and how the data has been
created so early (ie the estimate for Dec) and also

7. We explain why the globe is 0.23k (or whatever the final figure is)
cooler than 95 (NAO reversal, slight La Nina). Also that global annual
avg is only accuirate to a few hundredths of a degree (we said this
last year - can we be more exact, eg PS/MS 0.05K or is this to big??)

8. FROM NOW ON WE ANSWER NO MORE ENQUIRIES ABOUT 1996 GLOBAL TEMPS BUT
EXPLAIN THAT IT WILL BE RELEASED IN JANUARY.

9. We relesae the final estimate on 20 Jan, with a joint UEA/MetO
press release. It may not evoke any interest by then.

10. For questions after the release to Nuttall, (I late Dec, early
Jan) we give the same answer as we gave him.

Are you happy with this, or can you suggest something better (ie
simpler)? I know it sound a bit cloak-and-dagger but its just meant to
save time in the long run.

Im copying this to DEP and CKF also for comments.

Cheers

Geoff

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From: Wolfgang Cramer <cramer@xxxxxxxxx.xxx>
To: Mike Hulme <m.hulme@xxxxxxxxx.xxx>, VXT_COPR@xxxxxxxxx.xxx (I. Colin Prentice)
Subject: Re: EU proposals
Date: Fri, 22 Nov 1996 15:51:36 +0100

Hm, clearly coordination between the two (if it really ends up as two)
groups is absolutely essential, otherwise we would look entirely
stupid. The first thing that comes to my mind is that nitrogen would be
emphasizing a component of our overall idea which otherwise would not
receive great attention - hence it could be, perhaps, amalgamated. They
probably see it the other way around: In their problem, climatic
variability comes second in importance. My view on this is that all of
our model intercomparisons have shown that models essentially do crazy
things with interannual variability, simply because nobody ever has
tested them for that in any detail. Esser's model would probably be the
last candidate to use here, since it is "less mechanistic" than any of
the others - in fact, Colin and I seemed to agree to "not necessarily"
include it into this proposal. These are just some thoughts for the
moment.

I just finished a very first, rough draft of our outline, and I attach
it to the end of this message. I have just sent it to Martin Heimann,
but I have still not yet talked to him. I also send this whole thing to
Colin, hoping that he will catch the thread through it without
problems. Gerard Dedieu is the one I want to approach next - Alberte is
already talking to him about this in the context with other things.

Cheers,

Wolfgang

On Nov 22, 14:12, Mike Hulme wrote:
> Subject: EU proposals
> Wolfgang,
>
> This email (see below) has just arrived from Andrew Friend. I wonder
if we
> are in danger of competing amongst ourselves here, or is the role of
N
> sufficiently far away to avoid problems? Do you want me to talk with
Andrew
> again or shall I wait for you to get back to me next week after
contacting
> Martin? Would Gerd Esser be one of 'our' C modellers?
>
> Looking at the call for proposals it seems that 'Theme 1.1.1 Basic
processes
> in the climate system' fits best for us since there is a specific
item (5)
> which states: 'studies of global budgets of greenhouse gases with
> particular emphasis on fluxes, transformations and stroage in the
biosphere,
> lithosphere and oceans.'
>
> If not here, then maybe under '1.1.3 Climate variability, simulation
of
> climate and prediction of climate change' since there is an item (4)
> 'Development, validation and application of models for important
> climate-related quantities such as mean sea-levels, storm and surge
climates
> and carbon cycling.' But here there is an emphasis on European
approaches.
>
> About EU politics, Balabanis is the guy for ESCOBA, but that doesn't
mean he
> is necessarily the one for us. Troen handles a lot of the climate
projects
> in 1.1.1, 1.1.2 and 1.1.3. We have quite a bit to do with him. But
it
> depends if there is someone else on carbon etc. Maybe Balabanis is
the
> place to start.
>
> Regards,
>
> Mike
>
> ***********************
>
> Dear Mike
>
> Thank you very much for your hospitality the other day. I enjoyed my
visit and
> look forward to continued collaboration. With regard to ESCOBA, this
project is

> in domain 1.1 of the Environment and Climate Programme, and is thus
the
> responsibility of Balabanis.
>
> Has there been any progress with regard to a new proposal? I have
contacted
> Gerard Dedieu, and he says that he will have to think about the idea
some more.
> Meanwhile, I have received an invitation from Gerd Esser (another
ESCOBA
> partner) to put together a new proposal to look at 'The role of
nitrogen in the
> carbon balance of the terrestrial biosphere' for submission in
January. A
> couple of the other ESCOBA partners have expressed interest in this
proposal.
> Part of the new project will be to use global process-based carbon
models, such
> as our Hybrid model, to assess the biospheric sink for C (and its
geographical
> distribution) over the period 1750 to 1990. I guess there could be a
role for
> an improved climatology here.
>
> I could investigate further the current intention with regard to
climatology in
> this project if you wish.
>
> Andrew
>
>-- End of excerpt from Mike Hulme

2

Global, spatially explicit assessment of the interannual
variability in terrestrial carbon storage

VERY FIRST, INCOMPLETE draft for a new research proposal
to be submitted to the European Union
for the second phase of the Third Framework "Environment and
Climate"

Goal

A critical uncertainty in assessments of global change impacts
and feedbacks is the source/sink relationship for carbon
between atmosphere and the terrestrial biosphere, and
particularly its interannual variability. Recent advances in
modelling of atmospheric and biospheric processes, combined
with significant progress in data gathering for climate, CO2
and O2, now allow for a dedicated experiment that is likely to
reduce this uncertainty. Equilibrium approaches to the
simulation of global carbon fluxes are no longer adequate for
this, since empirical studies are showing both a long-term
trend and a significant interannual variability of CO2 fluxes,
which appear to be most strongly driven by climatic impacts on
terrestrial vegetation.
Experimental design

For a time period of several decades, we propose to perform a
simulation of biospheric carbon fluxes using:

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From: "Tatiana M. Dedkova" <tatm@xxxxxxxxx.xxx>
To: k.briffa@xxxxxxxxx.xxx
Subject: from Rashit
Date: Mon, 9 Dec 96 14:19:37 +0500

Dear Keith,
we received your letters concerning our paper for Dendrochronologia
and three long chronologies.
1. As regards individual ring width data of living trees from
Yamal we would remind you that you have them. Stepan gave to you
in England one diskette. There are data for Larix sibirica from
three sites (KHA - from Khadyta river, 67812'N 69850'E; JAH -
from Yahody river 67807'N 69854'E and POR - from Portsa river
67827'N 71800'E) and for Picea obovata from two points (SCH -
Shtshutshya river 66849'N 69850'E and KHD - from Khadyta river
67807'N 69854'E).
2. We would be very gratefull if you can do some corrections and
additions in the paper for Dendrochronologia. We did not quite
understand what we have to do on missing rings? Just enumerate
years when missing rings occur? If so, these are following years:

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We have to note that frequency of missing rings on increment
cores of living trees higher, because on samples of subfossil
trees we try to find this kind of rings on whole disc.
Some periods are notable for missing rings: xxx xxxx xxxxBC, 882 BC,
143 AD, xxx xxxx xxxxAD (especially 640 AD), xxx xxxx xxxxAD, 1453 AD
and beginning of 1800th AD.
3. Stepan ask what about book by Bailey?

Best wishes,
Rashit

Original Filename: **850162662.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: Keith Briffa <k.briffa@xxxxxxxxx.xxx>
To: tatm@xxxxxxxxx.xxx
Subject: the paper
Date: Mon Dec 9 15:17:xxx xxxx xxxx

Dear Rashit and Stepan
Thanks for the message and the missing data info. I will
make some additions and include a plot/list of these missing years. I assume
you don't mind me including your plot of the recent Yamal curve and statistics
about crossdating with Polar Urals. I'll send ammended paper as soon as possible. Thanks for the quick reply. Do you have a
working fax?
best wishes to you all
Keith

Original Filename: **850320678.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: Tim Carter <tim.carter@xxxxxxxxx.xxx>
To: d.viner@xxxxxxxxx.xxx (David Viner - Climate Impacts LINK Project)
Subject: ECLAT 2
Date: Wed, 11 Dec 1996 11:11:18 +0200
Cc: m.hulme@xxxxxxxxx.xxx

Dear David/Mike,

Thanks for sending me the ECLAT 2 proposal. First, let me say that I support
the idea of a continued role for activities co-ordinating and facilitating
the provision of climate change information for EC impacts research and
other related research and policy. ECLAT 2 is one way of achieving this, but
the fact that it is a Concerted Action Initiative imposes some limitations.

The major limitation is that CAIs are not supposed to involve original
research. They are networking activities, with a view on forging research
links and developing new research projects. In my view, there is a need for
a number of targetted research activities on scenario development, that
might be covered by the themes of the workshops you are suggesting in ECLAT
2, but which would be best served by some dedicated research projects. It
really isn't satisfactory to wait until the end of ECLAT 2 before embarking
on research. Many of the key topics are already known, and although research
may be proceeding in some of these areas (especially in downscaling
techniques, scenario development techniques, etc.), what is still lacking is
co-ordination across Europe in the selection and application of climate
change scenarios in impact assessment. In my view, there are two areas in
sore need of targetted research:

(1) A project to analyse all available information from GCMs and historical
data, which will provide some uncertainty bounds on the anticipated future
climate in Europe (by region) for use in policy as well as in impacts
assessment. Such a project should involve GCM groups (interpreting the GCM
outputs), scenario developers (who can apply methods of generalising across
a lot of GCM predictions and emissions scenarios, etc.), and a few impact
analysts, who can advise on suitable scenarios for use in a variety of
applications (entry level or basic scenarios).

(2) A project to develop guidelines for impact analysts on the application
of climate change (and related) scenarios in European impact assessments.
This work would need to be linked closely to any co-ordinated, entry-level
scenarios selected for use in EC projects.

However, unless you have a project proposal in the pipeline at CRU (?) I
don't think there is now time to develop a new proposal to meet the 15
January deadline.

Comments on the draft document:

1. It is unclear to me how Figure 1 relates to the text. The arrows are not
well differentiated in the fax version I have, and the boxes are not explained.

2. Similarly, Figure 2 is also misleading. It implies that there is a large
transfer of information from the CC modelling community to the CC impacts
community, but surely the whole function of the ECLAT SE would be to act as
a filter in this transfer. Note that the title of the figure should be revised.

3. PLEASE REMOVE the reference to ECLAIR - there is no such name! This was a
light- hearted emailed suggestion for ECLAT 2, not for Martin's CA which
doesn't have a name to my knowledge.

4. In the suggested steering committee, I would strengthen the

representation of the impacts community. This could be done by time horizon:
e.g. one hydrologist to cover a range of time periods from sub-daily to
century scale; one forester or soils expert for the long term, one
agriculture person for the medium term (maybe I could represent this
community), desertification/erosion/fire risk person for short to medium
term and/or an integrated assessment person (perhaps three or four persons).
You should try to avoid the group being dominated by GCM'lers (do all GCM
groups have to be represented?)

You might ask Ib Troen if there would be any opportunity to obtain EC
funding BEFORE THE FIFTH FRAMEWORK CALL FOR PROPOSALS for a targetted
research topic, if this was strongly and urgently recommended by a task
group workshop. Might there be special funding from DG XI, ENRICH or the
Environment Agency?

Best wishes,

Tim

****
Dr. Timothy Carter
Affiliation: Agricultural Research Centre of Finland
Postal address: c/o Finnish Meteorological Institute
Box 503, FIN-00101 Helsinki, FINLAND

Tel: +xxx xxxx xxxx
Fax: +xxx xxxx xxxx
Email: tim.carter@xxxxxxxxx.xxx
****

Original Filename: **853426848.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: Richard Warrick <cearsr@xxxxxxxxx.xxx>
To: 'Mike Hulme' <m.hulme@xxxxxxxxx.xxx>
Subject: RE: Scengen and CC:Train
Date: Thu, 16 Jan 1997 10:00:48 xxx xxxx xxxx

Dear Mike,

Thanks for your detailed reply concerning Scengen and CC:Train. I was not
proposing to incorporate Scengen in a major way into the training package,
and I am quite aware of the problems of consistency regarding aerosol
effects, natural variability, etc. Rather, I thought that the training
package would be an excellent way to introduce the existence of Scengen
(and MAGICC) to the Country Teams which are responsible for coordinating
national assessments. (the intention was NOT to provide intensive
technical training in its use -- the country team members are largely
coordinators, not technical climate experts). In this way, when it comes
time to actually carry out the national assessments, Scengen would be
recognised as a major tool for scenario generation and, if appropriate, CRU
could be contacted regarding its application, technical training or
collaboration. You had mentioned to me at the IPCC meeting in London that
one of your major aims was to get Scengen recognised as the "standard" for
scenario generation for impact assessments, and I simply thought I saw a
way of furthering that aim through the CC:Train mechanism.

Given the training programmes that you are currently proposing through
ENRICH and others, I can understand your fears that we might "muddy the
waters". Let me pose the following options; that we

(1) use some hard-copy examples from Scengen;
(2) incorporate a demonstration diskette (do you have one?);
(3) just mention the existence of Scengen;
(4) not mention Scengen at all.

Frankly, I am quite happy with any of these. The part on climate change
scenarios is really only a small bit of the overall V&A training package in
any case.

Good luck with your proposals.

Cheers,
Dick

From: Mike Hulme[SMTP:m.hulme@xxxxxxxxx.xxx]
Sent: Thursday, 16 January 1997 00:45
To: Richard Warrick
Cc: m.kelly@xxxxxxxxx.xxx; tim.carter@xxxxxxxxx.xxx; wigley@xxxxxxxxx.xxx
Subject: Re: Scengen and CC:Train

Dick,

And Happy New Year to you also.

You've posed me a tricky one re. SCENGEN and my answer about it being
incorporated into the CC:Train package as a component tool is going to have
to be 'no'. Let me explain.

We too here have plans to exploit SCENGEN (and MAGICC) in a
training/educational context. I ran a pilot seminar here for UNEP before
Christmas on scenario construction, although this was using the new
WINDOWS/Unix versions of both MAGICC and SCENGEN (MAGICC 2 and SCENGEN 2;
IPCC 1995 compatible) we have re-written. Also, I have just submitted a
proposal (called SPARCCS) to ENRICH in DGXII for a support package for
regional climate change scenarios. This would be a 2-year project with
emissions people, as well as MAGICC, SCENGEN and our new global historic
climatology. I think we have a good chance of funding.

With this background I do not want SCENGEN (and especially the old DOS
version) 'leaking' out into the climate training community at this stage.

I am confirmed in this view by thinking that the complex issues surrounding
scenario creation (and the new IPCC Taskgroup on scenarios for the 3rd
assessment is grappling with these - ask Tim Carter about it) should _not_
be an essential part of a vulnerability/adaptation package. And even if
you
think differently then let me suggest the following: if you think it should
be a minor part then I do not think that you need SCENGEN formally
incorporated; if you think it should be a major part then not only do I
think you are wrong in thinking so, but there is more to the scenario issue
than can be supplied by SCENGEN - for example, you need MAGICC, you need to
consider how you handle aerosols, and you need to think about natural
variability and signal/noise issues.

My feeling is that by all means use SCENGEN within CEARS in thinking about
the training package and coming up with some off-line examples (either
sample scenarios or guided sensitivity), but do _not_ incorporate it in the
package. [By the way SCENGEN does not have imaginery countries!]. If
people want more detailed thinking on scenarios then you could always refer
them to CRU (which is what our speciality is).

I hope you understand my feelings on this - I am not trying to be negative,
but am thinking ahead and about the complexity of the scenario issue. I
have talked with Tim Carter recently at some length about some of these
things so I will copy this correspondence to him.

Good luck with CC:Train anyway and I'm sure you'll come up with something
good.

Regards,

Mike

At 14:41 10/01/97 xxx xxxx xxxx, you wrote:
>Dear Mike,
>
>Happy New Year's Greetings from Downunder!
>
>I have a question for you regarding Scengen that relates to a "training
>package" which CEARS have agreed to develop for CC:Train (under UNITAR).
> CC:Train is currently developing about four such training packages
>pertaining to climate change, of which CEARS has agreed to undertake one,
>on Vulnerability and Adaptation assessment. The V&A and other packages
are
>supposed to be flexible enough to be used under a variety of regional and
>country contexts. These packages build upon existing guidelines and
>manuals (e.g. Carter et al's IPCC Guidelines...) and are designed for
>trainers who will be conducting training workshops for the coordinators of
>national assessments (the CC:Train "Country Teams"). Beginning on 21
>January, Tim Carter will be here for 3 weeks, as will Stephanie Lenhart
>(U.S. Country Studies Program), in order to help with this task. The V&A
>training modules will closely follow the IPCC Guidelines. I have proposed
>developing the package as a kind of role-playing simulation exercise in
>which the participants carry out a mini-assessment for a hypothetical
>country.
>
>One of the major steps in the assessment, of course, is the development of
>climate change scenarios. I thought it would be very effective to use
>Scengen for this purpose, and to make Scengen a component tool of the
>training package. Can I use Scengen for this purpose? One possible
>advantage of doing so is that Scengen could, de facto, quickly become the
>standard method used by various Country Teams in carrying out national
>assessments for UNFCCC reporting (or is this not an advantage?!).
>
>Please advise on how I should proceed.
>

>Best wishes to all at CRU.
>
>Cheers,
>Dick
>
>

Dr Mike Hulme tel: xxx xxxx xxxx
Climatic Research Unit fax: xxx xxxx xxxx
School of Environmental Sciences email: m.hulme@xxxxxxxxx.xxx
University of East Anglia web site: http://www.cru.uea.ac.uk/~mikeh/
Norwich NR4 7TJ

Mean temperature in C.England during 1996 was 0.3degC below the 1xxx xxxx xxxx
average.
The maximum temperature in Norwich: Tuesday 13 January: 9.1degC.

Original Filename: **854306192.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: druid@xxxxxxxxx.xxx (Gordon Jacoby)
To: k.briffa@xxxxxxxxx.xxx
Subject: Russia
Date: Sun, 26 Jan 1997 14:16:xxx xxxx xxxx

Hi Keith:

As you are aware, the situation in Russia is very uncertain with their
unfortunate economic condition, especially science support. There is
interest, hope, and dots on maps showing intent but actual activity is
difficult to judge. In the particular area I am interested in, the Taymyr,
there is no current active tree-ring research going on although it has been
previously sampled and some reports are in preparation. Ed probably told
you that I have submitted a proposal to do work there. My understanding is
that unless there is some external funding support, such as my project,
tree-ring sampling there is in abeyance. Several people, including
yourself, recognize the great potential in the region. From my perspective
it seems that the Polar Urals are being studied, Yokutia to the far east is
being studied, some work has been done by Szeicz and Macdonald at the Lena
but there is need for more intensive effort in Taymyr. I would like to hear
your perspective on the situation.

In a related topic, I am thinking of using the option in Ed's new ARSTAN to
use the regional standardization method. In Russia and other locales the
establishment of trees is episodic. In particular, in Alaska Glenn Juday
has data showing cohort groups being established in favorable times. In
Taymyr also, the establishment of trees is not evenly distributed through
time. There are times of growth and times of demise. This concerns me as it
could affect the development of a regional curve. do you see problems
arising from this?

I am also curious to hear any comments you care to make about my recent
letter to Fritz Schweingruber. He obviously will pursue any style of
sampling and analyses he chooses to. My only contention is that he should
not represent his data as the definitive tree-ring information,
particularly ring-width data. His opinions are influential but there is an
accumulating body of ring-width data that clearly shows him to be missing
much important information with his style of sampling. Scientists and
others should be aware of this fact.

Cheers, Gordon

Original Filename: **857600338.txt** | **Return to the index page** | **Permalink** | **Later Emails**

From: Arnulf GRUBLER <gruebler@xxxxxxxxx.xxx>
To: naki@xxxxxxxxx.xxx, becon@xxxxxxxxx.xxx, ja_edmonds@xxxxxxxxx.xxx, hm_pitcher@xxxxxxxxx.xxx,
Fewewar@xxxxxxxxx.xxx, t-morita@xxxxxxxxx.xxx, rob.swart@xxxxxxxxx.xxx, alcamo@xxxxxxxxx.xxx,
knut.alfsen@xxxxxxxxx.xxx, kennethgregory@xxxxxxxxx.xxx, akimoto@xxxxxxxxx.xxx, amann@xxxxxxxxx.xxx,
Jean-Paul.Hettelingh@xxxxxxxxx.xxx, m.hulme@xxxxxxxxx.xxx, schlesin@xxxxxxxxx.xxx, streetsd@xxxxxxxxx.xxx,
wagner@xxxxxxxxx.xxx
Subject: sulfur discussion paper
Date: Wed, 05 Mar 1997 17:18:58 +0000

Sulfur Emissions in New IPCC Scenarios

Arnulf Gruebler, IIASA

SUMMARY OF PROPOSED ACTIVITIES

1. Review and comments of present sulfur discussion paper
2. Revision by sulfur paper lead author
3. Preparation of comparison of regional sulfur scenarios (by lead
author with inputs from other members of writing team and experts)

Timing: August 1997.

4. Specification of minimum and desirable sulfur emission scenario
characteristics and specification (for modeling teams in open process)
5. Establishment of key relationships between sulfur emissions and
other salient scenario driving force variables (income,
technological change environmental, non-GHG policies) using the
simple metric of sulfur to carbon emission ratios.
6. Adoption of specific sulfur control scenarios in conformity with
overall scenario ``storylines''.
7. Distribution of ``template'' sulfur scenarios to selected modeling teams
for assessment of climate and acidification impacts of sulfur scenarios.

Timing: End of 1997.

DISCUSSION PAPER

1. Introduction

The purpose of this discussion paper is to review briefly
the assumptions on sulfur emissions in the IS92 IPCC scenarios,
advances in knowledge and modeling of future sulfur emission
scenarios since IS92, as well as to initiate a discussion on how
to incorporate future sulfur emissions trends into the new IPCC emissions
scenarios. The present draft will be revised based on feedback
received within the members of the IPCC writing team as well as
additional outside experts.

2. Sulfur emissions in IS92

The treatment of sulfur emissions in the IS92 scenarios was
comprehensive. In addition to the dominant energy sector emissions,
also sulfur emissions from industrial processes and land-use changes
(biomass burning) and (a constant flow) of natural sources were
included in the scenarios.

1990 base year values in IS92 were as follows in MtS
(Million tons, or Tg, elemental sulfur; to obtain
weight as SO2 multiply by 2.):

Energy Sector: 65 MtS
Other Industry: 8 MtS
Biomass burning: 2 MtS
Natural: 22 MtS
TOTAL: 98 MtS

These global base year values are well within the range given by
global sulfur emission inventories of 4 to 45 MtS natural sources
and 65 to 90 MtS anthropogenic sources in 1990 (IPCC, 1995:xxx xxxx xxxx).
A comparison of 1990 base year sulfur emission values from a number
of scenarios and integrated assessment models is enclosed as
attachment.

However, as observed in the evaluation of the IS92 scenarios (Alcamo et al.,
1995) regional sulfur emissions assumed in IS92 (e.g. for China) are
much more uncertain. There is for instance up to a factor two difference
between regionalized estimated of global inventories and aggregates of
national and regional emissions inventories. Thus, the good agreement of
base year values of IS92 at the global level masks important differences
and uncertainties at the regional level.

A first important task for the new IPCC scenarios is therefore to update the
regional sulfur emissions baseline values with the results of
latest regional sulfur emissions inventories. Such inventories are available
for Europe through EMEP and CORINAIR, North America (NAPAP), and more
recently also for Asia (e.g. the Worldbank sulfur project, Foell et al., 1995).

Improved modeling of regional sulfur emissions (and deposition, i.e.
impacts) patterns would also require a redefinition of the world
regions as used in the IS92 scenario series. For instance, Canada
is included in the region OECD-Europe, and the IS92 region "South
Asia" includes both the Indian subcontinent as well as Indonesia.
Their important differences in resource endowments lead to different
patterns of sulfur emissions. Their differing predominant weather

patterns and distinct ecosystems lead to differing acidic deposition
patterns and impacts. Both factors preclude their aggregation into
one single regional model. Active inputs from representatives of all
respective modeling communities (regional acidification impacts, regional
climate modelers, energy systems analysts) will be sought on this
issue and lessons learned within EMF activities (M. Schlesinger) on
appropriate sulfur regionalization (6 world regions) will be extremely
valuable.

Concerning future emissions of sulfur the IS92 scenarios project
global anthropogenic emissions of between 150 to 200 MtS by 2050 and
between 140 to 230 MtS by 2100 in the high growth cases, and of around 80-90
and 60 MtS in the two low scenarios (IS92c and IS92d) by 2050 and 2100
respectively. The IS92 scenario evaluation (Alcamo
et al., 1995:xxx xxxx xxxx) concluded that the IS92 scenario series only
partially reflect recent legislation to reduce sulfur emissions (e.g.
the amendments to the Clean Air Act in the US or the Second
European sulfur protocol). Hence, particularly regional sulfur
emissions in OECD countries projected in IS92 are much higher than
more recent scenarios taking account these legislative changes (as
also discussed by IPCC, 1995:xxx xxxx xxxx). For instance the recent
scenarios of the Commission of the European Communities (EC, 1996)
indicate that sulfur emissions by 2020 will be between 64 to 77 percent
below 1990 emissions levels, or between less than 2 to 3 MtS, compared to
8 in 1990. For comparison, the IS92 scenarios project for OECD
Europe (including Canada) sulfur emissions between 8.4 (IS92a and
IS92b) and 11.7 (all other scenarios) MtS by 2020, i.e. between 2 to
30 percent lower than in 1990 (12 MtS).

In addition, integrated assessment models are increasingly able to
model in greater detail driving forces of sulfur emissions as well
as acidification impacts (cf. discussion below). These model
simulations suggest that particularly in Asia acidification impacts
would require substantial sulfur emission control measures already
much earlier than 2050. The resulting global sulfur emissions
are substantially lower than suggested in the IS92 series: typically
in the range between 20 to 80 MtS by 2050 and between 20 to 120 MtS
by 2100. (A comparison of global sulfur emissions scenarios with and
without specific sulfur control assumptions in enclosed as
attachment.)

3. What's New since IS92 (scientific front)

The importance of aerosols including those from sulfur emissions
is by now widely recognized and considerable progress has been made
to quantify their effect on regional climate, both in large GCM
simulations as well as in more simplified integrated assessment models,
e.g. MAGICC's SCENGEN module (needs checking for details with Mike Hulme)
or Michael Schlesinger's work within the EMF (current status:
uncertain). The importance of sulfur emissions as input to climate models
is therefore larger than ever.

As a result of a major World Bank study on acid rain in Asia also
improved national and regional sulfur emissions inventories have
become available (Foell et al., 1995). Improved emissions
inventories outside North America, Europe (including the European
part of the former USSR), and Asia (excluding Oceania, for which
only sparse data seems to be available) have not been made available
since publication of IS92.
As a result, models and scenarios continue to rely on estimates, largely
based on approximate mass and sulfur balance approaches in the world regions
for the Middle East, Southern Africa, and Latin America (cf. discussion of
data availability below).

Similarly, acidification impact models are increasingly being
refined for regions outside OECD in particular for Asia.
Acidification impact studies for unabated sulfur
emissions of coal intensive ``business as usual'' scenarios indicate
exceedance of critical loads of up to a factor 10 already within the
next three to four decades (Amann et al., 1995) with enormous
impacts on natural ecosystems as well as important foodcrops (Fischer
et al., 1996).

Increasingly also energy sector and integrated assessment models
link regional acidification models with simplified climate models
enabling joint analysis of sulfur and climate policies and
impacts. Examples include the IMAGE model (Posch et al., 1996) and the
IIASA integrated assessment model (Rogner and Nakicenovic, 1996) that are
linked with the acidification model RAINS for Europe and Asia, the AIM
(Morita et al., 1994) model for Asia, or ???? for North America.
These models extend earlier energy sector models that dealt with a
comparative costs assessment of isolated sulfur and carbon reductions,
and joint mitigation respectively, such as the OECD GREEN model
(Complainville and Martins, 1994). The state of knowledge of joint
benefits of sulfur and carbon emission reductions was reviewed in

the 1995 IPCC WG III report (IPCC, 1996: xxx xxxx xxxx) and is expanding
rapidly.

4. Data requirements

The most obvious data requirements concern of course
comprehensiveness of sulfur emissions by major source category
(anthropogenic and natural, energy sector and other industrial sources).
Here the data model of the IS92 scenarios appears appropriate and would only
require a reassessment in view of most recent data concerning regional
emissions (particularly in China, where data uncertainties seem
largest).

A more difficult question concerns spatial disaggregation.
Independent from the question of which formal models are being used
to check for scenario consistency, the outmost spatial detail
currently in driving force models with global coverage available is
at the level of world regions (typically around 10, but going up to
around 20 world regions). Both climate as well as acidification
models require inputs at finer spatial resolution. It is unclear at
present what would constitute a ``minimum'' or ``desirable'' level
of spatial disaggregation for the variety of user communities of new
IPCC scenarios. Existing model links (like with the RAINS model)
could be used in some regions like Europe and Asia to generate
spatially highly disaggregated sulfur emission and deposition maps
as inputs for climate models and for impact assessment studies (e.g.
for agricultural crop yield models). In their most advanced
versions the model links even incorporate regionalized differential
growth trends and thus improve on the standard practice of
renormalizing base year spatial emission and deposition patterns
linearly with a particular sulfur emissions scenario.

For regions where similar links are unavailable, more simplified procedures
will need to be devised, keeping in mind the overall tight time frame of the
scenario exercise. Two data sets (are there more??) appear available for
regionalized sulfur emission patterns: the Oak Ridge
GAIA data set (spatial resolution: ????) and the Spiro et al. (1992)
data set (spatial resolution: one degree by one degree).

An open (but extremely critical) issue remaining to be resolved is
to identify mechanisms and responsible groups that could provide the
link between the spatial resolution of the new IPCC scenarios sulfur
emissions to whatever final geographical scales required by impact
assessment and climate models.

5. Scenarios and Sulfur Policies

There are two major sets of driving force variable that influence
future sulfur emissions. 1. Level and structure of energy supply and end
use, and 2. degree of sulfur control policies assumed. (Because of the
dominance of energy related sulfur emissions, they should receive
particular attention in the new scenarios. Industrial sources could
be included in the scenarios with much a simpler driving force
model, e.g. coupling to industrial output.)
Ceteris paribus, highest sulfur emissions occur in scenarios of high demand
growth, rapid resource depletion, limited technological change and absence
of sulfur control policies outside OECD countries. In terms of energy
supply structures such scenarios imply a massive use of coal, including
synfuel production. Typical examples would include the IS92e
and IS92f scenarios. Up to ca. 2050 sulfur emissions in such
scenarios roughly grow in line with fossil fuel use and resulting
carbon emissions, i.e. a roughly constant sulfur to carbon emissions
ratio. Post 2050, still in absence of sulfur control policies,
growth rates of sulfur emissions start to fall short of growth in
fossil fuel use due to the internal technology logic of synfuel
production: synfuel production requires prior coal conversion (e.g.
gasification) and removal of sulfur prior to further conversion,
e.g. to synliquids. Ceteris paribus, therefore sulfur emissions
relative to those of carbon decline.

Sulfur emissions are lower in scenarios with 1. lower demand, 2. more
ample resource availability (especially for natural gas), 3. higher
rates of technological change (especially for non-fossil energy
technologies), and 4. extent and timing of direct sulfur control policies
especially outside OECD countries (itself function of projected impacts like
acidification), and finally, 5. level of other environmental control
measures and valuation of environmental goods (e.g. sulfur emissions are
also lower in scenarios imposing limits on GHG emissions).

Next to environmental impacts and policies, there are also other key
relationships that need to be considered for the formulation of
future sulfur scenarios. For instance, the literature on
environmental Kuznets curves (cf. e.g. World Bank, 1992, or
IIASA-WEC, 1995) argues that with increasing affluence and valuation

of environmental goods, sulfur emissions decline. This hypothesis
is corroborated by both longitudinal and cross-sectional empirical
data. Thus, in the process of industrialization and economic development,
emissions rise initially, pass through a maximum (say at income levels
around 2000 $/capita) and decline thereafter with rising per capita incomes
and the resulting preference of cleaner end-use fuels, valuation of clean
environments, etc.

A scenario taxonomy along the dimensions of demand, resource
availability, and technological change in any case is necessary to
respond to the critique on the IS92 series that these important
driving forces were not varied appropriately to reflect both
uncertainty as well as new scientific knowledge and empirical
evidence. They form part of the overall scenario design process and
the scenario ``storylines'' and need not to be addressed
specifically in the work on sulfur emissions.

Separate ``sulfur stories'' could be developed in addition, based on various
relationships between sulfur emissions and levels of affluence,
industrial structure, etc. within the overall framework of the
scenario ``storylines''. Here sulfur emissions would be part of
other environmental policies (e.g. on water quality, urban traffic
related pollutants, etc.) that form integral part of particular
scenario ``storylines''.

A key variable remains the timing and extent of sulfur control
policies to be assumed for the new scenarios. First of all the
scenarios need to reflect changes in actual policies implemented.
As noted above, IS92 did not take full account of recent
environmental legislation in both North America and the second
European sulfur protocol. Secondly, the sulfur policies to be
assumed, need to reflect recent scientific findings, in particular
the very large local and regional impacts on agricultural crops and
ecosystems of unabated high sulfur emission scenarios, particularly
in Asia. Therefore, all scenarios should assume faster and
deeper reductions in sulfur emissions outside OECD countries than
were assumed for IS92 in light of this recent scientific evidence. The
exact timing and extent of such sulfur reduction measures could then
be scenario dependent. Also no specific reference to individual
policy measures would need to be made (to avoid normative policy
elements, or recommendations, in the scenarios), as reduction
profiles could be adopted from existing sulfur reduction scenarios
in the scientific literaursement by UE (Action COST) for the lecturer, but for this I hope to
>have an answer as soon as possible.
>
>Thank you for your answer
>
>Best regards
>
>I'm Bernardo Gozzini and I work with Marco Bindi in the organisation of this
>seminar because Marco in the next week will leave for USA for two months and
>he cannot follow it
>***
>Bernardo Gozzini
>Ce.S.I.A.-Accademia dei Georgofili
>Piazzale delle Cascine, 18
>50144 FIRENZE ITALIA
>
>tel: 39 xxx xxxx xxxxxxx xxxx xxxx/ 354897
>fax 39 xxx xxxx xxxxxxx xxxx xxxx
>e-mail: gozzini@xxxxxxxx.xxx
>***
>
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