CRU CORRESPONDENCE
##

210. 0981068343.txt
##

From: Martin Welp <Martin.Welp@pik-potsdam.de>
To: gberz@minichre.com, juergen.engelhard@rheinbraun.de, schlueter@mwv.de, gerd-rainer.weber@gvst.de, zimmermeyer@vda.de, jan.rispens@greenpeace.de, guentherr@wwf.de, gretz@mail1.tread.net, siegfried.jacke@dlr.de, paul.bergweiler@dlr.de, kohl.harald@bmu.de, klaus.hasselmann@dkrz.de, schellnhuber@pik-potsdam.de, Carlo.Jaeger@pik-potsdam.de, tol@dkrz.de, ccarraro@unive.it, ola.johannessen@nrsc.no, m.hulme@uea.ac.uk, wokaun@psi.ch, f.gruber@dbu.de, baldur.eliasson@ch.abb.com, sengbusch@dkrz.de, buchner.barbara@feem.it, Ottmar.Edenhofer@pik-potsdam.de, Martin.Welp@pik-potsdam.de
Subject: ECF
Date: Thu, 01 Feb 2001 17:59:03 +0100

Dear friends of the ECF,

Attached I send you:
- An executive summary of the ECF (to be revised anytime on the basis of your suggestions),
- The current version of the ECF "Manifesto" (to be revised anytime on the basis…),
- The minutes of the Amsterdam preparatory meeting of last November.

We do have an URL by now: <www.European-Climate-Forum.net>. We will gradually develop it, please feel free to make suggestions. We also have an internal section, see: <www.European-Climate-Forum.net/internal/>.

Her at PIK, Dr. Martin Welp will take care of ECF logistics for the time being. His e-mail address is: Martin.Welp@pik-potsdam.de.

As for the Logo search, I like these things very much, although I am really not knowledgeable at all. We might make a pre-selection and run a competition on our web-site, inviting cyber-visitors to give their opinion - we will still be free to choose what we like best. Perhaps rather than looking for the single best Logo right now it is more fruitful to identify which proposals find enough appreciation to become part of the web competition.

And let us enjoy these more playful moments without neglecting the key challenge we are faced with: Defining first joint projects and reaching agreements with relevant stakeholders to actually carry them out.

As for the foundation technicalities, we are preparing a background document that we will send out soon.

A final remark on e-mail etiquette: Could we put the string "ECF" in the subject line of all e-mails dealing with ECF, in order to enable our various browser to filter these pearls out of the ocean of e-mails we have begun to live in?

Best regards,
Carlo Jaeger
and Martin Welp

Attachment Converted: "c:\eudora\attach\ECF executive summary.rtf"

Attachment Converted: "c:\eudora\attach\ECF_Jan_01.rtf"

Attachment Converted: "c:\eudora\attach\ECF minutes Amsterdam.rtf"

211. 0981859677.txt
##

From: "John L. Daly" <daly@microtech.com.au>
To: Chick Keller <ckeller@igpp.ucsd.edu>
Subject: Re: Hockey Sticks again
Date: Sat, 10 Feb 2001 21:47:57 +1100
Reply-to: daly@microtech.com.au
Cc: "P. Dietze" <p_dietze@t-online.de>, mmaccrac@usgcrp.gov, Michael E Mann <mann@virginia.edu>, rbradley@geo.umass.edu, wallace@atmos.washington.edu, Thomas Crowley <tom@ocean.tamu.edu>, Phil Jones <p.jones@uea.ac.uk>, sfbtett@meto.govt.uk, daly@vision.net.au, onar@netpower.no, jarl.ahlbeck@abo.fi, richard@courtney01.compulink.co.uk, McKitrick <rmckit@css.uoguelph.ca>, Bjarnason <agust@rt.is>, Harry Priem <priem@dds.nl>, vinmary.gray@paradise.net.nz, balberts@nas.edu, Martin Manning <m.manning@niwa.cri.nz>, Albert Arking <arking@jhu.edu>, Sallie Baliunas <baliunas@cfa.harvard.edu>, Jack Barrett <100436.3604@compuserve.com>, Sonja Boehmer-Cristianse <sonja.b-c@geo.hull.ac.uk>, Nigel Calder <nc@windstream.demon.co.uk>, John Christy <christy@atmos.uah.edu>, cpaynter@greeningearthsociety.org, driessen@global-commpartners.net, dwojick@shentel.net, Myron Ebell <mebell@cei.org>, Ellsaesser <hughel@home.com>, John Emsley <j.emsley@ic.ac.uk>, Jim Goodridge <jdg@mcn.org>, gsharp@montereybay.com, Peter Holle <cog@escape.ca>, Douglas V Hoyt <dhoyt1@erols.com>, "W. S. Hughes" <wsh@unite.com.au>, Wibjörn Karlén <wibjorn.karlen@natgeo.su.se>, kidso@hotmail.com, KIrill Kondratyev <kirill.kondratyev@niersc.spb.ru>, "Dr. Theodor Landscheidt" <theodor.landscheidt@ns.sympatico.ca>, Ross McKitrick <rmckitri@uoguelph.ca>, omcshane <omcshane@wk.planet.gen.nz>, Pat Michaels <pmichael@cato.org>, pbrekke@esa.nascom.nasa.gov, "David M. Ritson" <dmr@SLAC.Stanford.EDU>, robert.balling@asu.edu, Tom Segalstad <t.v.segalstad@toyen.uio.no>, Fred Singer <singer@sepp.org>, Roy Spencer <roy.spencer@msfc.nasa.gov>, Hartwig Volz <Hartwig.Volz@rwedea.de>, Gerd-Rainer Weber <gerd-rainer.weber@gvst.de>, tlowery@ocean.tamu.edu, Rosanne D'Arrigo <druidrd@Ideo.columbia.edu>, k.briffa@uea.ac.uk

Dear Chick & all

> the first is Keith Briffa's rather comprehensive treatment of getting
> climate variations from tree rings: Annual climate variability in
> the Holocene: "interpreting the message of ancient trees", Quaternary
> Science Reviews, 19 (2000) 87-105. It should deal with many of the
> questions people raise about using them to determine temperatures.

Take this from first principles.

A tree only grows on land. That excludes 70% of the earth covered by water. A tree does no grow on ice. A tree does not grow in a desert. A tree does not grow on grassland-savannahs. A tree does not grow in alpine areas. A tree does not grow in the tundra

We are left with perhaps 15% of the planet upon which forests grow/grew. That does not make any studies from tree rings global, or even hemispheric.

The width and density of tree rings is dependent upon the following variables which cannot be reliably separated from each other.

sunlight - if the sun varies, the ring will vary. But not at night of course.

cloudiness - more clouds, less sun, less ring.
pests/disease - a caterpillar or locust plague will reduce photosynthesis
access to sunlight - competition within a forest can disadvantage or advantage some trees.
moisture/rainfall - a key variable. Trees do not prosper in a drought even if there's a heat wave.
snow packing in spring around the base of the trees retards growth
temperature - finally!

The tree ring is a composite of all these variables, not merely of temperature. Therefore on the 15% of the planet covered by trees, their rings do not and cannot accurately record temperature in isolation from the other environmental variables.

In my article on Greening Earth Society on the Hockey Stick, I point to other evidence which contradicts Mann's theory. The Idso's have produced more of that evidence, and a new article on Greening Earth has `unearthed' even more.

Mann's theory simply does not stack up. But that was not the key issue. Anyone can put up a dud theory from time to time. What is at issue is the uncritical zeal with which the industry siezed on the theory before its scientific value had been properly tested. In one go, they tossed aside dozens of studies which confirmed the existence of the MWE and LIA as global events, and all on the basis of tree rings - a proxy which has all the deficiencies I have stated above.

The worst thing I can say about any paper such as his is that it is `bad science'. Legal restraint prevents me going further. But in his case, only those restraints prevent me going *much* further.

Cheers

John Daly

--
John L. Daly
`Still Waiting For Greenhouse'
http://www.microtech.com.au/daly

replies to: daly@microtech.com.au

PLEASE NOTE:
WEBSITE URL HAS BEEN CHANGED TO http://www.microtech.com.au/daly
EMAIL ADDRESS HAS BEEN CHANGED TO daly@microtech.com.au
BOOKMARKS AND ADDRESS ENTRIES, IF ANY, SHOULD BE AMENDED ACCORDINGLY.

212. 0983196231.txt
###

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Wally
Date: Mon, 26 Feb 2001 09:03:51 -0500
Cc: mhughes@ltrr.arizona.edu, tom crowley <tom@ocean.tamu.edu>, rbradley@geo.umass.edu, tom@ocean.tamu.edu, k.briffa@uea.ac.uk, t.osborn@uea.ac.uk, mann@virginia.edu

Dear Phil,

Thanks for your response. I agree that I think these folks just don't quite seem to get it! Anyways, I've pasted in the text of Broecker's piece below (everything there but the figure. Trust me, the figure isn't worth looking at anyways). Will be very interested to hear your thoughts after reading this...

mike

PALEOCLIMATE:
Was the Medieval Warm Period Global?

Wallace S. Broecker*

The reconstruction of global temperatures during the last millennium can provide important clues for how climate may change in the future. A recent, widely cited reconstruction (1) leaves the impression that the 20th century warming was unique during the last millennium. It shows no hint of the Medieval Warm Period (from around 800 to 1200 A.D.) during which the Vikings colonized Greenland (2), suggesting that this warm event was regional rather than global. It also remains unclear why just at the dawn of the Industrial Revolution and before the emission of substantial amounts of anthropogenic greenhouse gases, Earth's temperature began to rise steeply.

Was it a coincidence? I do not think so. Rather, I suspect that the post-1860 natural warming was the most recent in a series of similar warmings spaced at roughly 1500-year intervals throughout the present interglacial, the Holocene. Bond et al. have argued, on the basis of the ratio of iron-stained to clean grains in ice-rafted debris in North Atlantic sediments, that climatic conditions have oscillated steadily over the past 100,000 years (3), with an average period close to 1500 years. They also find evidence for the Little Ice Age (from about 1350 to 1860) (3). I agree with the authors that the swing from the Medieval Warm Period to the Little Ice Age was the penultimate of these oscillations and will try to make the case that the Medieval Warm Period was global rather than regional.

One difficulty encountered when trying to reconstruct Holocene temperature fluctuations is that they were probably less than 1°C. In my estimation, at least for time scales greater than a century or two, only two proxies can yield temperatures that are accurate to 0.5°C: the reconstruction of temperatures from the elevation of mountain snowlines and borehole thermometry. Tree ring records are useful for measuring temperature fluctuations over short time periods but cannot pick up long-term trends because there is no way to establish the long-term evolution in ring thickness were temperatures to have remained constant. Corals also are not accurate enough, especially because few records extend back a thousand years. The accuracy of the temperature estimates based on floral or faunal remains from lake and bog sediments is likely no better than ±1.3°C (4) and hence not sufficiently sensitive for Holocene thermometry.

The Mountain Glaciation Record
At the Last Glacial Maximum, mountain snowlines throughout the world were on average about 900 m lower than today (5). On the basis of today's rates of temperature change with elevation, this required an air

temperature cooling at the elevation of the glacier of about 5°C (and a corresponding tropical sea surface temperature cooling of about 3°C). During the Younger Dryas--a cold "spell" of about 1200 years during the last deglaciation--snowlines in the Swiss and New Zealand Alps dropped to about 300 m below the lowest levels reached in the subsequent Holocene.

Since their 1860 maximum at the end of the Little Ice Age, the retreat of Swiss glaciers represents a rise in snowline of about 90 m (6). If this rise could be attributed entirely to air temperature, the required warming would be between 0.5° and 0.6°C. However, simple considerations suggest that precipitation changes result in a negative feedback of about 20% (7). The warming required to account for the post-1860 retreat of Alpine snowlines would then be between 0.6° and 0.7°C.

The post-1860 glacier retreat is not confined to Switzerland. With the exception of Antarctica, it has been well documented everywhere on Earth where ice-covered mountains are present (2). There is no doubt that the Little Ice Age was global in extent and that the post-1860 warming was also global. In this regard, the Mann et al. (1) reconstruction is consistent with the mountain snowline record.

The Medieval Warm Period has also left its traces in the Swiss Alps. Holzhauser has reconstructed the history of a larchwood aqueduct constructed by medieval farmers (8). It ran from a small mountain lake along the valley occupied by the Grosser Aletsch Glacier, supplying water to an Alpine village. The aqueduct was first constructed around 1200 A.D. (toward the end of the Medieval Warm Period). It was partially destroyed when the glacier advanced in 1240 A.D. and had to be totally rerouted after a further advance in 1370 A.D.

Swiss geologists and geomorphologists agree that the large moraines marking the maximum glacier extent during the Little Ice Age are a composite of debris left behind by a series of Holocene advances (9). For example, soils separating individual advance episodes have been found within the moraines. Precise dating has proven difficult, however, and the chronology of these prior advances remains uncertain.

Two recent studies of Holocene climate cycles in the Swiss Alps have greatly improved this situation. Both focus on establishing the times of glacial retreats rather than advances. Holzhauser (8), on the basis of radiocarbon dating of larchwood stumps exposed by the ongoing retreat of the Grosser Aletsch Glacier, finds warm episodes 2400 ± 300 and 1500 ± 200 calendar years ago. Hormes and Schlüchter (10-12) have dated wood and peat fragments that are being disgorged from beneath a number of Swiss glaciers. Radiocarbon dates of a large number of these samples cluster in three major groups centered at 8700, 6600, and 4300 calendar years before present. The correlation between these Alpine warm phases and the warm phases of Bond's North Atlantic ice-rafting record, although imperfect, is encouraging (see the figure).

Climatic oscillations during the Holocene. Circles show the ratios of iron-stained to total grains (for grains with diameters >63 mm) in a North Atlantic core (3). The chronology is taken from (22). The green (10-12) and yellow (8) boxes are based on radiocarbon dating on wood and peat formed when the glaciers had retreated to positions similar to or up-valley from those at present (see text).

CREDIT: FIGURE PREPARED BY AUTHOR FOR THIS PUBLICATION

Borehole Thermometry

Geothermal heat is produced deep inside Earth, and the shape of the vertical temperature profile measured in a borehole from any point on Earth's surface thus reflects the depth dependence of the thermal conductivity of the crustal material. The temperature at the surface does not remain constant, however, and the thermal profiles therefore have kinks that reflect past air temperature fluctuations. Mathematical deconvolutions are used to reconstruct these fluctuations from the temperature profile, but because of smoothing due to diffusive spreading of past thermal anomalies, many different time histories fit the observed downhole temperature record. The modeler selects from these possibilities the temperature history with the least complicated shape. The details are thus lost, and only the broad features of the time history are captured.

Deconvolutions of thermal records from holes drilled through the polar ice caps reveal broad maxima that reflect the colder temperatures during glacial times. In Greenland boreholes, this broad glacial feature is preceded by a narrower one, which requires a temperature oscillation to have occurred in the late Holocene. The timing of this swing broadly matches that of the Medieval Warm Period to Little Ice Age oscillation. Its magnitude is about 2°C (13). The borehole temperature record for Greenland thus appears to reflect the climate changes thought to have led to the establishment and eventual abandonment of the Viking colonies in southern Greenland (2). It is also consistent with records in the Swiss Alps.

Far Field Evidence

Evidence for the Medieval Warm Period from other parts of the world exists but is spotty and/or circumstantial. From an analysis of 6000 continental borehole thermal records from around the world (14), Huang et al. conclude that 500 to 1000 years ago, temperatures were warmer than today, but that about 200 years ago, they cooled to a minimum some 0.2° to 0.7°C below present. However, as is the case for the thermal profiles in ice, those for continental boreholes are highly smoothed. Although suggestive, the fluctuation postulated by Huang et al. does not prove that the Medieval Warm Period was global in extent.

Evidence that climate during the latter part of the Medieval Warm Period was much different from today's comes from moisture records for the western United States. Stine has studied lodgepole pine trees rooted at 8 to 19 m depth in Lake Tenaya in the high Sierra Nevada (15). For the trees to have grown, the lake must have been nearly dry. In contrast, only once during the past 50 years has the lake not overflown during snowmelt. Using radiocarbon dating and ring counting, Stine has shown that for 70 years before 1093 A.D., the lake stood at least 13 m below its outflow spillway, and for 141 years before 1333 A.D., it stood at least 11 m below its spillway (16). Stine has documented similar events at Mono Lake and the Walker River (17). He concludes that late in the Medieval Warm Period, California experienced several decade-long periods of profound drought.

If, as Bond et al. (3) suggest, the cyclic changes in ice-rafted debris composition reflect oscillations in the strength of the Atlantic's conveyor circulation, one might expect temperature changes in Antarctica to have been opposite in phase to those in the North Atlantic, as was the case during the last deglaciation (18). Clow has carried out a deconvolution of the temperature record at the Antarctic Taylor Dome site (19). His reconstruction shows that the air temperature was 3°C colder during the time of the Medieval Warm Period than during that of the Little Ice Age. This record suggests that conditions in Antarctica underwent an antiphased oscillation during the Medieval Warm Period-Little Ice Age period.

The Case for a Global Event

The case for a global Medieval Warm Period admittedly remains inconclusive. But keeping in mind that most proxies do not have adequate sensitivity, it is interesting that those capable of resolving temperature changes of less than 1°C yield results consistent with a global Medieval Warm Period. To test whether this is indeed the case, we require Holocene snowline fluctuation records for tropical and Southern Hemisphere sites and continued studies of wood and peat exposed by the continuing retreat of Northern Hemisphere glaciers. As the world's mountain glaciers continue to retreat, ever more evidence for past Holocene warm episodes will be exposed.

One might ask why the strength of the Atlantic's conveyor circulation oscillates on a time scale of one cycle per 1000 to 2000 years. I suspect that it has to do with the export through the atmosphere of water vapor from the Atlantic to the Pacific Ocean. The magnitude of this export has been estimated to be (0.25 ± 0.10) x 106 m3/s (20). If this freshwater loss were not balanced by the export of salt from the Atlantic, the latter's salt content would rise at the rate of about one gram per liter each 1500 years. Such an increase in salt content would densify cold surface water by an amount equivalent to a 4 to 5 K cooling, thereby strongly altering the buoyancy of surface waters in the North Atlantic and hence their ability to sink to the abyss.

I believe that this salt export is not continuous but episodic. The salt content of the Atlantic periodically builds up until a strong conveyor circulation mode is turned on, causing the salt content to drain down. Eventually, a weak circulation mode kicks in, allowing the salt content to build up again. I have suggested previously (21) that an apparent mismatch between radiocarbon and chlorofluorocarbon-based estimates of the rate of deep-water formation in the Southern Ocean may reflect a change in circulation after the Little Ice Age.

The geographic pattern of Holocene climate fluctuations remains murky, but several things are clear. The Little Ice Age and the subsequent warming were global in extent. Several Holocene fluctuations in snowline, comparable in magnitude to that of the post-Little Ice Age warming, occurred in the Swiss Alps. Borehole records both in polar ice and in wells from all continents suggest the existence of a Medieval Warm Period. Finally, two multidecade-duration droughts plagued the western United States during the latter part of the Medieval Warm Period. I consider this evidence sufficiently convincing to merit an intensification of studies aimed at elucidating Holocene climate fluctuations, upon which the warming due to greenhouse gases is superimposed.

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At 12:04 PM 2/26/01 +0000, Phil Jones wrote:
>
> Dear All,
> I was away over the weekend at Bowdoin College in Maine, giving a
>talk about the
> last 1000 years. There were three others as well on other paleo aspects,
>Richard Alley,
> Gary Clow and Wally Broecker ! The latter briefly mentioned to me that
>he had had
> something in last Friday's Science, which was getting at the Mann et al.
>series. He
> didn't have a copy so we've not seen it here yet. I tried to get a copy
>of Science on
> the bookstand at Logan airport last night - I guess it's not sold that
way !
> Wally was going on about this 1500 yr cycle of Bond's, which seemed
>pretty flimsy.
> I was showing all the various series in a general talk - and I used some
>of the overheads
> from the upcoming Science paper. This is due to appear in the issue for
>the last week
> of April. It is all accepted now. I will forward if you'll all abide by
>the Science rules. Both
> Wally and Alley seem convinced that the climate of Greenland changed by
>10 C in
> the space of 2-3 years at times in the past (Y Dryas etc). I had long
>talks with both
> and they don't seem to have got their heads around spatial scales (local
>changes
> and hemispheric). Also they don't seem to realise where we are coming
>from. He
> has a downer on trees (believes all the multiproxy series depend
>exclusively on
> trees) but he thinks Ed Cook is a great scientist. The latter is true,
>but he might
> just think that because he's at Lamont. I did tell him that Keith's paper

>on the age
> banding is out in JGR. I should send him a reprint and maybe ask that great
> scientist to go and explain it to him ! Ed's in NZ at the moment. Also
>Wally believes
> much more in glacier advances/retreats. I'll get Keith to send him
>Sarah's paper
> where the long Tornetrask reconstruction is shown to agree with
Storglaciaren
> advance/retreat dates from moraine evidence. Also Sarah's been working on
>similar
> glaciers in the Swiss Alps with long tree-ring reconstructions. One
>interesting
> thing was he didn't seem to realise that a lot of the tree-ring
>reconstructions use
> density. Seemed to think they were all ring widths and there had to be
>moisture
> changes we were not accounting for.
>   It is easy to respond to a Perspectives piece. Some of you did it
>with respect to
> one of mine. I'm not sure it will achieve much - it won't come out before
>the paper
> in the last week of April. I need to wait to se what he says. Our paper
>(me, Tim and
> Keith) clearly says that the MWP couldn't have been warmer (for the NH
>average)
> than the late 20th century.
>   Another possible reason for not doing anything is that the IPCC
>report will be out
> soon. The summary is written in pretty clear language.
>   The above is my first thoughts, not having read the piece and just
>got off the
> flight back.
>
>   Best to ignore Woijcek. All he seems to want to do is deflect us into
>responding.
>
> Cheers
> Phil
>
>
>
>At 11:47 25/02/01 -0700, mhughes@ltrr.arizona.edu wrote:
>>Dear all,
>>WHat mechanism does "Science" have for repsonding to Perspective pieces?
Most
>>of the answer to Wally is contained within his own piece - he comments on
the
>>ambiguity of the record, which, in various ways, we have all done. What he
>>doesn't offer, however, is anything other than an anecdotal alternative. As
>>always, he seeks to damn (in this case with faint praise) the records or
>>work
>>that don't serve his purpose , and to elevate any scrap of evidence that
does
>>serve it. I think it will be important for us to stick closely to what we
>>have
>>written in published papers. CHeers, MAlcolm
>>
>>Quoting "Michael E. Mann" <mann@virginia.edu>:
>>
>> > Dear Phil, Ray,
>> >
>> > What do you guys think. If we're all on board, than an appropriately
>> > toned,

>> > "high road" response here might be appropriate. We don't want to engage
>> > Wally in a personal battle, but simply should correct the record where
>> > Wally has muddied it. Again, Phil et al do have a Science article in
>> > press
>> > that serves this purpose to some extent, so I'm especially interested in
>> > what
>> > Phil thinks (Phil?)...
>> >
>> > mike
>> >
>> > At 02:52 PM 2/24/01 -0700, mhughes@ltrr.arizona.edu wrote:
>> > >Dear Mike et al., I think we should definitely let Wojick stew in his
>> > own
>> > >juice - as Mike pointed out to me the other day he, and his like, have
>> > a
>> > >specific agenda, and anything we write will be pressed into the service
>> > of
>> > that
>> > >agenda. I'm not so sure about Wally. I share Tom's disinclination to
>> > get
>> > into a
>> > >street fight with Wally - generally I take the view that life's too
>> > short and
>> > >uncertain for such activities. On the other hand, would we let such a
>> > shoddy
>> > >piece of work(and editing) go by if it were from another author? There
>> > are so
>> > >many holes in Wally's argument, and such a selective choice of evidence
>> > that it
>> > >should beggar belief. One of the more obvious holes is that he writes
>> > of the
>> > >Great Basin droughts of the 10th through 14th centuries as proof of
>> > warmer
>> > >conditions then, but doesn't explain why we don't have such conditions
>> > now.
>> > >Interestingly, Larry Benson, Dave Meko and others have good evidence
>> > that
>> > these
>> > >same multidecadal periods were marked by a great excess of
>> > precipitation
>> > just a
>> > >few hundred miles north in northern Nevada and California and southern
>> > Oregon.
>> > >He just hasn't grasped that the methods that are appropriate for
>> > tracking the
>> > >consequences of major changes in boundary conditions don't work in the
>> > late
>> > >Holocene. I've been trying to figure out the issue of "Was there a
>> > Medieval
>> > >Warm Period, and if so where and when" for a decade or so, and still
>> > have the
>> > >impression that the records for the 9th through 14th centuries are
>> > extremely
>> > >mixed. But then, I didn't come to the investigation with a certain
>> > knowledge of
>> > >the absolute truth, and have had to 'misfortune' to work with people
>> > who let
>> > >careful analysis get in the way - Henry Diaz, Ray and Mike, and others.
>> >
>> > >Anyway, the point of this rant is that I think we should give careful
>> > >consideration to making a measured response to Wally. Cheers, Malcolm
>> > >
>> > >

>> > >
>> > >
>> > >Quoting "Michael E. Mann" <mann@virginia.edu>:
>> > >
>> > >> Hi Tom,
>> > >>
>> > >> Thanks for your quick reply. I agree with you entirely. I think its
>> > very
>> > >> unfortunate he's chosen to disinform the community rather than engage
>> > in
>> > >> a
>> > >> constructive dialogue (we tried the latter w/ him in a series of
>> > emails
>> > >> last
>> > >> year, but clearly to no avail).
>> > >>
>> > >> On the other hand, think that a war of words w/ Broecker would be
>> > >> exploited
>> > >> by the skeptics, and perhaps we should just try to let this thing
>> > die...
>> > >>
>> > >> I'm not sure. I'd appreciate knowing what others think?
>> > >>
>> > >> mike
>> > >>
>> > >> At 10:25 AM 2/24/01 -0600, tom crowley wrote:
>> > >> >Mike,
>> > >> >
>> > >> >I was not aware of the Broecker piece - I am dismayed but not
>> > >> surprised. I
>> > >> >do not know what to do - I personally cannot stand the combative
>> > >> personal
>> > >> >approach Broecker relishes but it does seem as if some rebuttal is
>> > >> called
>> > >> >for. Maybe you Ray Phil I and Malcolm could pen a response - we are
>> > >> >heading to Germany in a week, for a month, so I am not sure how much
>> > I
>> > >> can
>> > >> >keep up on this but it seems as if some response is called for.
>> > >> >
>> > >> >What think ye?
>> > >> >
>> > >> >Tom
>> > >> >
>> > >> >
>> > >> >>Dear Mike,
>> > >> >>
>> > >> >>Thanks for passing this along.
>> > >> >>
>> > >> >>Wojick of course completely misrepresents Broecker, and puts his
>> > >> >>conventional intellectually dishonest spin on this.
>> > >> >>
>> > >> >>That having been said, it is a bit disappointing that Wally
>> > continues
>> > >> to
>> > >> >>cling to some of his flawed beliefs which aren't supported from
>> > either
>> > >> our
>> > >> >>best current understanding of the observations or of the results of
>> > >> careful
>> > >> >>modeling experiments. My own perception is that the climate
>> > community,
>> > >> >>modelers as well as observationalists, simply don't take seriously

>> > >> anymore
>> > >> >>the idea that the history of climate change over the past 1000
>> > years
>> > >> is
>> > >> >>part of an internal oscillation. The sediment core evidence oft
>> > cited
>> > >> by
>> > >> >>Broecker (e.g. Bond et al) for this is tremendously weak, and I, as
>> > >> well as
>> > >> >>the vast majority of my colleagues, simply don't buy it for even a
>> > >> second.
>> > >> >>But people don't like to challenge Broecker publically. He can and
>> > >> will
>> > >> >>play hardball.
>> > >> >>
>> > >> >>There is an odd irony. Broecker refused to accept the modeling
>> > >> evidence
>> > >> >>that the 100 kyr ice age Pleistocene variations were part of an
>> > >> internal
>> > >> >>oscillation paced by insolation variations, favoring instead the
>> > >> >>discredited notion that they were a direct response to (too weak)
>> > >> >>eccentricity forcing, until the evidence became insurmountable
>> > (from
>> > >> my
>> > >> >>adviser, Barry Saltzman, may he rest in piece, and people like Dick
>> > >> >>Peltier). Ironically, Broecker then took credit for the very
>> > >> proposition he
>> > >> >>had fought w/ tooth and nail.
>> > >> >>
>> > >> >>Broecker is even more wrong, and unfortunately equally stubborn, in
>> > >> this case.
>> > >> >>And, again, the reason: because his pet theory, that climate
>> > >> variability is
>> > >> >>a simple millennial oscillation, is finally being challenged w/
>> > hard
>> > >> data
>> > >> >>and hard facts.
>> > >> >>
>> > >> >>Broecker misrepresents the nature of that data that we and others
>> > have
>> > >> >>used, and misunderstands the source of the muted hemispheric trends
>> > >> (there
>> > >> >>*is* a hemispheric "medieval warm period" and "little ice age",
>> > just
>> > >> not of
>> > >> >>the magnitude or the distinctiveness that Broecker imagines).
>> > >> Individual
>> > >> >>regions in our reconstructions, and Phils, and others, vary by
>> > several
>> > >> >>degrees C, ie, the proxies we use have no problem whatsoever in
>> > >> resolving
>> > >> >>high-amplitude temperature variations in the past. The problem is
>> > that
>> > >> when
>> > >> >>we look at the different regions we find that periods of cold and
>> > >> warm
>> > >> >>often occur at very different times in different regions, and so in
>> > a
>> > >> >>hemispheric or global average, a lot of purely regional variability
>> > >> cancels
>> > >> >>out. The resulting trends are somewhat smaller. I remained
>> > befuddled
>> > >> as to

>> > >> >>why Wally doesn't understand this point. Its been explained to him
>> > >> time and
>> > >> >>time again. Maybe he's just not listening, or doesn't want to
>> > >> listen...
>> > >> >>
>> > >> >>In fact, Tom Crowley has clearly shown that the observed millennial
>> > >> >>temperature reconstruction is precisely consistent w/ our
>> > >> understanding of
>> > >> >>*forced* climate change over the past 1000 years (solar changes,
>> > >> volcancic
>> > >> >>output, and recent greenhouse gas concentrations). There is, simply
>> > >> put, no
>> > >> >>room for a global millennial internal oscillation. Regionally, such
>> > >> types
>> > >> >>of climate phenomena, associated for example with changes in the
>> > North
>> > >> >>Atlantic ocean circulation, are supported by the observations. This
>> > >> >>explains why, for example, European temperature variations are
>> > >> somewhat
>> > >> >>larger than those in other regions not effected so strongly by such
>> > >> climate
>> > >> >>processes.
>> > >> >>
>> > >> >>Other recent perspectives, by Ray Bradley and myself provide a far
>> > >> more
>> > >> >>balanced and nuanced (and less dogmatic or defensive) viewpoint.
>> > I'm
>> > >> not
>> > >> >>sure a written response to Broecker is worthwhile (this is,
>> > afterall,
>> > >> a
>> > >> >>"perspective" and everyone understands that a scientist may have a
>> > >> flawed
>> > >> >>perspective). If Wally wants this to be his legacy, so be it...
>> > >> >>
>> > >> >>Phil and others have a review article coming out in the near future
>> > >> which
>> > >> >>also provides a much more balanced perspective on the climate
>> > changes
>> > >> of
>> > >> >>the past millennium, and will set the record straight once again
>> > (good
>> > >> >>timing Phil!). Science's embargo policy prevents me from saying
>> > much
>> > >> more
>> > >> >>at this time, but if Phil or anyone else wishes to comment further,
>> > >> I'd
>> > >> >>encourage it.
>> > >> >>
>> > >> >>Well, I've still got some snow to shovel here in Charlottesville!
>> > >> Happy
>> > >> >>weekend to all,
>> > >> >>
>> > >> >>mike
>> > >> >>
>> > >> >>p.s. For those with electronic subscriptions, Broecker's latest
>> > piece
>> > >> can
>> > >> >>be found here:
>> > >> >>
>> > >> >> PALEOCLIMATE:
>> > >> >>   Was the Medieval Warm Period Global?
>> > >> >>   Wallace S. Broecker

>> > >> >>   Science Feb 23 2001: 1497-1499. [Summary] [Full Text]
>> > >> >>
>> > >> >>http://www.sciencemag.org/cgi/content/full/291/5508/1497
>> > >> >>
>> > >> >>While my previous perspective piece is here:
>> > >> >>   CLIMATE CHANGE:
>> > >> >>   Lessons for a New Millennium
>> > >> >>   Michael E. Mann
>> > >> >>   Science 2000 July 14; 289: 253-254. (in Perspectives) [Summary]
>> > >> [Full
>> > >> >>Text]
>> > >> >>URL:
>> > >>
>> > >>http://www.sciencemag.org/cgi/content/full/289/5477/253?maxtoshow=&HIT
>> S=10&h
>> > >>
>> > >>its=10&RESULTFORMAT=&author1=Mann&searchid=QID_NOT_SET&stored_search=&
>> FIRSTI
>> > >> >>NDEX=&fdate=10/1/1995&tdate=2/28/2001
>> > >> >>
>> > >> >>and Bradley's is here:
>> > >> >>
>> > >> >>   PALEOCLIMATE: Enhanced: 1000 Years of Climate Change
>> > >> >>   Ray Bradley
>> > >> >>   Science 2000 May 26; 288: 1353-1355. (in Perspectives) [Summary]
>> > >> [Full
>> > >> >>Text]
>> > >> >>
>> > >> >>URL:
>> > >>
>> > >>http://www.sciencemag.org/cgi/content/full/288/5470/1353?maxtoshow=&HI
>> TS=10&
>> > >>
>> > >>hits=10&RESULTFORMAT=&author1=Bradley&searchid=QID_NOT_SET&stored_sear
>> ch=&FI
>> > >> >>RSTINDEX=&fdate=10/1/1995&tdate=2/28/2001
>> > >> >>
>> > >> >>>Dear Michael--The third point below has comments on the
>> > controversy
>> > >> >>>betweenyou and Broecker--I'd be interested in your response (did
>> > >> Wally not
>> > >> >>>just understand what your data show?).
>> > >> >>>
>> > >> >>>Mike
>> > >> >>>
>> > >> >>>Three Wojick Pieces on Climate Change.
>> > >> >>>I've been busy busy.
>> > >> >>>
>> > >> >>>David
>> > >> >>>
>> > >> >>>FIRST, the latest issue of Insight Magazine includes a
>> > >> point-counterpoint
>> > >> >>>between measly old me and the great Robert Watson. Boy has he got
>> > >> >>>credentials! Too bad he's wrong.
>> > >> >>>
>> > >> >>><http://www.insightmag.com/archive/200103143.shtml>
>> > >> >>>
>> > >> >>>Symposium: Do scientists have compelling evidence of global
>> > warming?
>> > >> >>>
>> > >> >>>Yes: Rising sea levels worldwide and retreating Arctic glaciers
>> > are
>> > >> ominous

>> > >> >>>signs.
>> > >> >>>
>> > >> >>>By Robert T. Watson -- chairman of the UN Intergovernmental Panel
>> > on
>> > >> >>>Climate Change, chief scientist at the World Bank and former chief
>> > >> science
>> > >> >>>advisor to the Clinton White House.
>> > >> >>>
>> > >> >>>No: Despite the overheated rhetoric, there is no new evidence of
>> > >> warming
>> > >> >>>
>> > >> >>>By David E. Wojick -- covers climate policy for Electricity Daily
>> > and
>> > >> is a
>> > >> >>>science adviser to the Greening Earth Society
>> > >> >>><http://www.greeningearthsociety.org>, as well as Undereditor of
>> > the
>> > >> >>>Washington Pest <http://www.WashingtonPest.com>
>> > >> >>>
>> > >> >>>SECOND, the February 15 Eco-logic on-line has published "The Black
>> > >> Hole of
>> > >> >>>Global Climate Government" by David Wojick, my detailed attack on
>> > the
>> > >> >>>Framework Convention on Climate Change. It includes a lot of the
>> > >> actual
>> > >> >>>treaty language.
>> > >> >>>
>> > >> >>><http://www.eco.freedom.org/el/20010202/wojick.shtml>
>> > >> >>>
>> > >> >>>THIRD, here is a draft Electricity Daily article of mine. Seems
>> > I'm
>> > >> not the
>> > >> >>>only one who thinks the IPCC is nuts.
>> > >> >>>
>> > >> >>>Climate Guru Kicks The Hockey Stick
>> > >> >>>by David Wojick (dwojick@shentel.net)
>> > >> >>>
>> > >> >>>Global warming is natural and the recent warming is probably no
>> > >> exception.
>> > >> >>>This is the controversial argument made by prominent climatologist
>> > >> Wallace
>> > >> >>>S. Broecker in today's issue of Science.
>> > >> >>>
>> > >> >>>Broecker's bombshell bears the seemingly innocent title "Was the
>> > >> Medieval
>> > >> >>>Warm Period Global?" It may seem esoteric, but whether the
>> > apparent
>> > >> warmth
>> > >> >>>reported in Europe about 1000 years ago was global or simply local
>> > is
>> > >> >>>becoming a central issue in climate science. What makes it
>> > >> contentious is
>> > >> >>>the recent claims by the United Nations Intergovernmental Panel on
>> > >> Climate
>> > >> >>>Change that the earth is warmer now than it has been for
>> > millennia,
>> > >> and
>> > >> >>>that therefore human carbon dioxide emissions are to blame.
>> > Broecker,
>> > >> a
>> > >> >>>leading figure at Lamont-Doherty Earth Observatory, Columbia
>> > >> University,
>> > >> >>>questions both IPCC claims.

>> > >> >>>
>> > >> >>>The focus of the debate is a 1000-year temperature reconstruction
>> > >> known in
>> > >> >>>climate circles as the "hockey stick". Produced in 1999 by M. E.
>> > >> Mann, R.
>> > >> >>>S. Bradley, M. K. Hughes, the long handle of the hockey stick
>> > shows
>> > >> global
>> > >> >>>temperatures for the first 8 centuries as basically unchanging,
>> > >> followed by
>> > >> >>>the sharply up-tilting blade of the last 150 years or so. The Mann
>> > et
>> > >> al
>> > >> >>>hockey stick is the central feature of the recently released IPCC
>> > >> working
>> > >> >>>group one Summary for Policy makers, which claims to embody the
>> > best
>> > >> of
>> > >> >>>climate science.
>> > >> >>>
>> > >> >>>Broecker does not like the hockey stick, nor the conclusions the
>> > IPCC
>> > >> draws
>> > >> >>>from it. He says " A recent, widely cited reconstruction (Mann's)
>> > >> leaves
>> > >> >>>the impression that the 20th century warming was unique during the
>> > >> last
>> > >> >>>millennium. It shows no hint of the Medieval Warm Period (from
>> > around
>> > >> 800
>> > >> >>>to 1200 A.D.) during which the Vikings colonized Greenland,
>> > >> suggesting that
>> > >> >>>this warm event was regional rather than global. It also remains
>> > >> unclear
>> > >> >>>why just at the dawn of the Industrial Revolution and before the
>> > >> emission
>> > >> >>>of substantial amounts of anthropogenic greenhouse gases, Earth's
>> > >> >>>temperature began to rise steeply. Was it a coincidence? I do not
>> > >> think so.
>> > >> >>>Rather, I suspect that the post-1860 natural warming was the most
>> > >> recent in
>> > >> >>>a series of similar warmings spaced at roughly 1500-year intervals
>> > >> >>>throughout the present inter-glacial, the Holocene."
>> > >> >>>
>> > >> >>>Broecker presents the evidence for a global Medieval Warm Period,
>> > as
>> > >> well
>> > >> >>>as for a Little Ice Age from around 1300 to 1860, when the present
>> > >> >>>temperature rise begins. He also argues that the "proxy" evidence
>> > >> used by
>> > >> >>>Mann et al, such as tree ring data, is ill suited to the time
>> > period
>> > >> and
>> > >> >>>temperature variation -- less than one degree C -- in question.
>> > >> >>>
>> > >> >>>As he puts it, "In my estimation, at least for time scales greater
>> > >> than a
>> > >> >>>century or two, only two proxies can yield temperatures that are
>> > >> accurate
>> > >> >>>to 0.5 C: the reconstruction of temperatures from the elevation of
>> > >> mountain
>> > >> >>>snowlines and borehole thermometry. Tree ring records are useful
>> > for

>> > >> >>>measuring temperature fluctuations over short time periods but
>> > cannot
>> > >> pick
>> > >> >>>up long-term trends because there is no way to establish the
>> > >> long-term
>> > >> >>>evolution in ring thickness were temperatures to have remained
>> > >> constant."
>> > >> >>>
>> > >> >>>Broecker acknowledges that the proxy evidence is necessarily
>> > somewhat
>> > >> >>>"murky", but his conclusion is that "climatic conditions have
>> > >> oscillated
>> > >> >>>steadily over the past 100,000 years, with an average period close
>> > to
>> > >> 1500
>> > >> >>>years... The swing from the Medieval Warm Period to the Little Ice
>> > >> Age was
>> > >> >>>the penultimate of these oscillations." The implication being that
>> > >> some, if
>> > >> >>>not all, of the present warming is the natural swing out of the
>> > >> Little Ice
>> > >> >>>Age, and that Mann et al, as well as the IPCC, are mistaken.
>> > >> >>>
>> > >> >>>
>> > >> >>>--
>> > >> >>>
>> > >> >>>
>> > >> >>>Dr. David E. Wojick
>> > >> >>>President
>> > >> >>>Climatechangedebate.org
>> > >> >>>Subscribe to the free debate listserv at
>> > >> http://www.climatechangedebate.org
>> > >> >>>Non subscribers can follow the debate at
>> > >> >>>http://www.eScribe.com/science/ClimateChangeDebate/
>> > >> >>>
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>> > >> >>   University of Virginia
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http://www.evsc.virginia.edu/faculty/people/mann.html

213. 0983204299.txt
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##########

From: tom crowley <tom@ocean.tamu.edu>

To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
Subject: Re: Wally
Date: Mon, 26 Feb 2001 11:18:19 -0600
Cc: mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu, k.briffa@uea.ac.uk, p.jones@uea.ac.uk, t.osborn@uea.ac.uk

Mike,

you are really the most appropriate person to be the lead author on this - I was just volunteering myself as the unfortunate soul who has to bear the brunt of Wallys wrath

Tom

ps Peck would be fine of course but I don't know whether we want to get him tangled up in the acrimony - we could of course ask for his comments beforehand

>HI Tom,
>
>Thanks--I was thinking this too. Ray held out a real olive branch to Wally
>by the extremely balanced piece he wrote in Science last year (some of us
>thought he caved in a bit too much!). So there was absolutely no reason for
>Wally to write this piece.
>
>If Julie Uppenbrink gives us the go-ahead, I say lets do as Tom suggests. I
>think this has a lot more cachet if all on this list are willing to sign on
>as co-authors.
>
>Regarding primary authorship: On the one hand, it would be appropriate for
>me tsince it is primarily Mann et al that is explicitly under attack here,
>though all of us are implicitly under attack. However, I think the piece
>carries a lot more weight if it is authored by someone of Wally's stature,
>and I think Tom far better fits the bill in this regard. So if Tom is
>willing to bear the brunt of this, I would definitely endorse him being
>primary author.
>
>I would argue to include Peck too, but I think this would be a conflict for
>him, as he is pretty close to Wally. So best to leave it w/ the current
>group in my opinion. Lets pursue this further once Phil hears back from J.U...
>
>mike
>
>At 09:16 AM 2/26/01 -0600, tom crowley wrote:
>>Hi all,
>>
>>I vote for a response - quick and to the point - itemized in fact.
>>
>>The only problem is somehow has to volunteer to be the sacrificial lamb as
>>first author - that person will almost certainly be badgered by Wally and
>>probably charged with some trumped up unethical piece - he will also
>>probably try to subvert the review process by contacting the Editor of
>>Science. This is not paranoia - Wally did exactly this when some people
>>(some at Lamont!) questioned his conveyor explanation for the LIA that came
>>out in Science a year or so ago. He was actually screaming at some of
>>these people in the Lamont lunch room.
>>
>>That said, I say we must bite the bullet and do it - Wally doesn't like me
>>anyway so it wouldnt make as much a difference to me if I volunteered to go
>>to the slaughter but if there is anyone else who wants to take the lead,
>>thats fine with me!!

```
>>
>>Tom
>>
>>ps  as I indicated the other day I will be in only until this Friday after
>>which I am out for a month - I could write enough to get us going and then
>>hand it over to someone else to deal with the submission business (MIke?)
>>
>>
>>>Thanks a bunch Phil,
>>>
>>>Will look forward to hearing back w/ more info. I talked to Dick Kerr last
>>>week about related stuff (an IPCC article he's writing) and he made no
>>>mention of this at all! I wonder who did commission this, and why?
>>>
>>>mike
>>>
>>>At 02:51 PM 2/26/01 +0000, Phil Jones wrote:
>>>>A
>>>>
>>>>
>>>>
>>>>
>>>>
>>>>
>>>>   Mike,
>>>>     I've had a quick read and sent an email to Julia Uppenbrink to get her
>>>>views as
>>>>  she commissioned our piece. Also asked about a response, particularly on
>>>the
>>>>  high and low frequency indicators.  I was going to send Wally two papers
>>>>  (Sarah Raper's on linking trees and glaciers in J. Glaciol. and Brian
>>>>Luckman's
>>>>  in The Holocene, where the two are also linked but only in a qualitative
>>>>way).
>>>>  From the weekend it was clear he had no ideas about these. His lack of
>>>>knowledge
>>>>  of density data in trees come through in the article as well.
>>>>     In Maine he also went on at length about the Stine work. and seems to
>>>>in this
>>>>  piece as well.  Malcolm should know all about this.
>>>>     I'm going to go home soon as I'm getting knackered, but I'll email you
>>>>Julia's
>>>>  response.  I think she'll find out who asked Wally to do it, as he
>>>>implied to me it
>>>>  was.
>>>>
>>>>  Cheers
>>>>  Phil
>>>>
>>>>  PS Meant to say at the start that I see your points. Thanks for pasting
>>>>it to us.
>>>>
>>>>
>>>>
>>>>Prof. Phil Jones
>>>>Climatic Research Unit        Telephone +44 (0) 1603 592090
>>>>School of Environmental Sciences    Fax +44 (0) 1603 507784
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From: Phil Jones <p.jones@uea.ac.uk>
To: mhughes@ltrr.arizona.edu,"Michael E. Mann" <mann@virginia.edu>
Subject: Re: Wally
Date: Mon, 26 Feb 2001 12:04:32 +0000
Cc: <mhughes@ltrr.arizona.edu>,"Michael E. Mann" <mann@virginia.edu>, tom crowley
<tom@ocean.tamu.edu>, "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>,
<rbradley@geo.umass.edu>,<tom@ocean.tamu.edu>,
<mhughes@ltrr.arizona.edu>,k.briffa@uea.ac.uk,t.osborn@uea.ac.uk

<x-flowed>

Dear All,
I was away over the weekend at Bowdoin College in Maine, giving a talk about the
last 1000 years. There were three others as well on other paleo aspects, Richard Alley,
Gary Clow and Wally Broecker ! The latter briefly mentioned to me that he had had
something in last Friday's Science, which was getting at the Mann et al. series. He
didn't have a copy so we've not seen it here yet. I tried to get a copy of Science on
the bookstand at Logan airport last night - I guess it's not sold that way !
Wally was going on about this 1500 yr cycle of Bond's, which seemed pretty flimsy.
I was showing all the various series in a general talk - and I used some of the overheads
from the upcoming Science paper. This is due to appear in the issue for the last week
of April. It is all accepted now. I will forward if you'll all abide by the Science rules. Both
Wally and Alley seem convinced that the climate of Greenland changed by 10 C in
the space of 2-3 years at times in the past (Y Dryas etc). I had long talks with both
and they don't seem to have got their heads around spatial scales (local changes
and hemispheric). Also they don't seem to realise where we are coming from. He
has a downer on trees (believes all the multiproxy series depend exclusively on
trees) but he thinks Ed Cook is a great scientist. The latter is true, but he might
just think that because he's at Lamont. I did tell him that Keith's paper on the age
banding is out in JGR. I should send him a reprint and maybe ask that great
scientist to go and explain it to him ! Ed's in NZ at the moment. Also Wally believes
much more in glacier advances/retreats. I'll get Keith to send him Sarah's paper
where the long Tornetrask reconstruction is shown to agree with Storglaciaren
advance/retreat dates from moraine evidence. Also Sarah's been working on similar
glaciers in the Swiss Alps with long tree-ring reconstructions. One interesting
thing was he didn't seem to realise that a lot of the tree-ring reconstructions use
density. Seemed to think they were all ring widths and there had to be moisture
changes we were not accounting for.
It is easy to respond to a Perspectives piece. Some of you did it with respect to
one of mine. I'm not sure it will achieve much - it won't come out before the paper
in the last week of April. I need to wait to se what he says. Our paper (me, Tim and
Keith) clearly says that the MWP couldn't have been warmer (for the NH average)
than the late 20th century.
Another possible reason for not doing anything is that the IPCC report will be out
soon. The summary is written in pretty clear language.

The above is my first thoughts, not having read the piece and just
got off the
flight back.

Best to ignore Woijcek. All he seems to want to do is deflect us into
responding.

Cheers
Phil

At 11:47 25/02/01 -0700, mhughes@ltrr.arizona.edu wrote:
>Dear all,
>WHat mechanism does "Science" have for repsonding to Perspective pieces? Most
>of the answer to Wally is contained within his own piece - he comments on the
>ambiguity of the record, which, in various ways, we have all done. What he
>doesn't offer, however, is anything other than an anecdotal alternative. As
>always, he seeks to damn (in this case with faint praise) the records or
>work
>that don't serve his purpose , and to elevate any scrap of evidence that does
>serve it. I think it will be important for us to stick closely to what we
>have
>written in published papers. CHeers, MAlcolm
>
>Quoting "Michael E. Mann" <mann@virginia.edu>:
>
> > Dear Phil, Ray,
> >
> > What do you guys think. If we're all on board, than an appropriately
> > toned,
> > "high road" response here might be appropriate. We don't want to engage
> > Wally in a personal battle, but simply should correct the record where
> > Wally has muddied it. Again, Phil et al do have a Science article in
> > press
> > that serves this purpose to some extent, so I'm especially interested in
> > what
> > Phil thinks (Phil?)...
> >
> > mike
> >
> > At 02:52 PM 2/24/01 -0700, mhughes@ltrr.arizona.edu wrote:
> > >Dear Mike et al., I think we should definitely let Wojick stew in his
> > own
> > >juice - as Mike pointed out to me the other day he, and his like, have
> > a
> > >specific agenda, and anything we write will be pressed into the service
> > of
> > that
> > >agenda. I'm not so sure about Wally. I share Tom's disinclination to
> > get
> > into a
> > >street fight with Wally - generally I take the view that life's too
> > short and
> > >uncertain for such activities. On the other hand, would we let such a
> > shoddy
> > >piece of work(and editing) go by if it were from another author? There
> > are so
> > >many holes in Wally's argument, and such a selective choice of evidence
> > that it
> > >should beggar belief. One of the more obvious holes is that he writes
> > of the
> > >Great Basin droughts of the 10th through 14th centuries as proof of

> > warmer
> > >conditions then, but doesn't explain why we don't have such conditions
> > now.
> > >Interestingly, Larry Benson, Dave Meko and others have good evidence
> > that
> > these
> > >same multidecadal periods were marked by a great excess of
> > precipitation
> > just a
> > >few hundred miles north in northern Nevada and California and southern
> > Oregon.
> > >He just hasn't grasped that the methods that are appropriate for
> > tracking the
> > >consequences of major changes in boundary conditions don't work in the
> > late
> > >Holocene. I've been trying to figure out the issue of "Was there a
> > Medieval
> > >Warm Period, and if so where and when" for a decade or so, and still
> > have the
> > >impression that the records for the 9th through 14th centuries are
> > extremely
> > >mixed. But then, I didn't come to the investigation with a certain
> > knowledge of
> > >the absolute truth, and have had to 'misfortune' to work with people
> > who let
> > >careful analysis get in the way - Henry Diaz, Ray and Mike, and others.
> >
> > >Anyway, the point of this rant is that I think we should give careful
> > >consideration to making a measured response to Wally. Cheers, Malcolm
> > >
> > >
> > >
> > >
> > >Quoting "Michael E. Mann" <mann@virginia.edu>:
> > >
> > >> Hi Tom,
> > >>
> > >> Thanks for your quick reply. I agree with you entirely. I think its
> > very
> > >> unfortunate he's chosen to disinform the community rather than engage
> > in
> > >> a
> > >> constructive dialogue (we tried the latter w/ him in a series of
> > emails
> > >> last
> > >> year, but clearly to no avail).
> > >>
> > >> On the other hand, think that a war of words w/ Broecker would be
> > >> exploited
> > >> by the skeptics, and perhaps we should just try to let this thing
> > die...
> > >>
> > >> I'm not sure. I'd appreciate knowing what others think?
> > >>
> > >> mike
> > >>
> > >> At 10:25 AM 2/24/01 -0600, tom crowley wrote:
> > >> >Mike,
> > >> >
> > >> >I was not aware of the Broecker piece - I am dismayed but not
> > >> surprised. I
> > >> >do not know what to do - I personally cannot stand the combative
> > >> personal

> > >> >approach Broecker relishes but it does seem as if some rebuttal is
> > >> called
> > >> >for. Maybe you Ray Phil I and Malcolm could pen a response - we are
> > >> >heading to Germany in a week, for a month, so I am not sure how much
> > I
> > >> can
> > >> >keep up on this but it seems as if some response is called for.
> > >> >
> > >> >What think ye?
> > >> >
> > >> >Tom
> > >> >
> > >> >
> > >> >>Dear Mike,
> > >> >>
> > >> >>Thanks for passing this along.
> > >> >>
> > >> >>Wojick of course completely misrepresents Broecker, and puts his
> > >> >>conventional intellectually dishonest spin on this.
> > >> >>
> > >> >>That having been said, it is a bit disappointing that Wally
> > continues
> > >> to
> > >> >>cling to some of his flawed beliefs which aren't supported from
> > either
> > >> our
> > >> >>best current understanding of the observations or of the results of
> > >> careful
> > >> >>modeling experiments. My own perception is that the climate
> > community,
> > >> >>modelers as well as observationalists, simply don't take seriously
> > >> anymore
> > >> >>the idea that the history of climate change over the past 1000
> > years
> > >> is
> > >> >>part of an internal oscillation. The sediment core evidence oft
> > cited
> > >> by
> > >> >>Broecker (e.g. Bond et al) for this is tremendously weak, and I, as
> > >> well as
> > >> >>the vast majority of my colleagues, simply don't buy it for even a
> > >> second.
> > >> >>But people don't like to challenge Broecker publically. He can and
> > >> will
> > >> >>play hardball.
> > >> >>
> > >> >>There is an odd irony. Broecker refused to accept the modeling
> > >> evidence
> > >> >>that the 100 kyr ice age Pleistocene variations were part of an
> > >> internal
> > >> >>oscillation paced by insolation variations, favoring instead the
> > >> >>discredited notion that they were a direct response to (too weak)
> > >> >>eccentricity forcing, until the evidence became insurmountable
> > (from
> > >> my
> > >> >>adviser, Barry Saltzman, may he rest in piece, and people like Dick
> > >> >>Peltier). Ironically, Broecker then took credit for the very
> > >> proposition he
> > >> >>had fought w/ tooth and nail.
> > >> >>
> > >> >>Broecker is even more wrong, and unfortunately equally stubborn, in
> > >> this case.
> > >> >>And, again, the reason: because his pet theory, that climate

> > >> variability is
> > >> >>a simple millennial oscillation, is finally being challenged w/
> > hard
> > >> data
> > >> >>and hard facts.
> > >> >>
> > >> >>Broecker misrepresents the nature of that data that we and others
> > have
> > >> >>used, and misunderstands the source of the muted hemispheric trends
> > >> (there
> > >> >>*is* a hemispheric "medieval warm period" and "little ice age",
> > just
> > >> not of
> > >> >>the magnitude or the distinctiveness that Broecker imagines).
> > >> Individual
> > >> >>regions in our reconstructions, and Phils, and others, vary by
> > several
> > >> >>degrees C, ie, the proxies we use have no problem whatsoever in
> > >> resolving
> > >> >>high-amplitude temperature variations in the past. The problem is
> > that
> > >> when
> > >> >>we look at the different regions we find that periods of cold and
> > >> warm
> > >> >>often occur at very different times in different regions, and so in
> > a
> > >> >>hemispheric or global average, a lot of purely regional variability
> > >> cancels
> > >> >>out. The resulting trends are somewhat smaller. I remained
> > befuddled
> > >> as to
> > >> >>why Wally doesn't understand this point. Its been explained to him
> > >> time and
> > >> >>time again. Maybe he's just not listening, or doesn't want to
> > >> listen...
> > >> >>
> > >> >>In fact, Tom Crowley has clearly shown that the observed millennial
> > >> >>temperature reconstruction is precisely consistent w/ our
> > >> understanding of
> > >> >>*forced* climate change over the past 1000 years (solar changes,
> > >> volcancic
> > >> >>output, and recent greenhouse gas concentrations). There is, simply
> > >> put, no
> > >> >>room for a global millennial internal oscillation. Regionally, such
> > >> types
> > >> >>of climate phenomena, associated for example with changes in the
> > North
> > >> >>Atlantic ocean circulation, are supported by the observations. This
> > >> >>explains why, for example, European temperature variations are
> > >> somewhat
> > >> >>larger than those in other regions not effected so strongly by such
> > >> climate
> > >> >>processes.
> > >> >>
> > >> >>Other recent perspectives, by Ray Bradley and myself provide a far
> > >> more
> > >> >>balanced and nuanced (and less dogmatic or defensive) viewpoint.
> > I'm
> > >> not
> > >> >>sure a written response to Broecker is worthwhile (this is,
> > afterall,
> > >> a
> > >> >>"perspective" and everyone understands that a scientist may have a

> > >> flawed
> > >> >>perspective). If Wally wants this to be his legacy, so be it...
> > >> >>
> > >> >>Phil and others have a review article coming out in the near future
> > >> which
> > >> >>also provides a much more balanced perspective on the climate
> > changes
> > >> of
> > >> >>the past millennium, and will set the record straight once again
> > (good
> > >> >>timing Phil!). Science's embargo policy prevents me from saying
> > much
> > >> more
> > >> >>at this time, but if Phil or anyone else wishes to comment further,
> > >> I'd
> > >> >>encourage it.
> > >> >>
> > >> >>Well, I've still got some snow to shovel here in Charlottesville!
> > >> Happy
> > >> >>weekend to all,
> > >> >>
> > >> >>mike
> > >> >>
> > >> >>p.s. For those with electronic subscriptions, Broecker's latest
> > piece
> > >> can
> > >> >>be found here:
> > >> >>
> > >> >> PALEOCLIMATE:
> > >> >>   Was the Medieval Warm Period Global?
> > >> >>   Wallace S. Broecker
> > >> >>   Science Feb 23 2001: 1497-1499. [Summary] [Full Text]
> > >> >>
> > >> >>http://www.sciencemag.org/cgi/content/full/291/5508/1497
> > >> >>
> > >> >>While my previous perspective piece is here:
> > >> >>   CLIMATE CHANGE:
> > >> >>   Lessons for a New Millennium
> > >> >>   Michael E. Mann
> > >> >>   Science 2000 July 14; 289: 253-254. (in Perspectives) [Summary]
> > >> [Full
> > >> >>Text]
> > >> >>URL:
> > >>
> > >>http://www.sciencemag.org/cgi/content/full/289/5477/253?maxtoshow=&HIT
> S=10&h
> > >>
> > >>its=10&RESULTFORMAT=&author1=Mann&searchid=QID_NOT_SET&stored_search=&
> FIRSTI
> > >> >>NDEX=&fdate=10/1/1995&tdate=2/28/2001
> > >> >>
> > >> >>and Bradley's is here:
> > >> >>
> > >> >>   PALEOCLIMATE: Enhanced: 1000 Years of Climate Change
> > >> >>   Ray Bradley
> > >> >>   Science 2000 May 26; 288: 1353-1355. (in Perspectives) [Summary]
> > >> [Full
> > >> >>Text]
> > >> >>
> > >> >>URL:
> > >>
> > >>http://www.sciencemag.org/cgi/content/full/288/5470/1353?maxtoshow=&HI
> TS=10&

> > >>
> > >>hits=10&RESULTFORMAT=&author1=Bradley&searchid=QID_NOT_SET&stored_sear
> ch=&FI
> > >> >>RSTINDEX=&fdate=10/1/1995&tdate=2/28/2001
> > >> >>
> > >> >>>Dear Michael--The third point below has comments on the
> > controversy
> > >> >>>betweenyou and Broecker--I'd be interested in your response (did
> > >> Wally not
> > >> >>>just understand what your data show?).
> > >> >>>
> > >> >>>Mike
> > >> >>>
> > >> >>>Three Wojick Pieces on Climate Change.
> > >> >>>I've been busy busy.
> > >> >>>
> > >> >>>David
> > >> >>>
> > >> >>>FIRST, the latest issue of Insight Magazine includes a
> > >> point-counterpoint
> > >> >>>between measly old me and the great Robert Watson. Boy has he got
> > >> >>>credentials! Too bad he's wrong.
> > >> >>>
> > >> >>><http://www.insightmag.com/archive/200103143.shtml>
> > >> >>>
> > >> >>>Symposium: Do scientists have compelling evidence of global
> > warming?
> > >> >>>
> > >> >>>Yes: Rising sea levels worldwide and retreating Arctic glaciers
> > are
> > >> ominous
> > >> >>>signs.
> > >> >>>
> > >> >>>By Robert T. Watson -- chairman of the UN Intergovernmental Panel
> > on
> > >> >>>Climate Change, chief scientist at the World Bank and former chief
> > >> science
> > >> >>>advisor to the Clinton White House.
> > >> >>>
> > >> >>>No: Despite the overheated rhetoric, there is no new evidence of
> > >> warming
> > >> >>>
> > >> >>>By David E. Wojick -- covers climate policy for Electricity Daily
> > and
> > >> is a
> > >> >>>science adviser to the Greening Earth Society
> > >> >>><http://www.greeningearthsociety.org>, as well as Undereditor of
> > the
> > >> >>>Washington Pest <http://www.WashingtonPest.com>
> > >> >>>
> > >> >>>SECOND, the February 15 Eco-logic on-line has published "The Black
> > >> Hole of
> > >> >>>Global Climate Government" by David Wojick, my detailed attack on
> > the
> > >> >>>Framework Convention on Climate Change. It includes a lot of the
> > >> actual
> > >> >>>treaty language.
> > >> >>>
> > >> >>><http://www.eco.freedom.org/el/20010202/wojick.shtml>
> > >> >>>
> > >> >>>THIRD, here is a draft Electricity Daily article of mine. Seems
> > I'm
> > >> not the

> > >> >>>only one who thinks the IPCC is nuts.
> > >> >>>
> > >> >>>Climate Guru Kicks The Hockey Stick
> > >> >>>by David Wojick (dwojick@shentel.net)
> > >> >>>
> > >> >>>Global warming is natural and the recent warming is probably no
> > >> exception.
> > >> >>>This is the controversial argument made by prominent climatologist
> > >> Wallace
> > >> >>>S. Broecker in today's issue of Science.
> > >> >>>
> > >> >>>Broecker's bombshell bears the seemingly innocent title "Was the
> > >> Medieval
> > >> >>>Warm Period Global?" It may seem esoteric, but whether the
> > > apparent
> > >> warmth
> > >> >>>reported in Europe about 1000 years ago was global or simply local
> > > is
> > >> >>>becoming a central issue in climate science. What makes it
> > >> contentious is
> > >> >>>the recent claims by the United Nations Intergovernmental Panel on
> > >> Climate
> > >> >>>Change that the earth is warmer now than it has been for
> > > millennia,
> > >> and
> > >> >>>that therefore human carbon dioxide emissions are to blame.
> > > Broecker,
> > >> a
> > >> >>>leading figure at Lamont-Doherty Earth Observatory, Columbia
> > >> University,
> > >> >>>questions both IPCC claims.
> > >> >>>
> > >> >>>The focus of the debate is a 1000-year temperature reconstruction
> > >> known in
> > >> >>>climate circles as the "hockey stick". Produced in 1999 by M. E.
> > >> Mann, R.
> > >> >>>S. Bradley, M. K. Hughes, the long handle of the hockey stick
> > > shows
> > >> global
> > >> >>>temperatures for the first 8 centuries as basically unchanging,
> > >> followed by
> > >> >>>the sharply up-tilting blade of the last 150 years or so. The Mann
> > > et
> > >> al
> > >> >>>hockey stick is the central feature of the recently released IPCC
> > >> working
> > >> >>>group one Summary for Policy makers, which claims to embody the
> > > best
> > >> of
> > >> >>>climate science.
> > >> >>>
> > >> >>>Broecker does not like the hockey stick, nor the conclusions the
> > > IPCC
> > >> draws
> > >> >>>from it. He says " A recent, widely cited reconstruction (Mann's)
> > >> leaves
> > >> >>>the impression that the 20th century warming was unique during the
> > >> last
> > >> >>>millennium. It shows no hint of the Medieval Warm Period (from
> > > around
> > >> 800
> > >> >>>to 1200 A.D.) during which the Vikings colonized Greenland,
> > >> suggesting that

> > >> >>>this warm event was regional rather than global. It also remains
> > >> unclear
> > >> >>>why just at the dawn of the Industrial Revolution and before the
> > >> emission
> > >> >>>of substantial amounts of anthropogenic greenhouse gases, Earth's
> > >> >>>temperature began to rise steeply. Was it a coincidence? I do not
> > >> think so.
> > >> >>>Rather, I suspect that the post-1860 natural warming was the most
> > >> recent in
> > >> >>>a series of similar warmings spaced at roughly 1500-year intervals
> > >> >>>throughout the present inter-glacial, the Holocene."
> > >> >>>
> > >> >>>Broecker presents the evidence for a global Medieval Warm Period,
> > as
> > >> well
> > >> >>>as for a Little Ice Age from around 1300 to 1860, when the present
> > >> >>>temperature rise begins. He also argues that the "proxy" evidence
> > >> used by
> > >> >>>Mann et al, such as tree ring data, is ill suited to the time
> > period
> > >> and
> > >> >>>temperature variation -- less than one degree C -- in question.
> > >> >>>
> > >> >>>As he puts it, "In my estimation, at least for time scales greater
> > >> than a
> > >> >>>century or two, only two proxies can yield temperatures that are
> > >> accurate
> > >> >>>to 0.5 C: the reconstruction of temperatures from the elevation of
> > >> mountain
> > >> >>>snowlines and borehole thermometry. Tree ring records are useful
> > for
> > >> >>>measuring temperature fluctuations over short time periods but
> > cannot
> > >> pick
> > >> >>>up long-term trends because there is no way to establish the
> > >> long-term
> > >> >>>evolution in ring thickness were temperatures to have remained
> > >> constant."
> > >> >>>
> > >> >>>Broecker acknowledges that the proxy evidence is necessarily
> > somewhat
> > >> >>>"murky", but his conclusion is that "climatic conditions have
> > >> oscillated
> > >> >>>steadily over the past 100,000 years, with an average period close
> > to
> > >> 1500
> > >> >>>years... The swing from the Medieval Warm Period to the Little Ice
> > >> Age was
> > >> >>>the penultimate of these oscillations." The implication being that
> > >> some, if
> > >> >>>not all, of the present warming is the natural swing out of the
> > >> Little Ice
> > >> >>>Age, and that Mann et al, as well as the IPCC, are mistaken.
> > >> >>>
> > >> >>>
> > >> >>>--
> > >> >>>
> > >> >>>
> > >> >>>Dr. David E. Wojick
> > >> >>>President
> > >> >>>Climatechangedebate.org
> > >> >>>Subscribe to the free debate listserv at
> > >> http://www.climatechangedebate.org

> > >> >>>Non subscribers can follow the debate at
> > >> >>>http://www.eScribe.com/science/ClimateChangeDebate/
> > >> >>>
> > >> >>>
> > >> >>>
> > >> >>>
> > >>
> > >>____
> > >> >>   Professor Michael E. Mann
> > >> >>   Department of Environmental Sciences, Clark Hall
> > >> >>   University of Virginia
> > >> >>   Charlottesville, VA 22903
> > >>
> > >>____
> > >> >>e-mail: mann@virginia.edu   Phone: (804) 924-7770   FAX: (804)
> > >> 982-2137
> > >> >>   http://www.evsc.virginia.edu/faculty/people/mann.html
> > >> >
> > >> >
> > >> >
> > >> >
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> > >> >Dept. of Oceanography
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> > >> >979-845-6331 (alternate fax)
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> >   http://www.evsc.virginia.edu/faculty/people/mann.html
> >
> >

> >

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

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215. 0983280741.txt
###

From: PARRYML@aol.com
To: tgcia@meto.gov.uk
Subject: Proposed TGCIA meeting: 30th Mayto 1st June, Amsterdam
Date: Tue, 27 Feb 2001 08:32:21 -0500 (EST)

Dear TGCIAers:
A proposed date/place for the next TGCIA meeting is: 9.00 on Wednesday 30th
May to 14.00 on Friday 1st June at Shell International Bldg, Amsterdam. Rob
Swart and colleagues at WGIII TSU have kindly agreed to be local hosts.
I suggest this date after consulting with 9 TGCIA members present at WGII
plenary at Geneva last week. The window is narrow between IPCC and SUBSTA
meetings (the latter is now almost certainly delayed until mid June).
Please put this date in your diary, but also let me know of any major
conflict with IPCC/UNFCC-type schedules.
Unless I hear to the contrary(*let us say by Monday 5th March*), the
proposal is that this dates stands .
This meeting is particularly because top of the agenda from our last meeting
is consideration of developing a 'one-stop-shop' for data and guidance for
scenario-based climate impacts assessment, which would lay the foundations
for compatible research for the next IPCC assessment (whatever form it may
take). We might well also consider what recommendations to make concerning
the form of the next assessment (a subject probably on the agenda of the IPCC
London Plenary in September).
More follows next week, assuming these dates hold, about agenda and
arrangements.
Kind regards,
Martin parry

Prof. Martin L. Parry
Jackson Environment Institute
University of East Anglia
Norwich
NR4 7TJ

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Web: http://www.uea.ac.uk/env/jei

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###

##########

From: Phil Jones <p.jones@uea.ac.uk>
To: mann@multiproxy.evsc.virginia.edu
Subject: Fwd: RE: Science issue Feb 22/23
Date: Tue, 27 Feb 2001 10:14:09 +0000
Cc: mhughes@ltrr.arizona.edu,rbradley@geo.umass.edu,tom@ocean.tamu.edu,
k.briffa@uea.ac.uk,t.osborn@uea.ac.uk

<x-flowed>

Mike et al,
Sorry about the multiple sendings. I've forgotten my glasses and couldn't see I'd missed a comma.
Another thing to point to is the special issue of Climatic Change by Astrid Ogilbie and Trausti Jonsson. They point to the LIA not being very appropriate in Iceland.

Cheers
Phil

Mike,
So Julia handled it. Even she thought it was handwaving, but it passed the usual Science review process. Obviously this isn't great as none of us got to review it. Odd that she didn't send it to one of us here as she knew we were writing the article she asked us to ! Anyway that is water under the bridge.
As for authorship we have this article coming out so this rules us out. Tom isn't keen and he's away. Wally told me he didn't reckon Tom, so Tom has got the right vibes. Julia is asking us to go ahead and hinting at a joint response. One possibility is either you or Macolm taking the lead. Malcom and Henry wrote the MWP piece in Climate Change in 94. Keith and I think something pointed about the MWP is the way to go. Could add in that even the two warming periods in the 20th century don't show warming everywhere - especially the early 20th century.
Remember that we are all basically averaging long series together and if one site shows a big warming/cooling then the average will to a lesser extent. Also bring in a few of the papers where people have compared tree based reconstructions with glacial advances/retreats (eg Raper et al in J. Glaciology and Luckman et al in the Holocene. Also there are more in that Interhemispheric Linkages Book of Vera amd work by Ricardo Villalba and others).
Basically need to point to a load of literature that we would expect someone writing an article of this type to be aware of. Also the North Atlantic isn't the last word in NH and global averages. Clearly said in Hughes and Diaz and papers therein.

     Also the latest IPCC report will use and reference the latest curves,
but from
  1400 they are not that different from Bradley and Jones (1993), so why
the fuss now.
  Clearly the MWP is the issue that has got a few worked up, but we have
concluded
  nothing that couldn't have been gleaned in 1994. Maybe we're stating it
more clearly
  now, but the recent warmth of the 1990s is a factor as well.

  Cheers
  Phil

>From: "Julia Uppenbrink" <Juppenbrink@science-int.co.uk>
>To: "Phil Jones" <p.jones@uea.ac.uk>
>Subject: RE: Science issue Feb 22/23
>Date: Mon, 26 Feb 2001 17:05:45 -0000
>X-Mailer: Microsoft Outlook IMO, Build 9.0.2416 (9.0.2910.0)
>Importance: Normal
>
>Dear Phil
>
>Thanks for your message regarding Wally Broecker's Perspective. I am of
>course aware of this Perspective coming out - I did handle it - I realized
>that it was perhaps a bit handwaving in parts but I thought the message was
>interesting and the article passed the usual screening. But we are always
>open to criticism! So please do send a letter to us; you can send it
>directly to me, and you may cowrite it with Tom Crowley and Mike Mann or you
>can send separate letters (if the concerns overlap a lot then one letter is
>perhaps better than several). The letter will be handled through our letters
>department, and we will get a response from Wally plus possibly outside
>review before we make a decision to publish.
>
>I look forward to receiving your letter.
>
>Best wishes
>
>   Julia
>
>-----Original Message-----
>From: Phil Jones [mailto:p.jones@uea.ac.uk]
>Sent: 26 February 2001 14:40
>To: Julia Uppenbrink
>Subject: Science issue Feb 22/23
>
>
>
> >
> > Dear Julia,
>   I don't know if you have seen the Perspectives piece in last
>week's issue of
>   Science by Wally Broecker. I guess it was nothing to do with you and it
>contains
>   several inaccuracies and sweeping statements. I accept it is a personal
>view
>   and I've not seen the issue yet , only a copy that I was ironically given
>by Wally
>   Broecker as we were both guest speakers at a meeting at Bowdoin College,
>ME
>   on Saturday. I got back this morning to Norwich.
>   I talked to Wally about it over the weekend and will send him a few
>reprints
>   pointing out a few of the things he should have read. Some things he

>states are just
>   wrong.
>   I don't want to change the article already accepted, but what are
>the possibilities
>   of writing a response to Wally's piece in a later issue. I've been
>contacted by a couple
>   of people in the US about Broecker's piece (Mike Mann and Tom Crowley),
>who are
>   quite unhappy about it and would like to respond. They both know about
>the invited
>   piece and wanted me to comment, hence my email to you. The invited piece
>does
>   address some of the issues, but not the link between high and low
>frequency
>   proxy series.
>
>   Best Regards
>   Phil
>
>
>
>
>
>Prof. Phil Jones
>Climatic Research Unit   Telephone +44 (0) 1603 592090
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>Norwich   Email   p.jones@uea.ac.uk
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>--

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217. 0983452785.txt
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##########

From: "Thomas L. Delworth" <td@gfdl.noaa.gov>
To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
Subject: Re: letter to Science
Date: Thu, 01 Mar 2001 08:19:45 -0500
Cc: tom@ocean.tamu.edu, hpollack@geo.lsa.umich.edu, mhughes@ltrr.arizona.edu, rbradley@geo.umass.edu, p.jones@uea.ac.uk, k.briffa@uea.ac.uk

Dear Mike et al,

   I offer the following comments on your letter
for your consideration.

   It seems to me there are 2 primary issues to
address:

   (A) what does proxy evidence say about whether
the Medieval Warm period was global
   (B) what do we know about potential mechanisms
for the Medieval Warm period
      (i) evidence for a forced phenomenon
      (ii) evidence for internal variability

Issue (A) is currently dealt with in your sections (1) and (2). One point that could be perhaps conveyed more clearly is the necessity of using the spatial information conveyed in (multi) proxy reconstructions, rather than overly interpreting sets of local proxy evidence. I felt this point could have been stressed more, and is one which the casual reader may not appreciate.

Issue (B, Bi) is in your section (3). I suggest a more explicit mention of conclusions with regard to the Medieval Warm period in recent work on this topic. The first statement in this section doesn't provide (I don't think) explicit evidence to back itself up. The sentence starting "These results ..." could be more explicit about what those studies show with respect to the Medieval Warm period, in addition to the more general statement about the partitionng between forced and internal variability. A reader could ask "Ok, if 50% of the variability is explained by volcanic and solar forcing, that doesn't exclude the other 50% playing a strong role for events such as the Medieval Warming." Such a question could be dealt with in advance by stating what role these studies suggest for radiative forcing in the Medieval Warm period.

For issue (Bii), I would suggest being explicit that it is incumbent upon authors to provide some evidence to support their speculation. What evidence can the author provide to support his speculation concerning the role of the THC in the Medieval Warm period? Rather than explicitly stating this is not a likely mechanism, I would contrast the speculation he has offered on this topic to the stronger (in my opinion) evidence provided by modeling studies to support the idea of the importance of radiative forcing.

... a few more minor comments

(1) I agree with the overall message you are conveying, but might choose somewhat differing wording in a place or two. The statement is made "(1) It cannot reasonably be argued that the Middle Ages were as warm as the 20th century at global or hemispheric scales." This might be a bit strong ... I would think one can have a reasoned discussion on this topic. Perhaps something like "We strongly disagree with the assertion that the Middle Ages were as warm as the 20th century at global or hemispheric scales."

(2) In the second to last sentence, I would add the qualifying phrase "on planetary scales" after the text "... responsible for centennial-millenial changes ...".

Regards,
Tom Delworth

ps The central issue is one that I have not been heavily
involved in, and thus don't think it's appropriate for
me to sign on as an author. Good luck, and please
send me a copy of your final submission.

pps I previously provided to Tom correlations between the
THC and global/hemispheric temperature based on a 900 year
run of our R30 coupled model. These correlations were
relatively low (0.27), but probably significant. The
applicability of those correlations to the issue of the
Medieval Warming may not be strong. If the Medieval Warming
is a multi-century event, then I should really be looking at
the correlations of low frequency (>50 years) filtered model
output from a run of several millenia duration. Thus, the
900 year run may not be applicable. I will revisit this
topic using a multi-millenial R15 coupled run, but probably
won't have any results today. I don't think that would
change the essential conclusions, however. I recall that
experiments with the R15 model in which the THC was substantially
weakened through the addition of fresh water to the North
Atlantic provided strong regional temperature anomalies, but
their global expression was small. These experiments are
being repeated with the higher resolution model.
   In light of these issues, I suggest that the focus be
not so much on saying the THC cannot be responsible for the
Medieval Warming, but rather on saying (1) there is strong
evidence for a substantial role of radiative forcing, and (2) the
burden is on the author to provide evidence for the role of
the THC.
?

"Michael E. Mann" wrote:

> Dear Colleagues,
>
> Below is a draft of a short letter to Science that Tom Crowley and I
> have put together, after discussing w/ Phil, Ray, and Malcolm. We
> feel that a reply to Broecker's recent "Perspectives" piece is
> warranted to correct several misconceptions that Wally unfortunately
> chose to perpetuate (attached as an html file FYI). We have been given
> encouragement to submit this by Julia Uppenbrink at Science.
>
> We are working under a very tight timeline owing to Tom's travel
> schedule (leaves on an extended travel on friday) so we would greatly
> appreciate it if you could respond ASAP w/ comments, suggestions, etc.
> Please note that we are currently near the length limitations (and
> probably shouldn't include more than 15 references) so we're looking
> to sharpen and hone, but not lengthen the piece at this point.
>
> Thanks in advance for your feedback,
>
> mike
>
> ____
>
> Medieval Warming Redux
> In a recent "Perspectives" opinion piece, W. Broecker suggests that
> the
> "hockey stick" reconstruction of climate change over the past 1000
> years -
> with extreme warming only in the late 20th century - is incorrect, and

> that
> the so-called "Medieval Warm Period" was at least as warm as the 20th
> century and due to oscillations in the thermohaline circulation. To
> reach
> this conclusion, Dr. Broecker rejects traditional empirical "proxy"
> climate
> indicators of past climate (e.g. tree ring, ice core, coral, and long
> historical documentary records) that are the foundation of a number of
>
> hemispheric reconstructions, as well as our current best physical
> understanding of the factors controlling climate at
> century-to-millennial
> timescales. We disagree with Broecker on several major points:
> (1) It cannot reasonably be argued that the Middle Ages were as warm
> as the
> 20th century at global or hemispheric scales. Although regional warmth
>
> during the Middle Ages may have sometimes been significantly greater
> than
> present, four different hemispheric-scale reconstructions (Jones,
> Mann,
> Briffa, Crowley) have been completed for the last 1000 years -- all of
> them
> showing warmth in the Middle Ages that is either no warmer or
> significantly
> less than mid-20th century warmth. This is because it has been known
> for a
> quarter of a century that the timing of warmth during the Middle Ages
> was
> significantly different in different regions (Lamb, Dansgaard,
> Hughes).
> Failure to take this observation into account can lead to serious
> errors in
> the inference of hemispheric temperature trends. Although one analysis
> of
> heat flow measurements suggests warmer temperatures than the surface
> proxies during the Middle Ages (Huang and Pollack, GRL. 1997), the
> considerable sensitivity of the resulting trends to a priori
> statistical
> assumptions has lead borehole researchers to restrict their attention
> to
> the more reliably interpretable temperature fluctuations during the
> past
> five centuries (Huang and Pollack, Nature). Our conclusion is also
> supported by measurements from tropical glaciers indicating an
> unprecedented level of recent warming with respect to the last
> 1,000-2,000
> years (Thompson).
> (2) High-resolution proxy climate records which form the foundation of
>
> recent hemispheric temperature reconstructions are far more reliable
> indicators of century-to-millennial scale climate variability than is
> implied by Broecker. The potential limitations in interpreting
> long-term
> climate change from proxy indicators such as tree rings, have been
> long
> recognized by dendroclimatologists (e.g., Cook "segment curse" paper)
> and
> are almost always taken into account in framing interpretations of
> long-term trends. For example, Mann et al (1999) verified that a
> significant subset of multiple-millennial length tree ring and ice
> core
> proxy climate indicators used to reconstruct the trend over the past

> millennium passed rigorous statistical tests for fidelity at the
> millennial
> timescale, and that the basic attributes of the hemispheric
> reconstruction
> using more recent non-tree ring proxies available over the past few
> centuries yielded essentially the same result as that based on both
> tree
> ring and non-tree ring based information (Mann et al, Earth
> Interactions,
> 2000). Several independent reconstructions (Jones et al and Crowley
> and
> Lowery), using a wide variety of proxy climate indicators and
> different
> statistical approaches, yield similar hemispheric temperature trends.
> Even
> the centennial-scale changes within the so-called "Little Ice Age" of
> the
> 15th-19th centuries are largely in agreement. Furthermore these
> centennial
> changes have been shown to be in "agreement" , rather than "in
> opposition"
> (as argued by Broecker) with evidence from alpine glacial advances
> (Raper
> reference).
> (3) Physical considerations show that external forcing, not internal
> variability, played the dominant role in the transition from the
> "Medieval
> Warm Period" to "Little Ice Age" (these terms are used loosely and
> are, in
> fact, ill advised in the context of hemispheric or global temperature
> changes -see e.g. Bradley and Jones, 1993; Hughes and Diaz, 1994). One
> of
> the major points of Broecker's argument is that changes in the
> thermohaline circulation are a primary driver of climate change on
> this
> time scale. These results do not consider recent modeling studies
> (Free,
> Crowley) that demonstrate at a high significance level (>99%) that
> about
> 50% of the pre-anthropogenic (pre-1850) variance can be explained by
> changes in volcanism and low frequency solar irradiance. Although the
> latter term is still not well constrained from observational studies,
> there
> are a number of independent lines of evidence suggesting such changes
> (Hoyt, Lean, Lockwood).
> (4) It is not justifiable to argue that changes in the thermohaline
> circulation cause significant hemispheric or global changes in
> temperature.
> Although changes in the conveyor play a major role in the Atlantic
> Basin,
> to a first approximation changes in ocean circulation simply
> redistribute
> heat on the planet without significantly raising global temperature,
> or
> even hemispheric temperature. This conclusion is born out by very low
> correlations between warmth in the Greenland sector and the
> hemispheric
> indices over the last 1000 years (Crowley footnote ref.), a low
> correlation
> that is shared by coupled model experiments (Delworth citation)? In
> fact,
> sediment core data from the subtropical North Atlantic often cited as
> indicative of a distinct "Medieval Warm Period" and "Little Ice Age"

> (Keigwin Sargasso Sea), has recently been shown to be more consistent
> with
> changes in the North Atlantic Oscillation (Keigwin and Pickart),
> implying a
> zero sum pattern of regionally alternating warm and cold superimposed
> on
> far more modest hemispheric variations over the past 1000 years. This
> pattern itself may be forced, rather than internal in nature, and
> would
> explain the limited evidence for more dramatic cold and warm periods
> in
> regions such as Europe (see Mann, Sci Perspective, 2000).
> The above arguments lead us to conclude that, although the conveyor
> may be
> changing, radiative forcing perturbations were primarily responsible
> for
> centennial-millennial changes in the last 1000 years, with attendant
> implications for interpretation of earlier Holocene oscillations (e.g,
>
> Denton and Karlen). Furthermore, the weight of evidence indicates that
> the
> late 20th century hemispheric warming is significantly greater than
> the
> Middle Ages.
>
> Michael E. Mann
> Thomas J. Crowley
> WHO ELSE???
>
> ___________
> ____
>   Professor Michael E. Mann
>   Department of Environmental Sciences, Clark Hall
>   University of Virginia
>   Charlottesville, VA 22903
>
> ____
>
> e-mail: mann@virginia.edu   Phone: (804) 924-7770   FAX: (804)
> 982-2137
>   http://www.evsc.virginia.edu/faculty/people/mann.html

--
             Thomas L. Delworth
GFDL/NOAA   e-mail: td@gfdl.gov
P.O. Box 308   Phone: 609-452-6565
Princeton, NJ 08542 USA   FAX: 609-987-5063

218. 0983552403.txt
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##########

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: tom crowley <tom@ocean.tamu.edu>
Subject: Re: Science letter
Date: Fri, 02 Mar 2001 12:00:03 -0500
Cc: "Raymond S. Bradley" <rbradley@geo.umass.edu>, mhughes@ltrr.arizona.edu,
k.briffa@uea.ac.uk, tom@ocean.tamu.edu, p.jones@uea.ac.uk, td@gfdl.noaa.gov,
hpollack@geo.lsa.umich.edu

<x-flowed>
Thanks for clarifying Tom,

Yes, these are my sentiments as well, and I would conditionally sign-on to this effort. In the meantime, I think there is a lot of good science to be done!

mike

At 10:53 AM 3/2/01 -0600, tom crowley wrote:
>Dear All,
>
>A few more comments re Mikes note - Mike and I thought that if we cannot
>make a case to our colleagues, why muddy the waters further (as either
>Keith, Malcolm, or Ray said)?
>
>That said, I don't think this has been wasted time. I still think a
>thoughtful short paper on the subject of Holocene climate change wold be
>useful, this time stating it from OUR perspective (i.e., not focusing
>exculsively on Broeckers message). By broadening this it may be more
>interesting; we could also include a couple of figures and maybe add some
>input from Tom Delworth and Henry Pollack. I would be willing to take a
>crack at this, and if anyone wants to CONDITIONALLY sign on, I would be
>more than happy to include you.
>
>I probably would not begin this until late April, after our trip to Germany
>and the meeting in Virginia.
>
>Tom
>
>ps fyi I counted the average spacing between the warm and cold
>oscillations in the iron oscillations illustrated by Broecker. Regardless
>of whether warm or cold are used, the mean spacing is indeed 1.5 k,
>although the s.d. is 0.4k HOWEVER, the mean spacing between the four main
>warm phases illustrated by Broecker on the same figure is, believe it or
>not, 2.15! much closer to the solar peak. This calls to mind the
>interesting (and clever) Wigley and Raper paper in Proc. Roy. Soc. (1990)
>indicating that, given the uncertainties in chronology, solar forcing plays
>a role i n Holocenn climate change. It therefore seems that the conveyor
>is indeed oscillating but the time scale of the larger scale CLIMATE shifts
>may be more regulated by solar, with volcanism adding some stochastic
>contribution. Something like this is worth adding to the proposed Eos
>piece.
>
>Tom
>
>
>
>Thomas J. Crowley
>Dept. of Oceanography
>Texas A&M University
>College Station, TX 77843-3146
>979-845-0795
>979-847-8879 (fax)
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____
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____

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##########

From: Chick Keller <ckeller@igpp.ucsd.edu>
To: "Michael E Mann" <mann@virginia.edu>, <rbradley@geo.umass.edu>, "Phil Jones" <p.jones@uea.ac.uk>, <k.briffa@uea.ac.uk>, tom crowley <tom@ocean.tamu.edu>, "Jonathan Overpeck"<jto@u.arizona.edu>, Tom Wigley <wigley@ucar.edu>, Mike MacCracken <mmaccrac@usgcrp.gov>
Subject: Some thoughts on climate change proxy temperatures in the last 1,000 yrs
Date: Fri, 2 Mar 2001 15:54:57 -0800

<x-rich>Folks,

Two points here:

1. I read with some consternation Wally Broecker's latest piece in Science (23Feb. 2001). First you can all take up some other topic since Wally says only Boreholes and treeline changes are accurate enough to do low frequency trends. What does he mean by "only two proxies can yield temperatures that are accurate to 0.5°C"? and do you agree that tree rings and sediments, etc are not sufficiently accurate to exhibit correct low frequency trends?

2. Here are some references to recent Holocene time-frame records you probably have seen, but just in case... I found them interesting without knowing how good or representative they are.

Surprisingly they were given me by one who cited them as examples of evidence for a MWP and LIA. I read them differently but they caused me to consider one question I hadn't heard discussed (see below).

Based just on these four, one comes to the following tentative conclusions and observations:

Conclusions:

*MWP was a generally warm time interspersed with coolings and not well synchronized hemispherically or globally.

*LIA was global and capable of better (but not completely) synchronized large amplitude variations

*20th Cent. was the only time when all records agree (tree ring problems with CO2?)

MIGHT THIS RELATIVE UNIFORMITY BE USED AS A CHARACTERISTIC OF 20TH CENTU WARMING THAT SETS IT APART FROM PREVIOUS CLIMATE CHANGES?

*Borehole inversion is too smoothed to be of much use but it does indicate a larger temp amplitude if it weren't smoothed. ~1.5°C

And this brings up my question. How one averages these records.

One way would be to note that the temperature amplitude (1000 - 1950) for each is ~1.5°C. Thus you could conclude that hemispheric/global climate varied ay over a degree Celcius (although with regional differences)

Another way would be to average the records. The resulting temperature amplitude would be smaller because extremes would cancel since variability is large and each region's extremes occur at different times.

Thus, if people simply looked at several records they would get the impression that temperature variations were large, ~1.5°C. Imagine their surprise when they see that the ensemble averages you publish have much smaller amplitude.

Comparison of amplitudes is given below (although difficult to do since amplitude depends on averaging so these are very approximate).

Approximate Temperature Amplitudes for period 1000-1950

Mann et al 1999	~0.5
Jones et al 1998	~0.8
Crowley and Lowery	???
Briffa 2000	???
Dahl, Jensen	~1.5
Huang, et al	~0.8 (500 yrs only)
Overpeck et al	~1.3 (400 yrs, polar only)
Bradley & Jones(93)	~0.7 (600 yrs only)

(Not surprising that the contrarians take great exception to Michael's small amplitude.)

<bold><underline>This is important in the current debate even, it would appear, with people like Wally.</underline></bold> I have been looking for what the real issue is between researchers like yourselves and skeptical scientists. Politics and agendas aside, I think it is close to this.

Anyone looking at the records gets the impression that the temperature amplitude for many individual records/sites over the past 1000 years or so is often larger than 1°C. They thus recognize that natural variability is unlikely to generate such large changes unless the sun is having more effect than direct forcing, or there is some fortuitous but detectable combination of forcings. And they see this as evidence that the 0.8°C or so temperature rise in the 20th century is not all that special.

The community, however, in making ensemble averages gets a much smaller amplitude ~0.5°C. which they say shows that reasonable combinations of solar direct plus volcanos and internal variability with the help of THC can indeed explain this AND the 20th century warming is unique.

Thus, the impass--one side pointing to large temperature variations in many records around the globe and the other saying "yes, but not synchronous and so averaged hemispherically no big deal.

But, just replying that lack of synchronous events (sometimes by a few decades) is the reason might not be enough. It seems to me that we must go one step further. We must address the question: what forcings can generate large amplitude temperature variations over hundreds of years, regional though they may be (and, could these occur at different times in different regions due to shifting heat inertia patterns)? If we can't do this, then there might be something wrong with our rationale that the average is low amplitude even though many regions see high amplitude. This may be the nubbin of the disagreement, and until we answer it, many careful scientists will decide the issue is still unsettled and that indeed climate in the past may well have varied as much or more than in the last hundred years.

(Also, I note that most proxy temperature records claim timing errors of +-50 years or so. What is the possibility that records are cancelling each other out on variations in the hundred year frame due simply to timing errors? as in hitting or missing C&L's triple warming peak 1000-1200 AD)

Regards,

Referendes to proxy temp records

<excerpt>(1) Bodri, L. and V.Cermak Climate change of the last millennium inferred from borehole temperatures: Regional patterns of climatic changes in the Czech Republic - Part III, Global and Planetary Change, 21, 225-235. 1999

As with other borehole data the record is incredibly smoothed. It has essentially three warming features.

from 1000 to after 1500 there is a broad warming pulse;

1550-1750 cooling

1750-1850 warming

1850-1900 cooling

1900-1950? rapid warming   <underline>Total amplitude ~1°C (1.5°C if not smoothed?)

</underline>

I don't know what to make of the more than 500 year warming pulse.
Most records show warming either in the 1100's or 1200's but usually not both.

The rest of the record looks reasonable given the smoothing.

</excerpt>

<excerpt>

</excerpt>(2) Filippi , M.L., Lambert, P., et al, Climatic and anthropogenic influence on the stable isotope record from bulk carbonates and ostracodes in Lake Neuchatel, Switzerland during the last two millenia, Jour. of Paleolimnology, 21, 19-34, 1999

<excerpt>   Graph actually begins at 805 AD (all dates are advertised as +-50 yrs)

Starts out warm but already cooling which it does till about 1150.

warms till 1242, second peak 1298 then cools to minimum at 1500

warms significantly to 1600 then cools to about half of 1500 max and essentially stays that way till 1850 when cools to 1500 level again and immediately rebounds

into 1950s and still warming.   <underline>Total amplitude ~2.5°C</underline>

</excerpt>(3) Naurzbaev, M.M. and E.A.Vaganov,Variation of early summer and annual temperature in east Taymir and Putoran (Siberia) over the last two millennia inferred from tree rings, JGR 105, 7317-7326, 2000

Interesting record.

<excerpt>moderately cool 800-950,

rapid warming to max 1000 dip ~1050, recovers till ~1180

cools fast to minimum ~1250,

</excerpt> warms to max   ~1400

cools to 1450 slight cooling till 1700

<excerpt>warms to   ~1780

rapid cooling to   ~1830

rapid warming till   ~1930   <underline> Total Amplitude ~1.5°C</underline>

</excerpt>(4) Wilson, A.T., Hendy, C.H. and Reynolds, C.P., Short-term climate change

and New Zealand temperatures during the last millennium, Nature 1979,

<excerpt>   315-317,

Used stalagmites (delta 18 O proxy)

</excerpt>   This is a strange record, but the authors compare it favorably with the

<excerpt>   central England record.

1100 starts and warms in two pulses one at 1250, min at 1300, big max at 1400, followed by dive to minimum 1450

rises to max 1500

drops to min 1600

rises a bit 1700 and into 1850

drops to minor min1880 rises after that   <underline> Total Amplitude ~1.5˚C</underline>

STRANGE RECORD

</excerpt>

Charles. "Chick" F. Keller,

IGPP.SIO.UCSD - Attn: Chick Keller

9500 Gilman Drive

La Jolla, CA 92093-0225

(858) 822-1510 office

(858) 456-9002 home

Is the noticeable increase in surfers off Scripps Beach a possible

indication of global warming?

</x-rich>

220. 0984598451.txt
##

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: verification results
Date: Wed, 14 Mar 2001 14:34:11 -0500
Cc: srutherford@virginia.edu, mann@virginia.edu

HIi Tim,

That all sounds great, and indeed, the 19th century will be a *hot* topic
(pun intended) as we try to rectify Tom's model response w/ the
instrumental record and proxy reconstructions. Ironically, the 19th century
is one in most dispute over the past millennium, it seems!

You accurately summarize what my understanding is of the breakdown of lead
roles. I don't see any reason for changing that. I think Scott and I will
have our hands full w/ the other items, so if you can take the lead role on
the MXD paper (comparing the two methods, etc.) that would be great.

My intention is to give you and Scott full credit for anything I show at
meetings that is a result of mutual collaboration. Of course, both of your
are co-authors of my EGS talk.

So all sounds great! Scott: when Tim sends revised plots, can you prepare
some revised ppt files and let me know when they are available to download?
Hope to get all this straightened away next week after I return from the
frozen north (Michigan)...

mike

That soundsAt 07:05 PM 3/14/01 +0000, Tim Osborn wrote:
> >Thanks alot, these look good. I think we're really making some good
> >progress now.
> >
> >Just to confirm, my understanding is that you're next working on a similar
> >plot showing the
> >comparison of the REG-EM results w/ the straight gridbox age-banded
> >estimates you and Keith have produced over the longer period (ie, back to
> >1600 or so?). It would be great to be able to show those at EGS.
>
>Mike, you're welcome to show these results at EGS. I had to leave early
>today (Wednesday) as my wife was ill, but I'll be back at work tomorrow.
>What I'll do first is just to modify the figures I've already sent to you,
>comparing the verification REG-EM run with instrumental data over the
>1856-1900 period. What I want to do is to modify the final map so that the
>grid boxes that actually have tree-ring sites in them are highlighted in
>some way. Then we can visualise more clearly whether the 'local'
>information is much better than the 'non-local' information. I was in a bit
>of a hurry with my e-mail earlier, I didn't mention that the map is based on
>all those grid boxes with at least 20 years of instrumental data during the
>1856-1900 period. I found the year-by-year pattern correlations quite
>informative too, and was particularly impressed by the fact that there were
>no really poor years! (at least that's my recollection, not having the plots
>in front of me at home).
>
>Having modified the map as described, I'll repeat the analysis but comparing
>the 1404-1855 period of the full reconstruction from REG-EM with our
>existing year-by-year maps and quasi-hemispheric averages. I shall compare
>them against our "traditionally-standardised" version, since it would be
>unfair to compare them with the age-banded version. The year-by-year maps
>we have already got are calibrated on a grid-box by grid-box basis
>(individually) using simple linear regression between the density series and
>the instrumental temperature. This gives us coverage for those grid boxes
>with density data in them. We throw away those that do not correlate
>significantly with their local grid box temperature. That leaves around 100
>boxes, with fewer further back in time. We then try to reconstruct all
>remaining northern hemisphere grid boxes, using principal component
>regression (PCs of the calibrated density used as predictors on a grid box
>by grid box basis), but only actually retain those that have significant
>correlations during an independent verification period. So we gain quite a
>few more grid boxes, again time-varying. So we have this (perhaps rather

>odd!) combination of local regression plus principal component regression
>producing our maps. I shall use this set of year-by-year maps for the
>comparison with REG-EM, though as with the instrumental temperatures, I'll
>sometimes highlight or subsample just those with trees in (i.e. those
>locally-calibrated).
>
>Our original plan for carving up the analysis/papers was for me to take the
>lead on the comparison of methods with the same data set, Scott on the
>comparison of data sets with the same method, and Mike to concentrate on the
>19th century stuff including verification against the instrumental data etc.
> I saw Tom Crowley last week and he showed some results indicating how
>critical the 19th century is for getting a good match between his forced
>model results and the various proxy reconstructions - so the 19th century
>could certainly be a hot topic. Phil Jones would be useful here as he may
>know of more early instrumental data from Europe that might help (depending
>upon homogeneity!). Anyway, I'm refreshing our minds about the 3-way split
>of work because: (i) this might be an appropriate point to confirm that such
>a split is still the best way to go (I'm still happy with it); and (ii) to
>point out that the REG-EM comparisons with our existing density-based maps
>falls into the bit that I'm to take the lead on - so while I'm completely
>happy for you to show these at EGS or other meetings, I'd still like to
>write the comparisons up for a journal paper.
>
> >p.s. Tim: are you going to be at EGS? I know Phil will...Also, I'm hoping
> >that one of the 3 of you can make it to the Charlottesville workshop in
> >April. You and Phil have both indicated you can't go, I think? At present,
> >Keith hasn't yet confirmed. It would be a shame not to have him, you, or
> >Phil present. Can you suggest some sort of "alternate" (Schweingruber?) the
> >Europeans might invite if Keith can't make it. Thanks...
>
>I can't make it to EGS, as I have work to prepare for my 3 talks I'm giving
>at NCAR in the first week in April! For the Charlottesville workshop, I
>spoke to Keith yesterday and I think he has now booked his flights - so I'd
>take that as confirmation. He's in touch with Julie Jones at GKSS about it.
> I put in a good word about how pleasant Charlottesville was!
>
>Best regards to you both,
>
>Tim
>
>
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From: Tim Osborn <t.osborn@uea.ac.uk>
To: mann@virginia.edu, srutherford@virginia.edu
Subject: Re: verification results
Date: Thu Mar 15 16:38:31 2001

Mike & Scott,

I've redone the verification against instrumental temperatures for 1856-1899. Previously I'd used 1856-1900, but I've now realised that 1900 is not part of the verification period (the pattern correlation = 1 gave it away!). So I've now stopped in 1899. It makes virtually no difference to the quasi-hemispheric series and their correlations. What it does affect is the grid-box by grid-box temporal correlations, since I was previously using one perfect value at the end of each series. So the correlations are mostly a bit lower now, though still fairly good I think. There's a reasonable area with r > 0.3. Signal to noise should increase fairly dramatically if some kind of regional averaging were done. I've outlined the boxes that actually have chronologies in them. There's not enough instrumental data to verify the more northern ones, but the European and USA ones do well (r in range 0.5 to 0.9). The more distant oceanic regions are a bit poorer, excep the northern Indian Ocean. So that's it for the verification, for the moment.

I've compared the 1404-1855 (i.e., pre-instrumental) reconstruction with the Briffa et al. and Osborn et al. reconstructions. Correlations are all quite high (0.7 to 0.85) for the quasi-hemispheric series, while the pattern correlations average around 0.6. The box-by-box temporal correlations show many boxes with r in the range 0.6 to 1.0, indicating little sensitivity to the method used. One notable feature of the latter results is that there's less agreement in the boxes that actually have trees than those don't! There's two different interpretations of this that I'm working on, which seem equally possible. More later. I was going to send the time series and maps from this comparison, but I've just realised that I'm using anomalies from two different baselines (1961-90 for ours, 1901-60 for REG-EM) so the % variance explained and the time series aren't right - that'll have to wait till Friday now.

Tim

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From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: comparison with our existing reconstructions
Date: Fri, 16 Mar 2001 14:25:57 -0500 (EST)
Cc: Scott Rutherford <srutherford@virginia.edu>, Mike Mann <mann@virginia.edu>

Dear Tim, Scott

On the road w/ tenous email connection so have to be brief. This sounds
good. Hoping we can have age-banded connections by the end of tnext week
so I can show in Nice! Scott: can you rectify the comparisons that Scott
is
producing w/ your own comparisons that show more of a discpreapancy ?

Thanks,

mike

Fri, 16 Mar 2001, Tim Osborn wrote:

> Dear Mike & Scott,
>
> Attached is "traditional.ps", comparing the 1404-1855 (i.e.
> pre-instrumental) REG-EM reconstruction with our existing Osborn et al.
> maps and Briffa et al. quasi-hemispheric series (see refs below). Neither
> the REG-EM nor the existing reconstructions use the age-banded trees, so
> low frequencies are suppressed. [Scott - thanks for the new age-banded
> results, but I probably won't get to them till next week due to other
> commitments.]
>
> The time series comparisons are, as you see, quite good - thought you'd
> expect this as we're comparing two methods but identical data! Red is
> REG-EM, black is from the Osborn et al. existing reconstructions (then
> averaged into quasi-hemispheric means), while blue is from Briffa et al.
> (where we average the tree-density into regions/hemisphere *before*
> calibrating against regional/hemispheric temperature). Blue & black agree
> quite closely, so all correlations and % var explained are between red and
> black.
>
> Timeseries are:
>
> '0-90' = full spatial average over each of our existing maps.
> '0-70' = full spatial average over each of the REG-EM maps.
> 'masked' indicates REG-EM maps are masked by the time-dependent coverage of
> our existing maps.
> 'land20-90' or 'land20-70' indicates only land grid boxes north of 20N are
> averaged.
> 'treeboxes' indicates only those grid boxes that contain tree-ring sites
> are averaged together.
>
> The pattern correlations range from 0.2 to 0.8, with a mean of 0.6
> (approx). Fairly consistent then. The pattern of temporal correlations is
> reasonable, ranging from 0.0 to 0.9, with a mean of 0.6 (approx).
>
> Comments:
> (1) Time series generally have less variance in REG-EM, especially early
> on, though masking of data brings them closer to our time series.
> (2) Getting the mean level correct (I've converted REG-EM to behave like
> anomalies from 1961-90 mean) helps with the %variance explained considerably.
> (3) The temporal correlations are poorer for boxes containing trees than
> those that do not!
>
> The decreased variance early in the REG-EM [comment (1)] is, I guess,
> because the fewer the records with data, the earlier the
> truncation/weighting function kicks in etc. and therefore the less the
> variance that is reconstructed. As the 'skill' of REG-EM decreases, the
> more the values are filled in with something near to their mean, I seem to
> recall. This raises the question that the early values might be biased
> towards the observational mean? If so, it might be better to replace box
> values by missing values when their expected 'skill' becomes fairly low.
>
> Comment (3) can be explained two ways. In the non-tree boxes our two
> methods (REG-EM and principal component regression) have similarities, and
> given the common input data, one would expect similar reconstructions -
> which the high correlations indicate. In the tree boxes, however, the
> difference is our approach uses only local information, while REG-EM still
> uses non-local information too. So, either (i) our reconstructions are
> poorer *because* we're ignoring non-local information, or (ii) REG-EM
> reconstructions are poorer *because* real local variations are partly
> masked by regional-scale variations. It might be possible to choose either

> (i) or (ii) as a preferred explanation, using verification or other
> consideration, but I'd prefer to stick with (i) and (ii) as being equally
> possible and therefore justifying both approaches. This is politically
> better too! What I get out of the comparison is that the REG-EM is
> producing variability that is highly correlated with our method, given the
> same input data. The main concern is the difference in variance and hence
> absolute anomalies. We should look at this again when I've compared the
> age-banded stuff too.
>
> Another long e-mail, but I hope that this is useful (especially for EGS)
> and will form the basis of a comparion of methods paper.
>
> Have a good weekend!
>
> Tim
>
>
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>
> Briffa KR, Osborn TJ, Schweingruber FH, Harris IC, Jones PD, Shiyatov SG
> and Vaganov EA (2001) Low-frequency temperature variations from a northern
> tree-ring-density network. Journal of Geophysical Research 106, 2929-2941.
>
> Osborn TJ, Briffa KR, Schweingruber FH and Jones PD (2001)
> Annually-resolved patterns of summer temperatures over the Northern
> Hemisphere since AD 1400 from a tree-ring-density network. In preparation.
>

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Subject: Vulnerability in ATEAM
Date: Fri, 16 Mar 2001 22:17:24 +0100
Reply-to: Wolfgang Cramer <Wolfgang.Cramer@pik-potsdam.de>

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Dear everybody,

I am still busy compiling the report from the kickoff meeting (and I also still await some input pieces from some of you...).

For those of you who could not be there, let me just say that I enjoyed very much to see the group here, and to witness the really lively and productive discussions. Let's keep it that way.

While U wait for the report - I would like to get you thinking about the project again by circulating the second draft of a small piece which is edging towards a working definition of vulnerability, mostly written by Richard and with input from Pete, Miguel and myself. All comments are welcome. This is not intended for publication of course, but it could be a start of something more substantial in due course.

So please send me the elements still missing for the overall report, and comment to the four authors about the vulnerability piece.

Best regards,

Wolfgang

--
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PS: Sticking to my promise to avoid attachments, I send the plain ascii text here. Some time Monday you should find the pdf of it on the web site.

Internal ATEAM document “Towards a definition of vulnerability…” - do not cite
Draft version 2.0 (16/3/01)

TOWARDS A DEFINITION OF VULNERABILITY

OF ECOSYSTEM FUNCTIONING TO GLOBAL CHANGE

Richard J.T. Klein, Pete Smith, Miguel B. Araújo and Wolfgang Cramer

This document aims to stimulate the discussion of vulnerability to global change, which is a key feature of the EU project Advanced Terrestrial Ecosystem Analysis and Modelling (ATEAM). The goal of ATEAM is to develop an operational quantitative assessment of vulnerability across European ecosystems. The rationale for this assessment and its initial elements are also found in this document.

Common features in present definitions of vulnerability

Vulnerability is a multi-dimensional concept that has been a topic of study in many different scientific disciplines, ranging from anthropology and psychology to economics and ecology. As such, it has been defined and assessed in many different ways for many different purposes. The scientific literature provides many examples of vulnerability assessments, each with their own explicit or (more often) implicit interpretations of what vulnerability means to the object of study.

In spite of this diversity the various interpretations of vulnerability have a number of things in common:

1. Vulnerability is always an attribute of a system, in the broadest meaning of the term. Systems that may be vulnerable include individual people, communities, countries, economic sectors, landscapes, resources, ecosystems and so on. Importantly, in ATEAM the system of interest is not ecosystems per se but the set of functions that ecosystems perform in providing goods and services to human society.

2. Vulnerability always refers to some potential of or exposure to harm or damage. It is therefore meaningful to specify exactly to which forcing a system is thought to be vulnerable. In ATEAM multiple forcings are considered, all related in some way to global change. In response to needs expressed by the European Commission these forcings are the increasing atmospheric concentration of CO_2, the climate change that is the result of this increasing concentration, as well as the effects of changing land use and land-use policies.

3. Definitions of vulnerability tend to capture some notion of the extent to which the system would be unable to avoid, defend itself against, cope with, adjust to or otherwise prevent or minimise potential harm or damage. This mechanism of damage prevention or minimisation (termed adaptation in the context of climate change) is important because it defines the difference between the potential harm or damage and the actual or residual impacts that will occur. It can be argued that if a stress-exposed system has the ability to avert the potentially severe impacts that could ensue from this stress, then it is not vulnerable (footnote 1).

The first assessments of vulnerability to climate change (such as the First and Second Assessment Reports of the IPCC and many national vulnerability studies) were carried out without considering adaptation as an important aspect of vulnerability. These assessments implicitly assumed present-day behaviour and activities to continue unchanged in the future, irrespective of how they would be affected by climate change. By ignoring adaptation these studies did not distinguish between potential and residual impacts and thus their results represented serious overestimates of the system's vulnerability. On the other hand, the studies served to generate awareness of the potential magnitude of impacts and of the need for adaptation.

A recent discussion of vulnerability: the IPCC Working Group II

Each of the aforementioned features of vulnerability was incorporated in the proposed definition of vulnerability in the IPCC Working Group II Third Assessment Report, which was as follows:

> The degree to which a system is sensitive to and unable to cope with adverse impacts of climatic stimuli. Vulnerability is a function of a system's exposure and its adaptive capacity.

However, the IPCC Working Group II Plenary meeting in Geneva (13–16 February 2001) adopted a somewhat modified and expanded definition in the final, government-approved version of the Summary for Policymakers. The adopted definition no longer captures the important notion that vulnerability depends on both potential impacts and the inability to cope with these impacts, as was indicated by the word "and" in the first sentence of the above definition:

> The degree to which a system is susceptible to, or unable to cope with, adverse effects of climatic change, including climate variability and extremes. Vulnerability is a function of the character, magnitude and rate of climate variation to which a system is exposed, its sensitivity, and its adaptive capacity.

Building blocks for a definition to be used in ATEAM

The former definition of vulnerability captures the various aspects of vulnerability discussed above but it is likely to be too broad to be made operational in ATEAM. ATEAM addresses the interaction between ecosystems and society and in particular the provision of goods and services by ecosystems for human use. Of relevance to ATEAM are therefore not only the exposure and adaptive capacity of ecosystems to climate change but also the adaptive capacity of human systems in relation to a change in the provision of ecosystem goods and services. To develop a meaningful definition of vulnerability for ATEAM it could be useful to explore a number of related concepts: risk, sustainability and resilience.

A relatively widely accepted interpretation of risk is that it is a function of the probability of occurrence of an event combined with an estimate of the magnitude of its impact. For example, in the context of species conservation risk can be seen as a measure of the probability that a negative event (i.e., a threat) combined with the individual species' response to these events (i.e., an indicator of species' vulnerability) would lead a species to extinction (Araújo and Williams, 2000).

Amongst the many definitions of sustainability, a useful one is based on the conservation and substitutability of different types of capital: human-made capital, natural capital, human capital and social capital (Serageldin and Steer, 1994). Sustainable development, of which the most widely used definition is “development that meets the needs of the present without compromising the ability of future generations to meet their own needs” (WCED, 1987), prescribes that the total stock of capital does not decrease over time. Whether or not substitution and compensation of different types of capital are allowed depends on the preferred level of sustainability (cf. weak versus strong sustainability).

The relationship between sustainability and ecosystem vulnerability is based on the extent to which external forcings lead to a decrease in natural capital and thus in the potential of ecosystems to provide goods and services for human use. A possible (anthropocentric) definition of sustainability in the context of ATEAM could therefore be:

> The ability of an ecosystem to provide humans with goods and services in the present, without compromising the ability of future human generations to obtain these ecosystem goods and services in the future.

The concept of resilience is well known in ecology, although two distinct interpretations of the term exist. As defined by Holling (1973), resilience determines the persistence of relationships within a system and is a measure of the ability of these systems to absorb changes and still persist. According to Pimm (1984), however, resilience describes the speed with which a system returns to its original state following a perturbation. Holling (1973), on the other hand, considered this to be the stability of a system, whilst Pimm (1984) referred to stability as the combination of resilience, resistance, persistence and variability.

In an attempt to define the resilience of the Dutch coast, Klein et al. (1998) distinguished between a morphological, an ecological and a socio-economic component of coastal resilience, each of which represents another aspect of the coastal system’s capacity to cope with perturbations. They described coastal resilience as a measure of the extent to which a coast is able to respond to external pressures without losing actual or potential functions:

> The resilience of the coast is its self-organising capacity to preserve actual and potential functions of coastal systems under the influence of changing hydraulic and morphological conditions. This capacity is based on the (potential) dynamics of morphological, ecological and socio-economic processes in relation to the demands that are made by the functions to be preserved.

Given the focus of ATEAM on ecosystem services, we might want to work towards a similar type of definition of vulnerability, whereby vulnerability could be described in terms of the likelihood that an ecosystem loses a significant amount of its capacity to provide goods and services that are important to society. A definition that includes the temporal dimension of global change and sustainability could describe vulnerability in terms of the risk of ecosystem sustainability being compromised. Before suggesting a “final” definition, however, we would like to invite views and suggestions from the entire ATEAM consortium.

References
Araújo, M.B. and Williams, P.H., 2000: Selecting areas for species persistence using occurrence data. Biological Conservation, 96(3), 331–345.
Holling, C.S., 1973: Resilience and stability of ecological systems. Annual Review of Ecology and Systematics, 4, 1–24.
Klein, R.J.T., M.J. Smit, H. Goosen and C.H. Hulsbergen, 1998: Resilience and vulnerability: coastal dynamics or Dutch dikes? The Geographical Journal, 164(3), 259–268.
Pimm, S.L., 1984: The complexity and stability of ecosystems. Nature, 307, 321–326.
Serageldin, I. and A. Steer (eds.), 1994: Making Development Sustainable: From Concepts to Action. Environmentally Sustainable Development Occasional Paper Series No. 2, World Bank, Washington DC, iii+40 pp.
(WCED) World Commission on Environment and Development, 1987: Our Common Future, Oxford University Press, Oxford, UK, xv+383 pp.

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1 In this document we do not elaborate on the possible different interpretations of adaptation. Adaptation will be the subject of more detailed discussion at a later stage, aimed at an appropriate (semi-) quantitative operationalisation.
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From: "Michael E. Mann" <mann@virginia.edu>
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Subject: problem
Date: Wed, 04 Apr 2001 14:10:07 -0400
Cc: mann@virginia.edu, rbradley@geo.umass.edu

<x-flowed>
Phil et al,

There is a problem w/ figure 4 (and discussion thereof) in your paper to appear in Science. Unfortunately, I didn't catch this until I re-read the paper just now. You haven't shown the right Mann et al NINO3 reconstruction. Are you sure you have used the *cold-season* NINO3 reconstruction, as discussed (and available) in the Mann et al Earth Interactions paper, and not the annual mean reconstruction!!

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

I don't believe that has the trend that the series you show does. That NINO3 series agrees closely (r=0.63) w/ the Stahle et al series (once the sign has been flipped on that series, and the off-by-one-year date convention is taken into account), far closer than what you have shown. I'm pretty sure you've used the wrong series.

Moreover, it is inappropriate to refer (as you do) the Nino3 reconstruction as an SOI reconstruction, no matter whether it has been renormalized, sign-switched, etc. There are fundamennal differences between the low-frequency behavior of NINO3 and SOI, (consider

for example the 20th century!) and they aren't dynamically equivalent! To
say there is a "long-term trend" in our "SOI reconstruction"
is extremely misleading. There is a long-term trend in our *NINO3*
reconstruciton. Only Stahle produced an SOI reconstruction, and it is only
meaningful to correlate the two at annual timescales where they should
similarly reflect largely interannual ENSO variability.

Moreover, I don't think this is true (or as true) of our colld-season NINO3
series, which is the right one to use. Hopefully, you still
have a chance to change this in the galleys, etc.

Thanks in advance for your attention to this,

mike

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225. 0986486371.txt
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From: Mike Hulme <m.hulme@uea.ac.uk>
To: s.torok
Subject: Fwd: RE: kyoto survey - press inquiry from the THES
Date: Thu Apr 5 11:59:31 2001

Simon,
Could you - or Vanessa - buy a THES today from the paper shop and check this out. I would
quite like to draft a short letter to THES as suggested by Steve. But I need to see how
the issue was presented in this week's issue.
Thanks,
Mike

From: "Farrar, Steve" <steve.farrar@thes.co.uk>
To: 'Mike Hulme' <m.hulme@uea.ac.uk>
Subject: RE: kyoto survey - press inquiry from the THES
Date: Thu, 5 Apr 2001 09:45:33 +0100
X-Mailer: Internet Mail Service (5.5.2653.19)
Dear Mike,
thanks for that. I feel terrible but despite the pain it cost to reply to the survey,
the deadline has now passed. We had such a high response rate that we decided to run the
piece in this week's paper while the issue of the US withdrawl from the protocol was
still high in everyone's mind. So I cannot include your responses. However, you make a
number of very significant points, not least your reply to question 2 on the strength of
the evidence and the political framework outlined in your final sentences. I wonder -

and I know this is pushing it - whether you might consider rearranging some of
these
sentences to form a brief letter to the editor for the following week's paper?
I would
like this issue to stay alive in the THES and allow the paper to play a small
role in
persuading as many scientists as possible to take part in a
scientific/political debate
that may contribute to influencing those people who *can* change things. Not an
original
objective, I know, but the THES does have a fairly unique position within the
academic
community and hence a responsibility. Anyhow, sorry for the bad news
best wishes
Steve
****
Steve Farrar
Science Reporter
Times Higher Education Supplement
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London E1W 1BX
United Kingdom
[1]www.thes.co.uk
Tel: (44) 020 7782 3299
Fax: (44) 020 7782 3300
-----Original Message-----
From: Mike Hulme [[2]mailto:m.hulme@uea.ac.uk]
Sent: 04 April 2001 19:57
To: Farrar, Steve
Subject: Re: kyoto survey - press inquiry from the THES
Steve,
I hate these sort of questionnaires since Y or N answers are barely
adequate. However, I've given it a go with some other comments ....
(by the way, Prof. Trevor Davies is Head of my School here at UEA - I am
only Director of a Centre within the School, albeit a highly relevant one!).
You can quote me if appropriate, but let me know before hand.
Mike
At 12:30 02/04/01 +0100, you wrote:
>Dear Mike,
>
>hope you're well. I am conducting a survey of heads of UK university
>departments of environmental science for the Times Higher Education
>Supplement. I am keen to explore views concerning the United States and
>the Kyoto agreement. I wonder if you could answer the following Yes/No
>questions when you get a moment. Note, I will not identify you unless you
>specifically state that you do not mind being quoted.
>
>I do hope you can help
>
>all the bets
>
>Steve
>
>1: Do you believe human activities are at least in part responsible for
>driving global climate change?
YES
>2: Do you feel the evidence for this is sufficiently strong to start
>reducing emissions?
NO - to reduce emissions requires more evidence than that humans are
altering climate. We need to know something about the potential risks
associated with future climate change, whether these risks can be minimised
through adaptive action and then have some socially negotiated basis for
deciding about the necessity and extent of desirable emissions

reductions. On none of these issues do we have a good basis to work from. The precautionary principle, if chosen, would imply start reducing emissions now - but I am not convinced a blind application of the precautionary principle in this case is the most appropriate instrument.
>3: Do you think the measures proposed at Kyoto were too weak, correct, or
>too strong?
The 5.2% emissions reduction by 2010 by Annex I countries were not driven by science but by real-politik. By definition they were the best achievable. The real issue however is not about target setting - it's about the dynamics of change worldwide in energy technologies, investment strategies, consumer and community behaviour and aspirations, etc. It is *these* things that in the end will deliver a safer climate - not the Protocol per se. More attention should be directed at the diverse and myriad set of actions needed to decarbonise our societies.
>4: Are you disappointed that George Bush has abandoned the Kyoto agreement?
YES - but it is too early to say that Kyoto is dead. The USA does not have the power of veto - and Bush will have to propose some climate management strategy of his own. We wait and see.
>5: Should the rest of the world press on with the agreement without the
>United States?
Probably YES. This can be achieved and should provide valuable lessons in global climate management which we can learn from in the long-term.
>6: Do you feel the US should be allowed to count carbon sequestration
>measures such as planting new forests towards any carbon emissions
>reduction target?
YES. The UK are doing it in their national climate change programme so why not the USA?
>7: Are you optimistic that there will be a new emissions control agreement
>within the next 12 months?
A 'new' one? We haven't got one yet. I would think maybe not in the next 12 months, but the critical issues about global climate management will be clearer.
>8: Should the Kyoto preliminary targets be watered down to gain the
>Americans' support?
NO. If the USA don't like them, let them not ratify or propose a strategy of their own.
>If you would like to add any comments to this survey as to the
>implications of the US's rejection of Kyoto for the planet, what UK can do
>about it or what role scientists can play in this debacle, please do so.
In a literal sense the implications for global climate are trivial - what will affect the course of global climate (and only then climate beyond about 2030 - up until then climate is pretty much pre-determined by inertia in the system) in the long-run are the effects of cumulative decisions taken by many, many people/governments/businesses over the next 10-20 years. Let's not kid ourselves that the USA President is more powerful than he would like to think. The planetary system is much bigger than one 4-year term of a US president.
The UK is playing a key role both within the negotiating machinery of the FCCC, in pioneering new scientific analyses, and in working out new forms of adapting to climate change. This momentum in the UK is not going to be halted by Bush.
Scientists need to be there to point out the long-term nature of the problem - it is not a classic political issue where a one-term government can solve or worsen the problem. Scientists need to point out that for long-term planetary management we need new analytical tools, new criteria for investment decisions, a new appreciation of the concept of global citizenship. What climate change forces us to do is to think about the influence we are having on the quality of life for the next generation but one - not our own generation or even our children's
generation. Conventional politics is not a system geared up for this challenge.
>***********************
>Steve Farrar

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>
>
>---

****
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****

****

References

1. http://www.thes.co.uk/
2. mailto:m.hulme@uea.ac.uk
3. http://www.thes.co.uk/
4. http://www.tyndall.uea.ac.uk/

226. 0986499438.txt
##

From: "Michael Mann" <memann00@hotmail.com>
To: T.Osborn@uea.ac.uk
Subject: RE: problem
Date: Thu, 05 Apr 2001 15:37:18 -0000
Reply-to: mann@virginia.edu
Cc: k.briffa@uea.ac.uk, mann@virginia.edu, p.jones@uea.ac.uk

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HI Tim,

THanks for looking into this so quickly. I agree w/ your assessment. It is probably just the fact that the signal of interest in SOI and NINO3 is really the interannual signal, and this is not evident in the low-frequency component shown, which emphasizes discrepancies that are actually small compared to amplitude of the interannual signal present in both Stahle et al and Mann et al. So I would urge showing the annual reconstructions in this case, rather than smoothed for this reason...

In IPCC we only chose to show 1700 to present, which is a better calibrated/verified interval than back to 1650, so I'd encourage you guys to restrict it to 1700-present if you can. Other than that,
I think it is important to acknowledge that SOI and NINO3 have different low-frequency trends over the 20th century, and might well have different trends in the past. It is true that many of the proxies used are sensitive to the SOI (e.g. mexican tree rings), but others are sensitive to Pacific

SST (e.g. corals from GBR, New Caledonia, Galapagos) and our claim is that
the calibration process will select out the best estimate of the temperature
patterns, rather than SLP patterns, associated w/ ENSO, from the multiproxy
network. In the future, we'll be going after SLP reconstruction too, and
it'll be interesting to see what the difference is.

I hope that clarifies. Please let me know if I can be of any further help,
provide further clarification, etc. Thanks again,

mike

>From: Tim Osborn <T.Osborn@uea.ac.uk>
>To: "k.briffa" <k.briffa@uea.ac.uk>, Michael Mann <memann00@hotmail.com>,
>   "p.jones" <p.jones@uea.ac.uk>, "T.Osborn" <T.Osborn@uea.ac.uk>,
> mann <mann@virginia.edu>
>CC: rbradley <rbradley@geo.umass.edu>
>Subject: RE: problem
>Date: Wed, 4 Apr 2001 23:02:35 +0100
>
> >Thanks for getting back to me so quickly. I could be wrong, but i just
> >want to make sure. The cold-season NINO3 is far more consistent w/ DJF
>SOI
> >and Stahle's recon, so I just want to be sure that is the one that
> >is shown.
>
> >> >Are you sure you have used the *cold-season* NINO3
> >> >reconstruction, as discussed (and available) in the Mann et al Earth
> >> >Interactions paper, and not the annual mean reconstruction!!
> >> >
> >> >http://www.ngdc.noaa.gov/paleo/ei/ei_reconsb.html
> >> >
> >> >I don't believe that has the trend that the series you show does. That
> >> >NINO3 series agrees closely (r=0.63) w/ the Stahle et al series (once
> >> >the sign has been flipped on that series, and the off-by-one-year date
> >> >convention is taken into account
>
>Dear all,
>
>I've found a machine with telnet and have been able to check my files &
>programs. The file I'm using matches the ninocold-recon.dat file
>downloadable
>from the ei_reconsb.html. It also correlates at r=0.63 with Stahle. I
>don't
>have access to plotting here, so I cannot investigate further the reason
>for
>the apparent mismatch, though I wonder whether it is due to the heavy
>(30-yr)
>smoothing used in the Science paper - much more smoothing than is typically
>used when looking at ENSO! These 30-yr differences are in fact quite small
>in
>comparison with some of the interannual variations, and perhaps the series
>would look very much more alike if unfiltered? Anyway, as far as I can
>tell,
>the figure is ok.
>
> >> >Moreover, it is inappropriate to refer (as you do) the Nino3
> >>reconstruction
> >> >as an SOI reconstruction, no matter whether it has been
> >> >renormalized, sign-switched, etc. There are fundamennal differences
> >>between
> >> >the low-frequency behavior of NINO3 and SOI, (consider
> >> >for example the 20th century!) and they aren't dynamically equivalent!

>To
> >> >say there is a "long-term trend" in our "SOI reconstruction"
> >> >is extremely misleading. There is a long-term trend in our *NINO3*
> >> >reconstruciton. Only Stahle produced an SOI reconstruction, and it is
> >>only
> >> >meaningful to correlate the two at annual timescales where they should
> >> >similarly reflect largely interannual ENSO variability.
>
>Phil/Keith - I've got a copy of our paper with me and I agree with what
>Mike
>says above, but on the other hand the lack of space constrains us. I
>wonder
>whether we can squeeze anything in at the proofs stage (have you had them
>yet
>Phil?). With a quick read I couldn't actually spot the phrase "long-term
>trend", but we could still add something about SOI and SST being coupled on
>interannual time scales and possibly doing somewhat different things on
>longer
>timescales. Mike - would you not agree, however, that your predictors
>(excluding corals) are mainly remote from the Nino 3 SST region and that
>they
>are likely responding via atmospheric teleconnection patterns and therefore
>perhaps should pick up the SOI even if calibrated against Nino 3 SST? Feel
>free to disagree - just wanted to get your reaction!
>
>Best regards
>
>Tim
>
>

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##########

From: tom crowley <tom@ocean.tamu.edu>
To: Chick Keller <ckeller@igpp.ucsd.edu>
Subject: Re: Low Frequency signals in Proxy temperatures:
Date: Sat, 28 Apr 2001 09:54:18 -0500
Cc: tom@ocean.tamu.edu, p.jones@uea.ac.uk, rbradley@geo.umass.edu, k.briffa@uea.ac.uk, mann@virginia.edu, mhughes@ltrr.arizona.edu

Chick,

look at the instrumental record! there are huge differences between different regions - Alaska has warmed substantially while eastern North America cooled after the 1950s. locking onto local records, no matter how beautiful, can lead to serious errors. If the ice cores are so infallible why do they give substantially different stories for grip and gisp2 over the last 1500 years?

the bottom line is that one cannot make a robust case that decadally hemispheric temperatures over the last 1500 years were even as warm as the late 20th century, much less warmer.

Tom

>
>Well said indeed! This helps me to slowly understand what's being
>done and why.
>
>My nagging problem remains however, and that's that there seem to be
>too many paleo records published that show much larger amplitude
>variations. Now many can be explained, but some look more robust.
>For example I think most people are wondering about the total
>disagreement between isotope temperatures from GISP II and borehole
>temperatures from GRIP and Dye 3. Here the usual land use caution
>doesn't apply since I don't think the ice above the boreholes has
>changed much?
>
>And if I understand Tom Crowley's note to me, his reconstruction
>averaged normalized records, thus missing large amplitude variations
>such as the Keigwin Sargasso one, which he used, but which shows a
>large amplitude signal tantalizingly similar to the GRIP/Dye 3
>records. (Tom used GISP II which essentially has no low frequency
>amplitude)
>
>So I read all the papers, and am impressed by the painstakingly
>careful work, but still wonder about a world in which the
>hemispherical low frequency temperature amplitude could be (see Jones
>et al Science this week) only about 0.4°C between 1000 and about
>1950, while parts of the world could have seen amplitudes of up to
>2°C in the same period. I suppose you could say that, given natural
>forcing only, there can be much larger variance from the mean
>(spatially and temporally) than in the past hundred and fifty years
>when GHG forcing is forcing more uniformity, but does this make sense?
>
>This is why I keep asking questions about the ability of various
>proxies to return low frequency information.
>
>Anything you could say about this would be greatly appreciated.
>
>Finally consider this. I read recently (don't know the pedigree of
>this number but it WAS published!!) that Milankovitch cooling at this
>point in the Holocene should be about 0.4°C/millenium (other plots
>I've seen would suggest about 2.3 to 1/2 of that). If that's true,
>then all the cooling since the year 1000 is Milankovitch and there's
>no room for variations in solar activity and multiple volcanic
>eruptions. Now I'm not saying this is the best way to think about
>such things, but it does remind us that much of the cooling seems to
>have been due to Milankovitch, and, given the small amplitude of the
>proxy records, that is a bit worrisome. What do people think about
>this?
>
>Regards,
>
>
>Well said Malcolm...
>
>mike
>
>p.s. Chick: You might want to check out the review article by Jones
>et al in the latest Science...
>
>At 01:16 PM 4/26/01 -0700, Malcolm K. Hughes wrote:
>>Dear Chick - some thoughts on a couple of the points you raised,
>>Cheers, Malcolm
>>1.   There is no reference to the ABD in MBH 98 and 99 because
>>the technique

>>was not available at that time - see the dates on Keith's publications that
>>describe it.
>>2.   There are significant regions where the ABD method is not needed,
>>because the trees live much longer than those in the Schweingruber
>>network that
>>Keith has been using, and grow under conditions that make only very
>>conservative
>>standardization necessary. There is a growing body of evidence that these
>>tree-ring records can capture century-to-millennial change accurately (Hughes
>>and Graumlich, 1996 and Hughes and Funkhouser 1998, for example). In
>>fact, the
>>MBH reconstruction before AD 1400 was largely based on these.
>>3.   Keith has pooled information from extremely large regions
>>(presumably to
>>get large enough samples), whereas we (MBH) have been particularly
>>interested in
>>spatial variability, ruling out the use of ABD.
>>4.   The ABD method is new, needs testing, and, I predict, will be
>>modified
>>as it is tested.
>>5.   The benefit of annual resolution is that direct calibration and
>>cross-validation against instrumental records is possible with a
>>high degree of
>>rigor. We are relaxing this condition somewhat in our ongoing analyses,
>>and it
>>will be interesting to see how the uncertainties increase as one includes
>>records with poorer temporal resolution. This is an issue that the
>>advocates of
>>such records do not address, so far as I can see.
>>
>>
>>
>>Professor Malcolm K. Hughes
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>>University of Arizona
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>>phone 520-621-6470
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>
>____
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>
>Charles. "Chick" F. Keller,
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>Is the noticeable increase in surfers off Scripps Beach a possible
>indication of global warming?

Thomas J. Crowley

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228. 0988831541.txt
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##########

From: Edward Cook <drdendro@ldeo.columbia.edu>
To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
Subject: Re: hockey stick
Date: Wed, 2 May 2001 15:25:41 -0400
Cc: tom crowley <tom@ocean.tamu.edu>, esper@ldeo.columbia.edu, Jonathan Overpeck <jto@u.arizona.edu>, Keith Briffa <k.briffa@uea.ac.uk>, mhughes@ltrr.Arizona.edu, rbradley@geo.umass.edu, p.jones@uea.ac.uk, srutherford@virginia.edu

Hi Mike,

No problem. I am quite happy to work this stuff through in a careful way
and am happy to discuss it all with you. I certainly don't want the work to
be viewed as an attack on previous work such as yours. Unfortunately, this
global change stuff is so politicized by both sides of the issue that it is
difficult to do the science in a dispassionate environment. I ran into the
same problem in the acid rain/forest decline debate that raged in the
1980s. At one point, I was simultaneous accused of being a raving tree
hugger and in the pocket of the coal industry. I have always said that I
don't care what answer is found as long as it is the truth or at least
bloody close to it.

Cheers,

Ed

>Hi Ed,
>
>This is fair enough, and I'm sorry if my spelling out my concerns
>sounded defensive to you. It wasn't meant to be that way.
>
>Lets figure this
>all out based on good, careful
>work and see what the data has to say in the end. We're working towards
>this ourselves, using revised methods and including borehole data, etc.
>and will keep everyone posted on this.
>
>I don't in any way doubt yours and Jan's integrity here.
>
>I'm just a bit concerned that the result is getting used publically, by
>some, before it has gone through the gauntlet of peer review.
>Especially because it is, whether you condone it or not, being used as
>we speak to discredit the work of us, and Phil et al, this is dangerous.
>I think there are some legitimate issues that need to be sorted out
>with regard to the standardization method, and would like to see
>this play out before we jump to conclusions regarding revised estimates
>of the northern hemisphere mean temperature record and the nature of
>the "MWP".
>
>I'd

>be interested to be kept posted on what the status of the manuscript is.
>
>Thanks,
>
>mike
>
>On Wed, 2 May 2001, Edward Cook wrote:
>
>> Hi Mike,
>>
>> >A few quick points Ed,
>> >
>> >These "Wally seminars" are self-promoting acts on Broecker's part, and I
>> >think the community has to reject them as having any broader significance.
>> >If Broecker had pulled this w/ Ray, Malcolm, Keith, Phil, and Tom around,
>> >he wouldn't get away w/ such a one-sided treatment of the issue. I've been
>> >extremely troubled by what I have heard here.
>>
>> It appears that you are responding in a way that is a bit overly defensive,
>> which I regret. I am not supporting Broecker per se and only explained in a
>> very detailed fashion the origin of the work by Esper and me and how it was
>> presented to refute a very unfair characterization of tree-ring data in
>> Wally's perspective piece. The fact that Esper compared his series with
>> Jones, Briffa, and Mann et al. should not be viewed as an attack on your
>> work. It was never intended to be so, but it is was a clearly legitimate
>> thing to do. As I said, I have no control over Broecker. But it is unfair
>> and indeed incorrect to start out by dismissing the "Special Wally
>> Seminars" as self-promoting acts. To say that is simply wrong. He doesn't
>> bring people in to only express support for his point of view or pet
>> theory, as you are implying. So, I suggest that you cool down a bit on this
>> matter. It detracts from the scientific issues that should properly be
>> debated here. This is the only point on which I will defend Broecker.
>>
>> >I'm also a bit troubled by your comparisons w/ glacial advances, etc. and
>> >how these correlate w/ your reconstruction. Malcolm, Ray, Phil, and others
>> >have been over this stuff time and again, and have pointed out that these
>> >data themselves don't support the notion of globally-synchronoous changes.
>> >You seem to be arguing otherwise? And with regard to association w/
>> >volcanic forcing, Tom has already shown that the major volcanic events are
>> >captured correctly in the existing reconstructions, whether or not the
>> >longer-term trends are correct or not...
>>
>> I am not arguing for "globally-synchronous changes" and never have. To
>> quote what I said about neo-glacial advances, some of the fluctuations in
>> Esper's series "correspond well with known histories of neo-glacial advance
>> in some parts of the NH". Note the use of the word "some" in that quote.
>> That is a fair statement and why shouldn't I say it if it is true,
>> coincidently or not. Whether or not it argues for "globally-synchronous
>> changes" is up to you. I would never argue that everything happening on
>> multi-decadal time scales is phase-locked across the NH. That would be a
>> silly thing to say. But it is perfectly valid to point out the degree to
>> which independent evidence for cold periods based on glacier advances
>> appears to agree with a larger-scale indicator of temperature variablity. I
>> thought this is how science to supposed to proceed. I also don't see your
>> point about volcanic forcing. I mentioned this purely in the spirit of the
>> work of Crowley and others to suggest that the Esper series is probably
>> capturing this kind of signal as well. It has nothing to do with the issue
>> of centennial trends in temperature. You are reading far more into what I
>> wrote than I ever intended or meant.
>>
>> >Re the boreholes. Actually, if Tom's estimates are correct, and it is also
>> >correct that the boreholes have the low-frequency signal correct over the
>> >past few centuries, we are forced to also accept Tom's result that the

>> >so-called "MWP", at the hemispheric scale, is actually even COOLER relative
>> >to present than our result shows! That was clear in Tom's presentation at
>> >the workshop. So lets be clear about that--Tom's work and the boreholes in
>> >no way support Broecker's conclusion that the MWP was warmer than we have
>> >it--it actually implies the MWP is colder than we have it!
>> >Tom, please speak up if I'm not correct in this regard!
>>
>> I am not saying that Tom's results are wrong. And, I am certainly not
>> saying that Broecker is right. I merely described the results of a new
>> analysis of a somewhat new set of long tree-ring records from the
>> extra-tropics. My statement that the MWP appeared to be comparable to the
>> 20th century does not imply, nor was it meant to imply, that somehow the
>> 20th century temperature is not truly anomalous and being driven by
>> greenhouse gases. To quote from my email, "I would not claim (and nor would
>> Jan) that it exceeded the warmth of the late 20th century. We simply do not
>> have the precision or the proxy replication to say that yet." Note the use
>> of the word "precision". This clearly relates to the issue of error
>> variance and confidence intervals, a point that you clearly emphasize in
>> describing your series. Also note the emphasis on "late 20th century". I
>> think that most researchers in global change research would agree that the
>> emergence of a clear greenhouse forcing signal has really only occurred
>> since after 1970. I am not debating this point, although I do think that
>> there still exists a signficant uncertainty as to the relative
>> contributions of natural and greenhouse forcing to warming during the past
>> 20-30 years at least. Note that I also tried to emphasize the
>> extra-tropical nature of this series, and it may be that the tropics do not
>> show the same strength of warming. But I do argue strongly that we do not
>> have the high-resolution proxy data needed to test for a MWP in the
>> tropics. Please correct me if I am wrong here.
>>
>> >We are in the process of incorporating the borehole data into the
>> >low-frequency component of the reconstruction. The key difference will be
>> >that they are going to be calibrated against the instrumental record and
>> >weighted by the spatial coherence within the borehole data rather than what
>> >Pollack has done. I expect the results will be different, but in any case
>> >quite telling...
>>
>> Fine.
>>
>> >I'll let Malcolm and Keith respond to the issues related to the
>> >standardization of the Esper chronologies, though it immediately sounds to
>> >me quite clear that there is the likelihood of of having contaminated the
>> >century-scales w/ non-climatic info. Having now done some work w/
>> >chronologies in disturbed forests myself now (in collaboration w/ Dave
>> >Stahle), I know how easy it is to get lots of century-scale variability
>> >that has nothing to do w/ climate. I imagine the reviewers of the
>> >manuscript will have to be convinced that this is the case w/ what Esper
>> >has done. I'm very skeptical. I'm also bothered that Broecker has promoted
>> >this work prior to any formal peer review. There are some real issues w/
>> >the standardization approach and there is a real stretch in promoting this
>> >as a hemispheric temperature reconstruction.
>>
>> I appreciate your skepticism and I hope that Jan and I can convince you
>> otherwise. I also encourage you to continue getting your shoulders sore and
>> hands dirty on tree-ring sampling and analysis. Esper's analysis is not
>> perfect. Nor is anyone elses who works in this game. But if Esper's series
>> is wrong on century time scales, then Jones and Briffa are wrong too. If
>> Esper's series is also wrong on inter-decadal time scales, then your series
>> is wrong as well because on that time scale of variablity, his series
>> agrees very well with yours. So, I would be very cautious about declaring
>> that Esper's series is in some sense invalid. Finally, as I have said ad
>> nausem, I have no control over what Broecker thinks or does beyond
>> presenting to him a convincing case for the ability of certain tree-ring

>> series to preserve long-term temperature variability. And again, "I also
>> tried to emphasize the extra-tropical nature of this series." Please give
>> me a break here.
>>
>> >Finally, what is the exact spatial distribution of the sparse data he used.
>> >Scott R. drove home the point regarding the importance of taking into
>> >account spatial sampling in his talk at the workshop. A sparse
>> >extratratropical set of indicators, no matter how
>> >locally-temperature-sensitive they are, will not, unless you're *very*
>> >lucky w/ the locations, be an accurate indicator of true N. Hem temp. In
>> >general it will overestimate the variance at all timescales. The true N.Hem
>> >temperature (ie, weighted largely by tropical ocean SST) has much less
>> >variance than extratrpoical continents. There may be a large apples and
>> >oranges component to the comparisons you describe.
>>
>> I know your argument and I am sensitive to it, hence my emphasis on
>> "extra-tropical". So, don't look for disagreement on the importance of the
>> tropical SSTs to any estimate of NH temperatures. But let's be honest here.
>> Your reconstruction prior to roughly AD 1600 is dominated by extra-tropical
>> proxies. So, in a way, you are caught in the same dilemma as all other
>> people who have tried to do this.
>>
>> >We've shown that are reconstructions in continental extratropical regions
>> >have lots more variance and variability. It is, as we have all shown, the
>> >averaging over many regions that reduces the amplitude of variability. Our
>> >regional reconstructions show far more significant warm and cold periods.
>> >But they cancel out spatially!
>>
>> Understood, but it is still unclear how this all happens as your
>> reconstruction proceeds back in time with an increasingly limited and
>> spatially-restricted set of proxies. Confidence limits that you place on
>> your series is laudable and I agree, to first order, that the MWP in your
>> series could easily have been cooler than what you show. But it implicitly
>> assumes that the estimates are equally unbiased (or equally biased for that
>> matter) back in time. I don't know if that is an issue here, but I believe
>> that the issue of bias using an increasingly sparse number of predictors
>> scattered irregularly over space has not be investigated. Please correct me
>> if I am wrong here.
>>
>> >If a legitimate argument were to be made that we have significnatly
>> >understiamted, within the context of our uncertainty estimates, the
>> >amplitude of the MWP at the hemispheric scale, I'd be the first to accept
>> >it (note that, as Phil et al pointed out in their recent review article in
>> >Science, we do not dispute that temperatures eearly in the millennium,
>> >within the uncertainty estimates, may have been comparable to early/mid
>> >20th centurys--just not late 20th century temperatures).
>>
>> We are in agreement here. See my earlier comments.
>>
>> >Frankly though Ed, I really don't see it here. We may have to let the
>> >peer-review process decid this, but I think you might benefit from knowing
>> >the consensus of the very able group we have assembled in this email
>> >list, on what Esper/you have done?
>>
>> Of course, I know everyone in this "very able group" and respect their
>> opinions and scientific credentials. The same obviously goes for you. That
>> is not to say that we can't disagree. Afterall, consensus science can
>> impede progress as much as promote understanding.
>>
>> Cheers,
>>
>> Ed
>>

>> >Comments or thoughts?
>> >
>> >cheers,
>> >
>> >mike
>> >
>> >At 10:59 AM 5/2/01 -0400, Edward Cook wrote:
>> >> >Ed,
>> >> >
>> >> >heard some rumor that you are involved in a non-hockey stick
>>reconstruction
>> >> >of northern hemisphere temperatures. I am very intrigued to learn about
>> >> >this - are these results suggesting the so called Medieval Warm
>>Period may
>> >> >be warmer than the early/mid 20th century?
>> >> >
>> >> >any enlightenment on this would be most appreciated, Tom
>> >> >
>> >> >
>> >> >
>> >> >Thomas J. Crowley
>> >> >Dept. of Oceanography
>> >> >Texas A&M University
>> >> >College Station, TX 77843-3146
>> >> >979-845-0795
>> >> >979-847-8879 (fax)
>> >> >979-845-6331 (alternate fax)
>> >>
>> >>Hi Tom,
>> >>
>> >>As rumors often are, the one you heard is not entirely accurate. So, I
>>will
>> >>take some time here to explain for you, Mike, and others exactly what was
>> >>done and what the motivation was, in an effort to hopefully avoid any
>> >>misunderstanding. I especially want to avoid any suggestion that this work
>> >>was being done to specifically counter or refute the "hockey stick".
>> >>However, it does suggest (as do other results from your EBM, Peck's work,
>> >>the borehole data, and Briffa and Jones large-scale proxy estimates) that
>> >>there are unresolved (I think) inconsistencies in the low-frequency
>>aspects
>> >>of the hockey stick series compared to other results. So, any comparisons
>> >>with the hockey stick were made with that spirit in mind.
>> >>
>> >>What Jan Esper and I are working on (mostly Jan with me as second author)
>> >>is a paper that was in response to Broecker's Science Perspectives
>>piece on
>> >>the Medieval Warm Period. Specifically, we took strong exception to his
>> >>claim that tree rings are incapable of preserving century time scale
>> >>temperature variability. Of course, if Broecker had read the
>>literature, he
>> >>would have known that what he claimed was inaccurate. Be that as it may,
>> >>Jan had been working on a project, as part of his post-doc here, to
>>look at
>> >>large-scale, low-frequency patterns of tree growth and climate in long
>> >>tree-ring records provided to him by Fritz Schweingruber. With the
>>addition
>> >>of a couple of sites from foxtail pine in California, Jan amassed a
>> >>collection of 14 tree-ring sites scattered somewhat uniformly over the
>> >>30-70 degree NH latitude band, with most extending back 1000-1200 years.
>> >>All of the sites are from temperature-sensitive locations (i.e. high
>> >>elevation or high northern latitude. It is, as far as I know, the largest,
>> >>longest, and most spatially representative set of such
>> >>temperature-sensitive tree-ring data yet put together for the NH

>> >>extra-tropics.
>> >>
>> >>In order to preserve maximum low-frequency variance, Jan used the Regional
>> >>Curve Standardization (RCS) method, used previously by Briffa and myself
>> >>with great success. Only here, Jan chose to do things in a somewhat
>>radical
>> >>fashion. Since the replication at each site was generally insufficient to
>> >>produce a robust RCS chronology back to, say, AD 1000, Jan pooled all of
>> >>the original measurement series into 2 classes of growth trends:
>>non-linear
>> >>(~700 ring-width series) and linear (~500 ring-width series). He than
>> >>performed independent RCS on the each of the pooled sets and produced
>>2 RCS
>> >>chronologies with remarkably similar multi-decadal and centennial
>> >>low-frequency characteristics. These chronologies are not good at
>> >>preserving high-frquency climate information because of the scattering of
>> >>sites and the mix of different species, but the low-frequency patterns are
>> >>probably reflecting the same long-term changes in temperature. Jan than
>> >>averaged the 2 RCS chronologies together to produce a single chronology
>> >>extending back to AD 800. It has a very well defined Medieval Warm
>>Period -
>> >>Little Ice Age - 20th Century Warming pattern, punctuated by strong
>>decadal
>> >>fluctuations of inferred cold that correspond well with known histories of
>> >>neo-glacial advance in some parts of the NH. The punctuations also appear,
>> >>in some cases, to be related to known major volcanic eruptions.
>> >>
>> >>Jan originally only wanted to show this NH extra-tropical RCS
>>chronology in
>> >>a form scaled to millimeters of growth to show how forest productivity and
>> >>carbon sequestration may be modified by climate variability and change
>>over
>> >>relatively long time scales. However, I encouraged him to compare his
>> >>series with NH instrumental temperature data and the proxy estimates
>> >>produced by Jones, Briffa, and Mann in order bolster the claim that his
>> >>unorthodox method of pooling the tree-ring data was producing a record
>>that
>> >>was indeed related to temperatures in some sense. This he did by linearly
>> >>rescaling his RCS chronology from mm of growth to temperature
>>anomalies. In
>> >>so doing, Jan demonstrated that his series, on inter-decadal time scales
>> >>only, was well correlated to the annual NH instrumental record. This
>>result
>> >>agreed extremely well with those of Jones and Briffa. Of course, some of
>> >>the same data were used by them, but probably not more than 40 percent
>> >>(Briffa in particular), so the comparison is based on mostly, but not
>> >>fully, independent data. The similarity indicated that Jan's approach was
>> >>valid for producing a useful reconstruction of multi-decadal temperature
>> >>variability (probably weighted towards the warm-season months, but it is
>> >>impossible to know by how much) over a larger region of the NH
>> >>extra-tropics than that produced before by Jones and Briffa. It also
>> >>revealed somewhat more intense cooling in the Little Ice Age that is more
>> >>consistent with what the borehole temperatures indicate back to AD 1600.
>> >>This result also bolsters the argument for a reasonably large-scale
>> >>Medieval Warm Period that may not be as warm as the late 20th century, but
>> >>is of much(?) greater significance than that produced previously.
>> >>
>> >>Of course, Jan also had to compare his record with the hockey stick since
>> >>that is the most prominent and oft-cited record of NH temperatures
>>covering
>> >>the past 1000 years. The results were consistent with the differences
>>shown
>> >>by others, mainly in the century-scale of variability. Again, the Esper

>> >>series shows a very strong, even canonical, Medieval Warm Period - Little
>> >>Ice Age - 20th Century Warming pattern, which is largely missing from the
>> >>hockey stick. Yet the two series agree reasonably well on inter-decadal
>> >>timescales, even though they may not be 1:1 expressions of the same
>> >>temperature window (i.e. annual vs. warm-season weighted). However, the
>> >>tree-ring series used in the hockey stick are warm-season weighted as
>>well,
>> >>so the difference between "annual" and "warm-season weighted" is probably
>> >>not as large as it might seem, especially before the period of
>>instrumental
>> >>data (e.g. pre-1700) in the hockey stick. So, they both share a
>>significant
>> >>degree of common interdecal temperature information (and some, but not
>> >>much, data), but do not co-vary well on century timescales. Again,
>>this has
>> >>all been shown before by others using different temperature
>> >>reconstructions, but Jan's result is probably the most comprehensive
>> >>expression (I believe) of extra-tropical NH temperatures back to AD 800 on
>> >>multi-decadal and century time scales.
>> >>
>> >>Now back to the Broecker perspectives piece. I felt compelled to refute
>> >>Broecker's erroneous claim that tree rings could not preserve long-term
>> >>temperature information. So, I organized a "Special Wally Seminar" in
>>which
>> >>I introduced the topic to him and the packed audience using Samuel
>> >>Johnson's famous "I refute it thus" statement in the form of "Jan
>>Esper and
>> >>I refute Broecker thus". Jan than presented, in a very detailed and well
>> >>espressed fashion, his story and Broecker became an instant convert. In
>> >>other words, Wally now believes that long tree-ring records, when properly
>> >>selected and processed, can preserve low-frequency temperature variability
>> >>on centennial time scales. Others in the audience came away with the same
>> >>understanding, one that we dendrochronologists always knew to be the case.
>> >>This was the entire purpose of Jan's work and the presentation of it to
>> >>Wally and others. Wally had expressed some doubts about the hockey stick
>> >>previously to me and did so again in his perspectives article. So, Jan's
>> >>presentation strongly re-enforced Wally's opinion about the hockey stick,
>> >>which he has expressed to others including several who attended a
>> >>subsequent NOAA meeting at Lamont. I have no control over what Wally says
>> >>and only hope that we can work together to reconcile, in a professional,
>> >>friendly manner, the differences between the hockey stick and other proxy
>> >>temperature records covering the past 1000 years. This I would like to do.
>> >>
>> >>I do think that the Medieval Warm Period was a far more significant event
>> >>than has been recognized previously, as much because the high-resolution
>> >>data to evaluate it had not been available before. That is much less
>>so the
>> >>case now. It is even showing up strongly now in long SH tree-ring series.
>> >>However, there is still the question of how strong this event was in the
>> >>tropics. I maintain that we do not have the proxies to tell us that now.
>> >>The tropical ice core data are very difficult to interpret as temperature
>> >>proxies (far worse than tree rings for sure and maybe even unrelated to
>> >>temperatures in any simple linear sense as is often assumed), so I do not
>> >>believe that they can be used alone as records to test for the
>>existence of
>> >>a Medieval Warm Period in the tropics. That being the case, there are
>> >>really no other high-resolution records from the tropics to use, and the
>> >>teleconnections between long extra-tropical proxies and the tropics are, I
>> >>believe, far too tenuous and probably unstable to use to sort out this
>> >>issue.
>> >>
>> >>So, at this stage I would argue that the Medieval Warm Period was probably
>> >>a global extra-tropical event, at the very least, with warmth that was

>> >>persistent and probably comparable to much of what we have experienced in
>> >>the 20th century. However, I would not claim (and nor would Jan) that it
>> >>exceeded the warmth of the late 20th century. We simply do not have the
>> >>precision or the proxy replication to say that yet. This being said, I do
>> >>find the dismissal of the Medieval Warm Period as a meaningful global
>>event
>> >>to be grossly premature and probably wrong. Kind of like Mark Twain's
>> >>commment that accounts of his death were greatly exaggerated. If, as some
>> >>people believe, a degree of symmetry in climate exists between the
>> >>hemispheres, which would appear to arise from the tropics, then the
>> >>existence of a Medieval Warm Period in the extra-tropics of the NH and SH
>> >>argues for its existence in the tropics as well. Only time and an enlarged
>> >>suite of proxies that extend into the tropics will tell if this is true.
>> >>
>> >>I hope that what I have written clarifies the rumor and expresses my views
>> >>more completely and accurately.
>> >>
>> >>Cheers,
>> >>
>> >>Ed
>> >>
>> >>==================================
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>> >>Tree-Ring Laboratory
>> >>Lamont-Doherty Earth Observatory
>> >>Palisades, New York 10964 USA
>> >>Phone: 1-845-365-8618
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229. 0990119702.txt
##

From: "Michael E. Mann" <mann@virginia.edu>
To: Ed Cook <drdendro@ldeo.columbia.edu>
Subject: Re: Comments on "Extending NAO Reconstructions ..."
Date: Thu, 17 May 2001 13:15:02 -0400
Cc: Juerg Luterbacher <juerg@giub.unibe.ch>, Keith Briffa <k.briffa@uea.ac.uk>, Phil Jones <p.jones@uea.ac.uk>, "Michael E. Mann" <mann@virginia.edu>, Scott Rutherford <srutherford@virginia.edu>

<x-flowed>
Hi Ed,

On the road, but just had to chime into this debate briefly.

What you say is of course true, but we have to start somewhere. Step #1 is producing a reconstruction. Without some reasonable estimate of uncertainty, a reconstruction isn't
very useful in my opinion. Step #2 is producing some reasonable estimate of uncertainty. In my mind, this is based on looking at the calibration residuals, seeing if they pass some basic tests for whiteness, normality, etc., looking at the verification statistics, and seeing if this continues to hold up in an independent sample. It is important to use the longest instrumental records we have for independent verification where possible. Of course, there may be additional biases in the predictors that are difficult to identify even in a relatively long verification interval (e.g., ultra low-frequency problems w/ fidelity). Step #3 is trying to evaluate this as best we can (looking at the frequency domain structure of the predictors themselves, seeing if there is loss of variance at very long timescales, looking at the robustness of long-term trends to standardization issues, etc.), etc...I see this as a successive series of diagnostics and self-consistency checks that iterate towards getting a reasonable handle on the uncertainties. This is the approach
that we have taken, and I think it is the most appropriate...

I firmly believe that a reconstruction w/ out some reasonable estimate of uncertainty is almost useless! If the community wants to use paleodata for signal detection, model validation, etc. I believe that this is absolutely essential to do, whether or not we can do a perfect job.

I would be very surprised if Hans would disagree w/ my statement above!

anyways, my two cents on the matter...

mike

At 09:50 AM 5/17/01 -0400, Ed Cook wrote:
>Hi Juerg,
>
>I've done an admittedly quick read of your paper "Extending NAO
>Reconstructions Back to AD 1500" and find it to be fine overall. One slight
>correction on pg. 3 concerning the Cook et al. (1998) recon. The tree-ring
>records used also included some from England, as well as the eastern US and
>northern Fennoscandia. On pg. 10, sentence 8-9 in Conclusions, the wording
>is a little confusing. You say "Including station pressure of Gibraltar and
>Reykjavik as predictors in 1821 lead to a decrease of the confidence
>estimates". This almost sounds like you are doing worse when adding in
>Gibraltar and Reykjavik, when I know you mean the opposite. So, a change in
>wording to something like "... lead to increased confidence in the
>estimates of monthly NAO". Also in Table 1, is the Cullen R4 NAO
>reconstruction the one with instrumental data in it? If so, it has used
>some of the same data as yours. I don't recall if R4 is the one with
>instrumental data. But if it is, you ought to mention that.
>
>
>On a thematic note that doesn't have much direct bearing on the paper as it
>stands now (but which may be of interest to Keith, Phil, and Mike as well),
>I have growing doubts about the validity and use of error estimates that
>are being applied to reconstructions, such as those you have applied in
>Fig. 3. First, as you say at the end of the paper, there is a clear
>frequency dependence in the strength of relationship between the actual and
>proxy-estimated data that is not being considered, i.e. "the SE ... become
>smaller when considering low-pass filtered time series" (pg. 10). The
>assumption of the error estimates as now estimated and applied is that the
>error variance is truly white noise, i.e. equally distributed across all
>frequencies. That is surely not the case. This is different from questions
>about autocorrelated residuals, which tell you nothing about the frequency
>dependence of the quality of the estimates. This is where classic
>regression theory falls down. It is based on the notion that each
>observation is a random sample with no time history or frequency domain
>structure. When we use long time series of observations (climate or proxy)
>to reconstruct some climate variable, we are also using predictors that
>have time series structure and history that cannot vary in a completely
>random fashion even if the data could be completely resampled. This is
>because they represent a series of prior "observations" of
>climatic/environmental conditions. This lack of randomness of the
>observations used for reconstructing past climate again causes me to doubt
>the validity of the error estimates being applied. The degree to which the
>reconstruction can actually vary from year to year within the prescribed
>error limits is itself constrained by the time history of the observations
>themselves used for reconstruction. In contrast, the 2SE limits in your
>Fig. 3 prior to 1821 contain almost all of the estimates. This result could
>be used to claim that there is effectively no useful time history of
>variation in the NAO reconstruction prior to 1821 because each estimate may
>fall with equal probability anywhere in the error envelop. I would regard
>this interpretation as completely wrong. Thus, I would say that the decadal
>period of above-average winter NAO in your reconstruction around AD 1700 is
>real, assuming that the predictors used are providing unbiased estimates,
>even though it is fully enclosed by the 2SE limits that intersect zero.
>This is getting towards the debate with Von Storch over "most probable"
>estimates. I am probably not explaining myself well here and undoubtedly
>need to think more about it. But I really think that error bars, as often
>presented, may potentially distort and unfairly degrade the interpreted
>quality of reconstructions. So, are the error bars better than nothing? I'm
>not so sure.
>
>Cheers,

>
>Ed
>
> >Hello Ed
> >
> >thanks very much for your nice mail. I hope these little
> >comments were useful for you and yes of course
> >we hope too that we can merge the data base sometime
> >later on. This would be great.
> >
> >Do you think that you could send me some comments
> >on our paper by tomorrow?
> >Is your paper for the Orense book?
> >
> >Many greetings and till later
> >
> >Juerg
>
>
>==================================
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230. 0990718382.txt
###

From: "Michael E. Mann" <mann@virginia.edu>
To: christy@nsstc.uah.edu
Subject: Re: FYI: Fwd: Re: IPCC
Date: Thu, 24 May 2001 11:33:02 -0400
Cc: rbradley@geo.umass.edu, tkarl@ncdc.noaa.gov, tom crowley <tom@ocean.tamu.edu>, mhughes@ltrr.arizona.edu, jto@u.arizona.edu, rbradley@geo.umass.edu, p.jones@uea.ac.uk, k.briffa@uea.ac.uk, "Folland, Chris" <ckfolland@meto.gov.uk>, jouzel@lsce.saclay.cea.fr, trenbert@cgd.ucar.edu, steig@ess.washington.edu, mann@virginia.edu

<x-flowed>
John:

For future reference, I think its also important to clarify for you what
the Dahl-Jensen, Clow et al borehole results actually show (see Dahl-Jensen
et al, "Past Temperatures Directly from the Greenland Ice Sheet", Science,
282, October 1998).

In fact, the results show that the amplitude of variability over the past 1000+ years differs by a factor of 2 between the GRIP and Dye 3 borehole estimates (the latter only 865 km to the south). This is an example of extreme regional-scale variability, which should give pause to those who want to draw large-scale inferences.

However, even more importantly, they show in the case of Dye 3, the mid 20th century warm period in the record actually exceeds the Medieval warm peak! (see Fig 4, lower panel, blue curve). So here we have two temperature histories less than 1000 km apart in Greenland, which give different stories regarding the level of Medieval warmth, with at least one of the histories conforming precisely to the hemispheric trends presented in IPCC chapter 2 (note that in the chapter, we actually discuss the evidence of conflicting temperature trends in Greenland, though not specifically referring to Dahl-Jensen et al).

So do I understand correctly that you are referring to the results of Dahl-Jensen et al as conflicting with what we say in the chapter? At the face of it, this argument has no merit whatsoever. I think we should all use a better explanation from you, since you seem to be arguing publically that the Dahl-Jensen et al record undermines what we've said in the chapter.

Thanks in advance,

mike

p.s. I've cc'd in Eric Steig, a collaborator of Clow's and a Greenland & Antarctic Ice Core expert, to make sure my facts above have been presented accurately. Perhaps Eric woudl be kind enough to forward my email to Gary Clow, and Gary can let us know directly if he disagrees with any of my remarks above.

At 03:30 PM 5/23/01 -0400, Michael E. Mann wrote:
>John,
>
>I appreciate your reply.
>
>However, I don't agree at all w/ your assessment. It was determined early
>on that the ice core borehole results would be discussed in the context of
>the millennial-scale variability section, as they arguably don't have the
>resolution to address the timescales relevant to the past 1000 years. So
>this was in Jean's domain, not mine, and if the cross-references between
>the sections aren't clear enough in that regard, that is indeed our fault.
>
>However, there is considerable discussion of the fact that the
>Arctic/North Atlantic regions are inappropriate for inferences into
>hemispheric-scale temperature patterns, and this remains fundamentally
>from any reasonable treatment of the underlying climate dynamics that
>influence that region.
>
>The various hemispheric temperature reconstructions discussed in our
>chapter (the emphasis was on the commonality between them), including Mann
>et al, Jones et al, Briffa et al, Crowley and Lowery, and others, make
>considerable use of just about all of the available reliable low-res and
>high-res paleo data available, and come to a clear concensus regarding the
>relative warmth of the Medieval period at the hemispheric/global scale.
>Crowley's modeling results come to the same conclusion, and it entirely
>independent of
>any empirical paleoclimate reconstructions.
>
>You misrepresent the Mann et al reconstruction--it is not based on "tree
>rings", but uses all high-resolution proxy information commonly available.

>We have shown, in fact, that our reconstruction is robust to the
>inclusion/disclusion of tree ring information. The Crowley and Lowery
>reconstruction, which is discussed in our chapter, makes use of almost no
>tree ring data, and employs lower-resolution proxy indicators, including
>the very records (Keigwin, Lamb's central england temperature record,
>GISP2 o18) that are often used to argue for a warmer MWP, and yet comes to
>the same conclusion. And Tom shows that when averaged across the
>hemisphere, a warmer-than-present-day MWP just doesn't hold up.
>
>Our treatment of this subject in the chapter was far more careful, far
>more inclusive and detailed, and far more nuanced than you give us credit
>for. Your comments below remain disturbingly selective and myopic, and we
>have dealt w/ similar comments many times over...
>
>If ABC is looking to do a hatchet job on IPCC so be it (this doesn't
>surprise me--Stossel has an abysmal record in his treatment of
>environmental issues, from what I had heard), but I'll be very disturbed
>if you turn out to have played into this in a way that is unfair to your
>co-authors on chapter 2, and your colleagues in general. This wouldn't
>have surprised me coming from certain individuals, but I honestly expected
>more from you...
>
>Mike
>
>>Date: Wed, 23 May 2001 13:50:49 -0500
>>From: John Christy <christy@nsstc.uah.edu>
>>X-Mailer: Mozilla 4.04 (Macintosh; I; PPC)
>>To: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
>>Subject: Re: IPCC
>>
>>Hi Mike:
>>
>>Here's what happened. ABC News 20/20 with Stossel wanted me to be part
>>of a segment that will air at the end of June on the climate change
>>issue. Specifically the piece will be dealing with the alarmist
>>rhetoric that tends to be found in the media. I am more than happy to
>>talk about that because I've been very disappointed with what has gone
>>on even with respect to some of the IPCC elders and their pronouncements
>>for forthcoming disasters.
>>
>>In one of the pre-interviews they asked about the "Hockey Stick". I
>>told them of my doubts about the intercentury precision of the record,
>>especially the early part, and that other records suggested the period
>>1000 years ago was warmer. I remember saying that "you must give the
>>author credit for including the large error bars for that time series in
>>the figure." I also specifically said that the most precise record of
>>century scale precision, Greenland Borehole temps, was very important to
>>note but that the figure was not in the IPCC. I then looked quickly at
>>the IPCC reference list and saw the citation of Dahl-Jensen and assumed
>>that it was at least commented on in the 1000 year time series material
>>and told ABC as much.
>>
>>ABC called back a few days later and said they couldn't find a reference
>>to the Greenland stuff in the IPCC discussion of the past 1000 years.
>>So I read the final version, and ABC was right. I said this was an
>>omission that should not have happened - and that I take part of the
>>blame because I had mentioned it at each of our Lead Author meetings.
>>
>>Last Thursday night, I was one of the guys flown to NY City for the
>>taping of the show. There was only one question on this particular
>>issue (it was even after Stossel had left the room) and I gave much the
>>same answer as I indicated above (as best as I can remember)- that the
>>"Hockey Stick" (I don't think I used the term "Hockey Stick", and I'm

>>almost positive I did not mention your name at any point) is one
>>realization of temperatures but that other data are not included and
>>that I had thought the "other" data were clearly mentioned in the IPCC,
>>but weren't. I mentioned the large error bars (as a credit to you) and
>>that I was partly to blame for this omission. If they use my remark,
>>they could slice and dice it to make it as provocative as possible.
>>
>>Four of us were taped for almost 2 hours, and from this they will select
>>about 8 minutes, so I doubt my remarks will make the show. When Stossel
>>came back in after all was said and done, he said to me that I might be
>>a good scientist but I didn't have the emotion and passion necessary to
>>excite the audience. In one way, that is a compliment I suppose. I
>>think Pat M. will have a good chunk of air time (I don't remember
>>whether he added any comments on the 1000-year time series, but he may
>>have).
>>
>>Whatever is shown, just keep it in context. There is no way a clear
>>scientific point with all the caveats and uncertainties can come across
>>in such venues. However, I do agree with Stossel's premise (though I
>>don't know what the piece will actually look like so I may be
>>disappointed) that the dose of climate change disasters that have been
>>dumped on the average citizen is designed to be overly alarmist and
>>could lead us to make some bad policy decisions. (I've got a good story
>>about the writers of the TIME cover piece a couple of months ago that
>>proves they were not out to discuss the issue but to ignore science and
>>influence government.)
>>
>>It is not bad science to look at arguably the most precise measure of a
>>point temperature (actually two boreholes) when that point shows a 600+
>>year period of greater warmth than today. On that time scale, the
>>equivalent spatial scale is much larger than any of the regional
>>oscillations we now identify. But, there are several other (admittedly
>>less robust) measures that suggest greater warmth 1000 years ago that
>>are outside the N. Atlantic area. I just don't think tree rings, if
>>averaged over a century, can tell us which century was warmest. We've
>>never had two complete, independent centuries of global instrumental
>>data (separated by more than one century) to even test this idea. (By
>>the way, I came to my own conclusions long before Broekers piece
>>appeared.) This is an area of further work that I promoted to the NRC
>>about 2 months ago (more funding for Paleo work to assess intercentury
>>precision of all proxy records.)
>>
>>Regarding the IPCC. The IPCC TAR is good, but it is not perfect nor
>>sacred and is open to criticism as any document should be. In some
>>cases it is already outdated. Some of the story lines used to generate
>>high temperature changes are simply ridiculous. The IPCC is us. We are
>>under no gag rule to keep our thoughts to ourselves. I thought our
>>chapter turned out pretty good overall, and I attribute that to the
>>open, working relationship we all had (some other chapter groups did not
>>experience this) and to the tireless efforts of our convening lead
>>authors.
>>
>>Good to hear from you.
>>
>>John C.
>>
>>
>>--
>>**
>>John R. Christy
>>Director, Earth System Science Center   voice: 256-961-7763
>>Professor, Atmospheric Science   fax: 256-961-7751
>>Alabama State Climatologist

>>University of Alabama in Huntsville
>>http://www.atmos.uah.edu/atmos/christy.html
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##########

From: Kevin Trenberth <trenbert@cgd.ucar.edu>
To: "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Fwd: Recent Paper from the Competitive Enterprise Institute
Date: Thu, 24 May 2001 11:35:06 -0600 (MDT)
Reply-to: <trenbert@ucar.edu>
Cc: <rbradley@geo.umass.edu>, <tkarl@ncdc.noaa.gov>, tom crowley
<tom@ocean.tamu.edu>, <mhughes@ltrr.arizona.edu>, <jto@u.arizona.edu>,
<rbradley@geo.umass.edu>, <p.jones@uea.ac.uk>, <k.briffa@uea.ac.uk>, "Folland,
Chris" <ckfolland@meto.gov.uk>

Mike:

You are right: this is a disinformation campaign.
Some remarks

1) On the Christy et al grl paper, I sent the following to John following
the IPCC Shanghai mtg.:

Date: Mon, 22 Jan 2001 15:39:20 -0700 (MST)
From: Kevin Trenberth <trenbert@cgd.ucar.edu>
To: John Christy <christy@atmos.uah.edu>
Subject: your grl paper

John:

Just back from IPCC. One surprise was the strong Saudi delegation
distributed your recent grl paper and wanted it inserted into the SPM! In
spite of the fact that you are a lead author on Chapter 2 , the paper is
referenced, etc. In fact Simon Brown was there.

Chris Folland made a comment about his hypothesis for this: related to

changes/growth in ships. My hypothesis focusses on the buoy data.
See our recent paper submitted to jgr:

http://www.cgd.ucar.edu/cas/papers/jgr2001b/jgr2.html also

http://www.cgd.ucar.edu/cas/papers/jgr2001a/jgr_interann.html

This shows that during and following El Nino there is an anomalous flux of heat out of ocean into atmosphere in the east Pacific of order 50 W m-2 over many months: so ocean T warms relative to air. During La Lina flux goes other way. i.e. air warms relative to ocean.

So your results must be affected by 1997-98 event at end of series and that may explain trend differential.

Hope this helps
Regards
Kevin

i.e. the result is not as advertized.

====================

2) wrt Lindzen's paper

Here is the text from my recent Senate testimony

The determination of the climatic response to the changes in heating and cooling is complicated by feedbacks. Some of these can amplify the original warming (positive feedback) while others serve to reduce it (negative feedback). If, for instance, the amount of carbon dioxide in the atmosphere were suddenly doubled, but with other things remaining the same, the outgoing long-wave radiation would be reduced and instead trapped in the atmosphere. To restore the radiative balance, the atmosphere must warm up and, in the absence of other changes, the warming at the surface and throughout the troposphere would be about 1.2\dg C. In reality, many other factors will change, and various feedbacks come into play, so that the best IPCC estimate of the average global warming for doubled carbon dioxide is 2.5\dg C. In other words, the net effect of the feedbacks is positive and roughly doubles the response otherwise expected. The main positive feedback comes from increases in water vapor with warming.

In 2001, the IPCC gave special attention to this topic. The many issues with water vapor and clouds were addressed at some length in Chapter 7 (of which I was a lead author, along with Professor Richard Lindzen (M.I.T.), and others). Recent possibilities that might nullify global warming (Lindzen 2001) were considered but not accepted because they run counter to the prevailing evidence, and the IPCC (Stocker et al., 2001) concluded that ``the balance of evidence favours a positive clear sky water vapour feedback of the magnitude comparable to that found in the simulations."

===
Here is a more complete rebuttal, written March 23 to MacCracken.

Subject: Re: Recent Lindzen paper

Kevin Trenberth

1) The paper is based on very simple conceptual ideas that do not mesh with reality. Fig. 2 is simply not correct. For a more correct view of the overturning see:

Trenberth, K. E., D. P. Stepaniak and J. M. Caron, 2000: The global monsoon as seen through the divergent atmospheric circulation. {J. Climate}, 13, 3969--3993.

This paper also shows that the flow in the tropics is dominated by transients (and thus mixing) of all kinds. The mean overturning is only about a third of the daily mean variance for a month and much less if the intra diurnal variations and interannual variations are included.

2) The "observations" analysis makes absolutely no sense to me at all. There is a totally inadequate description of what is done and no way to decipher what a dot in Fig 5 or Fig 6 is. Given 20 months, and daily values (how was that done?) why are there only about 330 points? Why isn't Fig 6 part of Fig. 5?

In any event the results are totally at odds with other evidence. Here I refer to the Goes Precipitation Index which uses 3 hourly data on OLR, and thus on high cloud, as an index of rainfall, and it is clear from many studies that OLR generally decreases (convection and high cloud increase) with SST, the reverse of the relationship in Fig. 5.

Moreover the whole conceptual basis for anything here is surely flawed. As stated, on short time scales SST is not changing. But clouds are NOT caused by local SST, rather they arise from either transients, like the MJO, or for the ITCZ and SPCZ (which are major operators in this region), they come from moisture convergence (P>>E) and so it is the patterns of SST (gradients) as well as where the warmest water is that determines where the convergence and clouds occur. Now in the warm pool, the convergence is focussed more on the edges, as that is where the pressure gradients are greater, and so the convergence is not where SST is necessarily highest.

In any case, moisture is not equal to cloudy air. Many analyses show that moisture is much more extensive, see for example
Trenberth, K. E., and C. J. Guillemot, 1998: Evaluation of the atmospheric moisture and hydrological cycle in the NCEP/NCAR reanalyses. {Climate Dyn.}, {14}, 213--231.

Even with such results, other factors need to be considered.
One process might be
High SST => convergence => rainfall and cloud
OR
Less cloud => more solar radiation => higher SST

Those give opposite relations and both operate. The latter is more important in the Indian Ocean where subsidence (from the Pacific) dominates.
However, it also operates over the oceans in the region in question in northern summer, because that is the monsoon season, and the main convection is over land, meaning subsidence over the ocean.

None of this is sorted out in any way in this paper.
In fact it is so bad in this regard I do not know how it got published.

In Fig 5 etc, no correlations are given, nor are their significance levels.
My rough estimate is that the correlation is about 0.2 to 0.3 and that is significant if the 330 or so points are independent. But why should I have to guess at that.
Again I would question the editorial and review process.

3) Finally, I refer you to chapter 7 of IPCC which is a more balanced assessment. Lindzen was a coauthor of that with me and others. Lindzen wrote 7.2.1 and the same figure 1 in the BAMS article was included as 7.1 in chapter 7 along with similar ones from models, showing that these things are

fully simulated in good models, although better with higher resolution.
Anyway, his arguments were fully considered in chapter 7 and you can read it to see the result. The whole of 7.2.1, including 7.2.1.1. 7.2.1.2 and 7.2.1.3 was put together originally by Lindzen, Pierrehumbert and Le Treut, but basically the final version was rewritten by me to provide better balance. Pierrehumbert is an agnostic of sorts: disbelieves everything including models but seems to have faith in simple theories. Le Treut was sound on the modeling. I did not change the substance of what they prepared, I did reshape it and polish and it ended up in a form they accepted.

Note at the end it clearly states:
"the balance of evidence favours a positive clear sky water vapour feedback of the magnitude comparable to that found in the simulations."

The 4 subsections together are quite long and throughly air the issue, much moreso than any previous IPCC report. For those of you who do not have it:
7.2.1 "Physics of the water vapour and cloud feedbacks" (draft written by Lindzen) is 1.3 pages, 7.2.1.1 (I think Pierrehumbert) "Water vapour feedback", is 1 page, 7.2.1.2 "Representation of watre vapour in models" is 1.5 pages (Le Treut) and 7.2.1.3 "Summary on water vapour feedbacks" is half a page or so.

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****

On Thu, 24 May 2001, Michael E. Mann wrote:

> FYI. I received this from a colleague. This gives you some idea of who is
> behind this latest disinformation push.
>
> A note to all regarding the Broecker piece, which has been heavily referred
> to in this and other similar recent pieces (though it is an opinion piece,
> and not peer-reviewed).
> A response by Bradley, Briffa, Crowley, Hughes, Jones, and Mann appears in
> tomorrows issue of "Science". This response simply points out that old
> fallacies that are simply reiterated in Broecker's piece...
>
> mike
>
>
>
>
>
> >   COMPETITIVE ENTERPRISE INSTITUTE
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> >   Advancing the principles of free enterprise and
> >   limited government
> >
> >
> >   5/16/01
> >
> >   Latest Global Warming Report Already Obsolete
> >

> >   By Paul J. Georgia
> >
> >
> >
> >   The United Nations Intergovernmental Panel on Climate Change
> >(IPCC) is
> >   conducting a campaign of fear to convince us that energy
> >suppression is
> >   our only salvation. The "Summary for Policymakers" of the
> >group's latest
> >   report ? the report itself has not been officially released ?
> >paints a horrific
> >   picture of a climate system gone mad.
> >
> >   The new report, known as the "Third Assessment Report" (TAR),
> >is
> >   expected to be the focal point for policymakers for the next
> >five years as
> >   they decide what to do about global warming, just as the 1995
> >Second
> >   Assessment Report has guided policymakers for the last five
> >years.
> >   Indeed, the bureaucrats driving the global warming process
> >are using the
> >   IPCC to justify their anti-energy policies. Klaus Toepfer,
> >executive
> >   director of the United Nations Environment Programme, said,
> >"The
> >   scientific consensus presented in this comprehensive report
> >about
> >   human induced climate change should sound alarm bells in
> >every
> >   national capital and in every local community."[1]
> >
> >   In the midst of this campaign, however, the science continues
> >to move
> >   apace, leaving many of the IPCC's underlying assumptions and
> >   subsequent conclusions in shambles. A sampling of scientific
> >studies
> >   published after the completion of the final drafts of the TAR
> >is presented
> >   here to give the reader a taste of the constant flux of
> >scientific inquiry and
> >   our rapidly changing understanding of the climate system.
> >Indeed, if
> >   recent studies are correct there would be little
> >justification for Kyoto-style
> >   policies that would ultimately impede humanity's ability to
> >provide itself
> >   with the wealth- and health-enhancing benefits of modern
> >civilization.
> >
> >   Water Vapor Feedback. The biggest uncertainty in climate
> >science
> >   remains "feedback" effects on the climate. The conventional
> >explanation
> >   by proponents of global warming theory always assumes that
> >   human-induced increases in atmospheric concentrations of
> >greenhouse
> >   gases, primarily carbon dioxide, could lead to catastrophic
> >warming of
> >   the planet. Man-made greenhouse gas emissions, however, are
> >only an

> >   indirect cause of the forecasted warming. A doubling of
> >carbon dioxide
> >   concentrations alone would lead to slight warming of about
> >one degree
> >   Celsius (1.8 degrees Fahrenheit) over the next 100 years.
> >This small
> >   amount of warming, according to standard global warming
> >theory, speeds
> >   up evaporation, thereby increasing the amount of water vapor
> >(a major
> >   greenhouse gas) in the atmosphere. This "positive water
> >vapor feedback"
> >   effect is where most of the predicted warming comes from.
> >This
> >   assumption has never been tested.
> >
> >   A recent study in the Bulletin of the American Meteorological
> >Society
> >   suggests that the reverse is true.[2] The authors find a
> >negative water
> >   vapor feedback effect that is powerful enough to offset all
> >other positive
> >   feedbacks. Using detailed daily observations of cloud cover
> >from
> >   satellites in the tropics and comparing them to sea surface
> >temperatures,
> >   the researchers found that there is an "iris effect" in which
> >higher
> >   temperatures reduce the warming effect of clouds.
> >
> >   According to a NASA statement about the study, "Clouds play a
> >critical
> >   and complicated role in regulating the temperature of the
> >Earth. Thick,
> >   bright, watery clouds like cumulus shield the atmosphere from
> >incoming
> >   solar radiation by reflecting much of it back into space.
> >Thin, icy cirrus
> >   clouds are poor sunshields but very efficient insulators that
> >trap energy
> >   rising from the Earth's warmed surface. A decrease in cirrus
> >cloud area
> >   would have a cooling effect by allowing more heat energy, or
> >infrared
> >   radiation, to leave the planet."[3]
> >
> >   The researchers found that a one degree Celsius rise in ocean
> >surface
> >   temperature decreased the ratio of cirrus cloud area to
> >cumulus cloud
> >   area by 17 to 27 percent, allowing more heat to escape.
> >
> >   In an interview, lead author Dr. Richard S. Lindzen said the
> >climate
> >   models used in the IPCC have the cloud physics wrong. "We
> >found that
> >   there were terrible errors about clouds in all the models,
> >and that that will
> >   make it impossible to predict the climate sensitivity because
> >the
> >   sensitivity of the models depends primarily on water vapor
> >and clouds.
> >   Moreover, if clouds are wrong, there's no way you can get

> >water vapor
> >   right. They're both intimately tied to each other." Lindzen
> >argues that
> >   due to this new finding he doesn't expect "much more than a
> >degree
> >   warming and probably a lot less by 2100."[4]
> >
> >   The study is the best empirical confirmation to date of the
> >negative
> >   feedback hypothesis proposed by Lindzen early on in the
> >global warming
> >   debate. It builds on earlier empirical work by Drs. Roy
> >Spencer of NASA
> >   and William Braswell of Nichols Research Corporation. Their
> >1997 study
> >   also cast doubt on the assumption of a positive water vapor
> >feedback
> >   effect.[5] They found that the tropical troposphere, the
> >layer of air
> >   between 25,000 and 50,000 feet, is much dryer than climate
> >modelers
> >   previously thought. Further empirical work will no doubt
> >confirm whether
> >   this phenomenon is common throughout the tropics, which act
> >as the
> >   Earth's exhaust vents for escaping heat.
> >
> >
> >   Black Carbon. In 1995, the IPCC had to explain in its Second
> >   Assessment Report why its previous predictions of global
> >temperature
> >   change were nearly three times larger than observed in the
> >actual
> >   temperature record. The SAR concluded that emissions of
> >sulfate
> >   aerosols from burning coal were offsetting the warming that
> >should be
> >   caused by carbon dioxide levels in the atmosphere. Sulfate
> >aerosols,
> >   according to this explanation, reflect incoming solar
> >radiation back to
> >   space, thereby cooling the planet.
> >
> >
> >   The TAR takes the sulfate aerosol idea even further. The SAR
> >had
> >   predicted a temperature rise of 1 to 3.5 degrees C (1.8 to
> >6.3 degrees F)
> >   over the next 100 years. The TAR goes even further,
> >anticipating a 1.4 to
> >   5.8 degrees C (2.52 to 10.44 degrees F) rise in temperature.
> >The
> >   extreme case scenario of a 5.8 degrees C of warming, for
> >instance, is
> >   based partly on assumptions that the whole world will raise
> >its level of
> >   economic activity to that of the U.S., will equal U.S. per
> >capita energy
> >   use, and energy use will be carbon intensive. The primary
> >assumption
> >   behind the new scenario, however, is that sulfate aerosol
> >emissions will
> >   be eliminated by government regulation, giving carbon dioxide

> >free
> >   reign.[6]
> >
> >   Sulfate aerosols, then, are a key component of catastrophic
> >global
> >   warming scenarios. Without them, the IPCC cannot explain why
> >the
> >   earth is not warming according to their forecasts, nor can
> >they
> >   reasonably claim that global warming will lead to
> >catastrophes of biblical
> >   proportions.
> >
> >   A new study in Nature eliminates sulfate aerosols as a
> >corrective for the
> >   models. [7] The author, Mark Jacobson, a professor with the
> >Department
> >   of Civil & Environmental Engineering at Stanford University,
> >examines
> >   how black carbon aerosols affect the Earth's climate. Unlike
> >other
> >   aerosols that reflect solar radiation back into space, black
> >carbon (soot)
> >   absorbs solar radiation, thereby raising atmospheric
> >temperatures.
> >
> >   Until now the warming influence of black carbon was thought
> >to be minor,
> >   leading researchers to ignore it. James Hansen, with the
> >Goddard
> >   Institute for Space Studies, in a paper published in August
> >2000, first
> >   suggested that black carbon plays an important role in global
> >   warming.[8] Jacobson found "a higher positive forcing from
> >black carbon
> >   than previously thought, suggesting that the warming effect
> >from black
> >   carbon may nearly balance the net cooling effect of other
> >anthropogenic
> >   aerosol constituents."
> >
> >   There you have it. Soot offsets the cooling effect of other
> >aerosols,
> >   meaning we are back at square one. Scientists still do not
> >have a
> >   plausible explanation for why the Earth has failed to warm in
> >line with
> >   climate model results. Indeed, all the prognostications of
> >the IPCC are
> >   wrong if the Nature study is right.
> >
> >
> >   Natural Cycles. The main propaganda device of the TAR is the
> >"hockey
> >   stick graph." The graph is a temperature record derived from
> >tree rings
> >   dating back to 1000 AD and running through 1900, with the
> >20th century
> >   thermometer-based temperature data attached at the end.[9]
> >It claims to
> >   show that global temperatures have remained steady or even
> >decreased
> >   during the last millennium until the industrial age, when

```
> >there was an
> >                anomalous warming represented by the blade of the hockey
> >stick.  The
> >                hockey stick is largely bogus, however.  The margin of error
> >is so large
> >                that nearly any temperature trend could be drawn to fit
> >within it.
> >
> >
> >
> >                The hockey stick features prominently in all of IPCC Chairman
> >Robert
> >                Watson's speeches, and to the uninitiated it is very
> >persuasive.  Senator
> >                John McCain (R-AZ), for example, expressed alarm when he saw
> >the
> >                graph at Commerce Committee hearings last May.
> >
> >
> >                Watson uses the hockey stick to claim that current warming is
> >greater
> >                than at any other time in the last 1,000 years.  The Medieval
> >Warm
> >                Period (MWP) and the Little Ice Age (LIA) were two naturally
> >occurring
> >                events during the last millennium where the range of global
> >temperature
> >                change exceeded that of the 20th century.  During the MWP,
> >global
> >                temperatures were higher than they are today.  The MWP,
> >however, does
> >                not show up in the hockey stick graph.
> >
> >                The hockey stick has effectively been dismantled in a recent
> >study in
> >                Science, however.[10]  Wallace Broecker, of the
> >Lamont-Doherty Earth
> >                Observatory, argues that the MWP and the LIA were indeed
> >global
> >                phenomena.  Referring to the hockey stick, Broecker notes, "A
> >recent,
> >                widely cited reconstruction leaves the impression that the
> >20th century
> >                warming was unique during the last millennium.  It shows no
> >hint of the
> >                Medieval Warm Period (from around 800 to 1200 A.D.) during
> >which the
> >                Vikings colonized Greenland, suggesting that this warm event
> >was
> >                regional rather than global. It also remains unclear why just
> >at the dawn
> >                of the Industrial Revolution and before the emission of
> >substantial
> >                amounts of anthropogenic [manmade] greenhouse gases, Earth's
> >                temperature began to rise steeply."
> >
> >
> >                Broecker reviewed several scientific studies which
> >reconstruct the Earth's
> >                temperature history into the distant past using various
> >proxies.  He
> >                concludes, "The post-1860 natural warming was the most recent
> >in a
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> >               series of similar warmings spaced at roughly 1500-year
> >intervals
> >               throughout the present interglacial, the Holocene."[11]  In
> >other words,
> >               the current warm period may just be attributable to natural
> >cycles.
> >
> >
> >               Flawed Temperature Data.  The National Oceanic and
> >Atmospheric
> >               Administration (NOAA) claimed that the year 2000 was the
> >sixth
> >               warmest since 1880.  Other temperature records find less
> >warming.[12]
> >               Last year was only the 14th warmest, or 9th coolest, year
> >since 1979
> >               according to the satellite temperature record,[13] and only
> >the 9th
> >               warmest, according to records that include only measurements
> >from
> >               meteorological stations.[14]
> >
> >               The NOAA data, which is cited by government officials and the
> >news
> >               media, may be the least accurate, according to a study that
> >recently
> >               appeared in Geophysical Research Letters.[15]  The NOAA
> >datasets "are
> >               a mixture of near-surface air temperatures over land and sea
> >water
> >               temperatures over oceans," according to lead author Dr. John
> >Christy,
> >               professor of atmospheric science and director of the Earth
> >System
> >               Science Center at the University of Alabama in Huntsville.
> >
> >               Since actual air temperature data over many large ocean areas
> >are
> >               nonexistent, the NOAA uses sea surface temperatures as a
> >"proxy,"
> >               assuming that sea surface temperatures and air temperatures
> >move in
> >               lock step.  This is not the case, according to the data
> >compiled by
> >               Christy and his colleagues at the Hadley Centre of the United
> >Kingdom's
> >               Meteorological Office, who worked on the study.  The
> >researchers used
> >               buoy data in the tropical Pacific Ocean to compare "long-term
> >(8-20 year)
> >               trends for temperatures recorded one meter below the sea
> >surface and
> >               three meters above it."
> >
> >               What they found was a significant discrepancy.  "For each
> >buoy in the
> >               Eastern Pacific, the air temperatures measured at the three
> >meter height
> >               showed less of a warming trend than did the same buoy's water
> >               temperatures at one meter depth," the study said.  The
> >difference is a
> >               near-surface seawater warming trend of 0.37 degrees C per
> >decade and
```

> >   an air temperature trend of only 0.25 degrees C per decade
> >during the
> >   20-year period tested. Replacing the sea surface
> >temperatures with the
> >   air temperature data reduces the Earth's global warming trend
> >by a third,
> >   from 0.19 to 0.13 degree C per decade.
> >
> >   This is significant due to difficulties with reconciling the
> >various global
> >   temperature data sets, particularly the discrepancy between
> >tropospheric
> >   temperatures measured by satellites that show little to no
> >warming, and
> >   the surface-based temperature data that show slightly more
> >warming.
> >   Last year, the National Research Council stated that both
> >temperature
> >   records are correct and speculated about an explanation.[16]
> >
> >   This brings up another problem, however. The standard
> >explanation of
> >   the greenhouse effect suggests warming occurs first five
> >kilometers
> >   above the earth's surface in the atmospheric layer known as
> >the
> >   troposphere. How events at the surface are connected to what
> >happens
> >   high in the atmosphere is not clear, but it is believed that
> >surface
> >   warming would follow tropospheric warming through climatic
> >processes
> >   such as air circulation.[17] If both temperature records are
> >correct, then
> >   this explanation of the greenhouse effect is wrong. Christy
> >et al. brings
> >   the surface temperature data into closer agreement with the
> >satellite
> >   data, suggesting that a better explanation for the
> >discrepancy is flawed
> >   surface data.
> >
> >   Progressive Science. At a press conference at the National
> >Press
> >   Club on April 18, Mr. Jan Pronk, chairman of the Sixth
> >Conference of the
> >   Parties of the United Nations Framework Convention on Climate
> >Change
> >   said most issues were still on the table in the ongoing Kyoto
> >negotiations
> >   but the scientific basis of catastrophic global warming could
> >not be
> >   questioned. That would be like going back ten years, he
> >said. This is a
> >   myopic and erroneous view of science. Science is not static
> >but
> >   dynamic. It reaches tentative conclusions at best, and those
> >   conclusions constantly give way to new data. The IPCC is a
> >static
> >   process, however. The Third Assessment Report is already
> >obsolete and
> >   it has not even been released yet. With these four recent
> >studies, it may

> >   be time to bid catastrophic global warming theory a warm
> >farewell.
> >
> >
> >
> >
> >
> >   [1] "Evidence of Rapid Global Warming Accepted by 99 Nations,"
> >Environment News Service, January 22,
> >   2001.
> >   [2] Richard S. Lindzen, Ming-Dah Chou, and Arthur Y. Hou, "Does the
> >Earth Have an Adaptive Infrared Iris?,
> >   Bulletin of the American Meteorological Society, 82:417-32, March
> >2001.
> >   [3] ftp://www.gsfc.nasa.gov/pub/PAO/Releases/2001/01-18.htm
> >   [4] "Is Globe Warming? Sure, But Far Less than Alarmists Say,"
> >Tech Central Station
> >   (http://www.techcentralstation.com/BigShotFriday.asp), March 5,
> >2001.
> >   [5] Roy W. Spencer and William D. Braswell, "How Dry is the
> >Tropical Free Troposphere? Implications for
> >   Global Warming Theory," Bulletin of the American Meteorological
> >Society, 78:1097-1106.
> >   [6] In correspondence with Nature magazine, one of the IPCC's
> >coordinating lead authors, Thomas Stocker of
> >   the Physics Institute at the University of Bern in Switzerland,
> >wrote, "First, although climate modeling has
> >   advanced during the past five years, this is not the main reason
> >for the revised range of temperature
> >   projections. The higher estimates of maximum warming by the year
> >2100 stem from a more realistic view of
> >   sulphate aerosol emissions. The new scenarios assume emissions
> >will be reduced substantially in the coming
> >   decades, as this becomes technically and economically feasible, to
> >avoid acid rain. Sulphate emissions have
> >   a cooling effect, so reducing them leads to higher estimates of
> >warming." See "Climate panel looked at all
> >   the evidence," Nature, 410: 299, March 15, 2001.
> >   [7] Mark Z. Jacobson, "Strong radiative heating due to the mixing
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> >   [8] James D. Hansen, Makiko Sato, Reto Ruedy, Andrew Lacis, and
> >Valdir Oinas, "Global Warming in the
> >   twenty-first century: An alternative scenario," Proceedings of the
> >National Academy of Sciences,
> >   97:9875-9880.
> >   [9] The tree ring data originated with Michael E. Mann, Raymond S.
> >Bradley and Malcolm K. Hughes,
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> >Inferences, Uncertainties, and Limitations,"
> >   Geophysical Research Letters, 26: 759, March 15, 1999.
> >   [10] Wallace S. Broecker, "Was the Medieval Warm Period Global?"
> >Science, 291: 1497-99, February 23,
> >   2001.
> >   [11] Also see H.H. Lamb, Climate History and the Modern World, (New
> >York: Routledge, 1985), and Brian
> >   Fagan, The Little Ice Age: How Climate Made History, 1300-1850,
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> >   [12] http://www.ncdc.noaa.gov/ol/climate/research/2000/ann/ann.html
> >   [13] http://www.ghcc.msfc.nasa.gov/MSU/msusci.html
> >   [14] http://www.john-daly.com/press/press-01.htm#Phil
> >   [15] John R. Christy, David E. Parker, Simon J. Brown, Ian Macadam,
> >Martin Stendal, and William B. Norris,

> >   "Differential Trends in Tropical Sea Surface and Atmospheric
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> >   [16] Reconciling Observations of Global Temperature Change,
> >National Academy Press: Washington, D.C.,
> >   2000.
> >   [17] Richard S. Lindzen, "Climate Forecasting: When Models are
> >Qualitatively Wrong," George C. Marshall
> >   Institute, Washington, D.C., 2000.
> >
> >
> >
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>
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From: Keith Briffa <k.briffa@uea.ac.uk>
To: Stepan Shiyatov <stepan@ipae.uran.ru>
Subject: Re: Article and money
Date: Fri Jun 8 13:38:08 2001

   Stepan
   it is just pressure of work. I am afraid the final report did not go to INTAS . I will do
   it this week! I still expect we will get the money outstanding - just late . Sorry.
   Keith
   At 02:22 PM 5/31/01 +0600, you wrote:

     Dear Keith,
     Thank you for the print of collaborative article published in the J.
     of Geophysical Research I have received some days ago. The article is
     very interesting and, I think, these reconstructions will be used by
     many researchers of different disciplines.
     At the end of the last year Janet asked me to send the account of the
     bank to transfer the rest money of the INTAS project (737 Euros). I have
     sent you the necessary form to transfer the money for my name, but
     the Ekaterinburg Branch of Bank for Foreing Trade did not receive the
     money until now. Do you know the reason?
     This summer I am very busy. Along with Fritz Schweingruber and his team
     (four persons)we will visit many sites (using helicopter) on the North of
     European and Siberian Arctic and Subarctic (from the Lower of Pechora
     river in the west to the Lover of Khatanga river in the East). We will
     try to find a new sources of subfossil wood material between the Yamal
     Peninsula and Taimyr Peninsula, on the one hand, and between the Yamal
     Peninsula and Kola Peninsula, on the other hand. The second aim is to
     collect samples from living trees of different ages for estimating

biomas changes.
After this trip I and my post-graduate student will be working in the
Polar Urals (large-scale mapping of forest-tundra ecosystems over the
forest-tundra ecotone for three time intervals: the beginning of the
XXth century, the 1960ties and the 2000ties.
At the end of September I intend to be in Davos.
Best Regards,
Stepan
stepan@ipae.uran.ru

--
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##

From: Phil Jones <p.jones@uea.ac.uk>
To: "Michael E. Mann" <mann@virginia.edu>, Thomas R Karl <Thomas.R.Karl@noaa.gov>
Subject: Re: NRC report on climate change
Date: Tue, 12 Jun 2001 08:46:36 +0100
Cc: trenbert@ucar.edu,"Michael E. Mann" <mann@virginia.edu>, rbradley@geo.umass.edu,tom crowley <tom@ocean.tamu.edu>, mhughes@ltrr.arizona.edu,k.briffa@uea.ac.uk, Folland Chris <ckfolland@meto.gov.uk>

<x-flowed>

Dear All,
I'd just like to echo all the points made by Mike and Kevin. The logic behind saying that there isn't enough paleo data before 1600 yet there may have been even early millennia which experienced warming of almost 2 C per millennium escapes me. As Kevin points out they have mixed up all the various factors that force climate on interannual to intermillennial timescales.
One of the main points of IPCC is to synthesize the science, with particular reference to potential future changes. Changes in the distant past (glacial and deglaciation) are of less relevance to the 21st century because of differences in boundary conditions. The last few hundred to a thousand years are clearly more important to the near future. At least from my quick reading there seems no explicit reference to changes in the thermohaline circulation.
Perhaps the paleo people on this list need to redouble their efforts to empahasize the importance of the last few thousand years, stressing absolute dating, calibration and verification. Another issue that is mixed up in the report (apart from the forcing) is spatial scales. I will try and address these at the Chicago meeting. What are the 4 useful sites ?
I just hope in the US that people read the full IPCC reports and the summaries, rather than this hastily cobbled together document. I also hope that Europeans don't read it. It has already got some air time here and may get some more with Bush here this week. Issues

  like star wars and capital punishment were commented upon whilst I came to work. Kyoto
  wasn't mentioned.

  Cheers
  Phil

At 10:45 11/06/01 -0400, Michael E. Mann wrote:
>Hi Tom,
>
>Thanks for your message. I know how hard you worked to make the report as
>balanced as possible, and realize this experience must have been a bit
>frustrating for you, after all the careful and hard work you and Chris put
>into our IPCC chapter. While the idea that the limited panel involved in
>the NAS report can provide an improved or more objective assessment of the
>science relative to IPCC seems, of course, ridiculous to a lot of us. But
>I'm very thankful you were on the panel. Needless to say, my criticism
>below is in no way directed towards you, but rather some of the other
>panel members whom I think did a real injustice to the science.
>
>Having seen the list of authors and reviewers of the report, I think I
>have a pretty good idea what the source of a good deal of that skepticism
>is and I think much of it is spurious and unfair. There are legitimate
>caveats and uncertainties--I think we've been very honest about these in
>our publication, and we (as Phil, Keith, and others) are working earnestly
>to improve the reconstructions. But the claims we make (e.g. the
>anomalousness of recent warmth) are guided by the substantial
>uncertainties in the reconstructions, which of course take into account
>uncertainty due to increasingly sparse information back in time, and I
>have yet to see any legitimate argument that our reconstruction (or Phils,
>Toms, Keiths, etc.) is "wrong" within the context of the diagnosed
>uncertainties. Unfortunately, much of the criticsm that has been advanced
>recently is knee-jerk and unsubstantiated, particularly with regard to
>dendroclimatological issues (which Malcolm and Keith can comment on best).
>Much of this has to do w/ a lack of understanding of tree ring information
>(to be honest Tom, I didn't see one name in the list of authors or
>reviewers of the NAS report whom I think is qualified to comment on
>dendroclimatological climate reconstruction and its strengths and
>weaknesses, and that is a real problem. In such a vaccum it is easy, for
>example, for Wally to wave around some highly non-standard, un
>peer-reviewed tree-ring analysis that he has been promoting (which Ed Cook
>himself, a co-author on this, admits makes use of a questionable
>standardization approach), in an attempt to dismiss all other climate
>reconstructions which use tree ring information.
>
>The criticism that there are only "4 useful sites" for reconstructing
>climate over the past 1000 years is especially irksome and ignorant. Does
>Tom C. agree that there are only 4 meaningful records that contribute to
>his reconstruction? Does Phil, or Keith? Where does that number come from?
>The same source as R.L.'s GHG sensitivity factor of 1.0 (i.e., the ether)
>I suspect.
>
>The discussion of paleo in the report (which I realize you had very
>limited control over) is disturbingly misleading and flawed to many of us
>who actually work in this area. There are throwaway statements about
>millennial trends of 2 C in global temperatures being typical during the
>early Holocene that have no basis in fact. They are again probably based
>on this increasingly disturbing notion that Arctic ice core borehole
>thermometry or other ice core information tells us anything at all about
>the hemisphere let alone globe. A small number of scientists are really
>misleading the scientific community in this regard. How odd that the panel
>was happy to claim that there were millennial periods with 2 degree C

>warming in global temperature during the holocene (for which there is no
>reliable empirical evidence whatsoever) and yet focuses its skepticism on
>much more detailed and careful assessments of the most recent millennium.
>I think you can see why some of us are frustrated by this type of
>inconsistency, and suspect some degree of bias or agenda at work. There
>was a clear bias in the panel in the promotion of ice cores (which sample
>a very limited portion of the globe and are very questionable in their
>ability to say *anything* about hemispheric or global temperature
>variations). I am disturbed by this because the NAS report shouldn't have
>been promoting a particular specific area of funding. It seems to have.
>
>Finally, with regard to one of the primary supposed discrepancies in the
>paleo record of the past 1000 years, temperature reconstructions from
>boreholes vs. other proxies, I'll be presenting some results in Chicago
>which I think you'll all find quite elucidating. Turns out there is no
>discrepancy after all. More on that soon. I'll also try to confront both
>the "real" and "imagined" sources of uncertainty and bias in
>paleoreconstructions in my presentation there, and we should all be able
>to have a very healthy discussion of this.
>
>I really think that there was a bias in this panel which cannot be
>considered representative of the community as a whole. So I vote that we
>not over-react. I'm anxious to see Lindzen, Broecker, or Mike Wallace
>publish a peer-reviewed critical analysis of proxy data over the past 1000
>years. Until that day, I take their comments w/ a shaker of salt...
>
>mike
>
>At 09:41 AM 6/11/01 -0400, Thomas R Karl wrote:
>>Kevin,
>>
>>I agree with most of your points. It was a very interesting Panel. I should
>>emphasize however, that the Paleo record (at least the last 1000 years)
>>has many
>>critics, and we really need to show how the data prior to 1600 stands
>>up. Some
>>contend there are only 4 good sites in the first part of the record. I
>>am not sure
>>of this, perhaps Mike and others will explain this in Chicago.
>>
>>Regards, Tom
>>
>>Kevin Trenberth wrote:
>>
>> > FYI
>> >
>> > Some comments on the NRC/NAS report on the IPCC and global warming
>> >
>> > Kevin Trenberth
>> > 6/7/2001
>> >
>> > While the report overall is an endorsement of the IPCC report and the
>> > process, it has a lot of "buts" in it, and the overall tone is to somewhat
>> > downplay the problem. It does not focus on policy relevant issues. The
>> > report was done in a very hurried fashion and perhaps as a result,
>> there are
>> > several factual errors or misstatements and there are errors of
>> omission. My
>> > impression is that it tends to overstate the caveats and need for
>> questioning
>> > of results and understate the certainties and likelihoods.
>> >
>> > 1. In dealing with natural variability, there are two aspects that are

>> >   mixed in this report. There is natural variability of climate
>> >   that is tied to external forcings, such as variations in the sun,
>> >   volcanoes, and the orbital variations of the Earth around the sun. The
>> >   latter is the driver for the major ice ages and interglacials. The
>> second
>> >   kind of natural variability is that internal to the climate system
>> arising
>> >   from interactions between the atmosphere and ocean, such as El
>> Nino, for
>> >   instance. This variability occurs even in an unchanging climate.
>> >
>> >   In the section dealing with this and in the summary, both kinds of
>> >   variability are discussed as if they are the second kind. Glacial to
>> >   interglacial differences are discussed without any mention of the known
>> >   causes and as if these can happen without a cause. This is
>> misleading at
>> >   best. A consequence is that there is no clear statement that the
>> >   recent warming is outside the realm of natural variability - and that a
>> >   cause is needed. And the cause is human induced changes in the
>> >   atmospheric composition.
>> >
>> > 2. The report does not clearly address issues in attribution of recent
>> >   climate change to human activities. At the end of p 3 in the
>> summary it
>> >   makes an equivocal statement. It avoids the issue that the recent
>> >   temperature increase is outside any estimates of natural variability
>> >   without any forcings. What else is the warming due to?
>> >
>> >   On p 14, it does not sum up the forcings and make a clear statement
>> about
>> >   the total. Nowhere does it say that the recent warming has to be
>> because
>> >   of an increase in heating. This reasoning also put limits on how large
>> >   aerosol cooling can be.
>> >
>> >   On p 17, the ambiguity over the term "natural forcing" is used to
>> say that
>> >   a causal link can not be unequivocally established. It does not mention
>> >   estimates of variability from the paleo record and how well they
>> agree (or
>> >   not) with model estimates.
>> >
>> >   It does not note on p 17 that many models show the signal of
>> greenhouse gas
>> >   effects emerging from the noise of natural variability about 1980. The
>> >   attribution statement is weak.
>> >
>> > 3. Several statements about the hydrological cycle, rainfall, and
>> warming are
>> >   misleading and even wrong. One direct consequence of this is that
>> >   statements about changes in extremes are missing, understated and
>> incorrect.
>> >   Another is to understate the threats in the tropics and subtropics.
>> >
>> >   It begins in the first sentence of the summary: "Greenhouse gases are
>> >   accumulating as a result of human activities, causing surface air
>> >   temperatures and subsurface temperatures to rise." Later in the
>> paragraph
>> >   it states "Secondary effects are suggested by computer model
>> simulations
>> >   and basic physical reasoning. These include increases in rainfall
>> rates
>> >   and increased susceptibility of semi-arid regions to drought."

>> >   While the first statement is true, is is misleading. The increased
>> >   greenhouse gases cause increased heating (also called radiative
>> forcing in
>> >   this report). It is also referred to as "warming". The latter term is
>> >   ambiguous and misused in this report, by confusing where it should mean
>> >   "heating" versus where it should mean "increased temperature". So
>> while
>> >   some of the increased heating does in fact cause an increase in surface
>> >   temperature, much of the heating goes into evaporation of surface
>> >   moisture. This changes the moisture content of the atmosphere and
>> >   rainfall. This increase in the hydrological cycle is NOT a secondary
>> >   effect, it is a primary one.
>> >
>> >   Moreover, the increase in atmospheric moisture content is much
>> greater than
>> >   the increase in evaporation, because it is controlled by the
>> temperature
>> >   (which determines the water holding capacity of the atmosphere
>> through the
>> >   so-called Clausius Clapeyron effect) while the evaporation is
>> controlled
>> >   by the surface heating. For doubled CO2, evaporation and the overall
>> >   hydrological cycle speeds up by about 3%, but the moisture in the
>> >   atmosphere increases by about 6% per degree C, or about 15% for a
>> doubling
>> >   of CO2.
>> >
>> >   The rainfall intensity is determined by the available moisture, and
>> so it
>> >   increases at about the latter rate. But the total precipitation
>> increases
>> >   only at the former rate, and so the frequency of precipitation must
>> >   decrease in some way. This also means that the residence time for
>> water
>> >   vapor increases in a world with increased heating. The increased
>> drying
>> >   means increased risk of drought everywhere, not just semi-arid
>> locations,
>> >   and increased intensity increases risk of floods. These increases
>> in risk
>> >   of extremes are direct consequences and are not adequately
>> mentioned. In
>> >   the section on "Future climate change", p 19, one statement is
>> wrong: "An
>> >   increase in the recycling rate of water in the hydrological cycle is
>> >   anticipated in response to higher global average temperatures." The
>> >   increased hydrological cycle is in response to increased heating, not
>> >   increased temperatures (and may not occur if only the temperature is
>> >   increased). The term "recycling" is normally used to refer to
>> moisture that
>> >   evaporates and precipitates in the same catchment, and is
>> misleading here.
>> >
>> >   A consequence of all this is that in the summary on p 4 in
>> addressing the
>> >   question "What will be the consequences of global warming (e.g.,
>> extreme
>> >   weather, ...)...", there is no statement about increased risks of
>> extremes
>> >   of floods and droughts, and heat waves. It also underplays the
>> risks of
>> >   increases in pests and diseases (like fungal diseases) in agriculture.
>> >

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>> > 4) The report contends that emissions in the last decade have averaged
>> less
>> >    than in IPCC predictions, notably for CO2 and methane. However, the
>> IS92c
>> >    scenario had flat CO2 emissions till 2020 and then declining
>> emissions to
>> >    2100, and for methane values projected are quite close to those
>> observed.
>> >    In any case they are not forecasts but scenarios, to be used for
>> planning
>> >    purposes.  Statements in the summary on p 4 and on p19 are misleading.
>> >    Also, the claim that CO2 emissions will accelerate for mid-range
>> estimates
>> >    is not true: those have emissions increasing at a close to constant
>> rate.
>> >
>> > 5) The report dodges the issue of what is a "safe" level of
>> concentration of
>> >    greenhouse gases, and has a strong US bias.  It does not list on p
>> 21, for
>> >    instance, the vulnerability of small island States to sea level
>> rise and
>> >    of poorer countries to all aspects of climate change.  Again it avoids
>> >    discussion of changes in extremes.  It is also incorrect in stating
>> "The
>> >    largest changes occur consistently in the regions of the middle to high
>> >    latitudes."  This is true only for temperature and NOT for
>> precipitation
>> >    (also p 8) perhaps because of the issues raised in item 2).
>> >    Therefore it understates the threats to tropical countries.
>> >
>> > Some details:
>> >
>> > p 6: The accepted value of forcing for doubled CO2 with a stratosphere in
>> > adjustment (which occurs rapidly) is 3.5 W m-2, not 4.
>> >
>> > p 11: sheep are just as much a source of methane as cows and cattle.
>> >
>> > p 24: the list of variables needed for an observing system should include
>> > those for the ocean.
>> >
>> > ---------------
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From: "Michael E. Mann" <mann@virginia.edu>
To: "Dr. Nanne Weber" <weber@knmi.nl>
Subject: Re: workshop report
Date: Mon, 18 Jun 2001 11:50:15 -0400
Cc: "Michael E. Mann" <mann@virginia.edu>, Julia Cole <jcole@geo.arizona.edu>, rbradley@geo.umass.edu, jto@u.arizona.edu, storch@gkss.de, wanner@giub.unibe.ch, tom crowley <tom@ocean.tamu.edu>, k.briffa@uea.ac.uk

<x-flowed>
Hi Nanne,

Thanks for your comments. I've asked Julie Cole, who is attempting a revised draft, to incorporate your suggestions. Hans or you should also provide a revised paragraph 7 that is more to your liking than what I wrote.

I'm requesting that Julie wait until the end of this week (Friday, Jun 22) to give the others time to get their comments in also. Then, after Julie provides me w/ her revised draft, I'll try to make a few more small changes and sent that onto the group for suggested final changes.

I hope this sounds acceptable to all concerned?

thanks,

mike

At 03:22 PM 6/18/01 +0000, Dr. Nanne Weber wrote:
>Hi Mike (and others),
>
>Below follow some comments on the draft report for EOS that you send
>around. The
>general outline is fine for me. Responding to Julie's comment on the
>large-scale/regional
>reconstruction issue: I guess that the three different approaches
>mentioned are not
>necessarily restricted to large-scale. Especially (1) can be for all
>scales, (2) will work
>better for large scales, but (3) could be very well applied to regional
>scales
>like African monssoon or NAO. However, I do think that this 'scales
>issue'
>should be addressed explicitly in the text (as indicated in my
>comments).
>
>We can not cover all of the workshop in a small EOS report, but I do
>think
>that there should be more emphasis on the different model strategies
>presented,
>process-based proxy modeling and some more mention of historical
>documentary data.
>
>I am willing to take my share in the rewriting task. Just let me know

>what is most
>convenient for you.
>
>
>One practical point: the Netherlands funding agency is called National
>Research Program (NRP) of the Netherlands (KNMI is my affiliation, but
>it
>did not pay the bill)
>
>Thanks,
>
>Nanne
>===
>
>
>First para, first sentence: name all boundary conditions relevant
>for geological timescales (astronomical forcing, orography, GHG
>concentrations)
>or none.
>
>First para, fifth sentence: Three distinct approaches have .... in
>reconstructing
>the LARGE-SCALE AND REGIONAL climate history of past centuries and
>millenia.
>
>First para, point (3): the assimilation of paleoclimatic proxy data
>directly
>into (leave out 'forced') climate model integrations (using statistical
>models to upscale the proxy data to large-scale climatic patterns), in a
>manner
>conceptually etc,.
>
>
>
>Second para: can be written in a more condense manner. One ore two
>sentences
>discussing the large-scale versus regional climate issue should be
>added. For example:
>(i) add after the second sentence ('The first method...'): This holds
>for
>spatial scales ranging from local (in the case of site-by-site
>calibration) to
>large scale (in the case of pattern calibration, e.g. ENSO and NAO) and
>up to hemispheric/global.
>(ii) add just before 'It was our belief that a meeting': The second and
>third
>approaches are more suitable for reconstructing the actual large-scale
>climatic
>state, as the local climate is inherently noisy and only to a limited
>amount determined by external forcing or related to large-scale patterns
>
>like e.g. the NAO.
>
>Second para, modify the description of the third approach as follows:
>The third approach can be thought of......, but it is nudged
>toward the actual observed large-scale climatic state at the time
>resolution provided by the proxy data. This method is more resistant to
>the potential biases......model-based approaches, but it is relatively
>untested to the application of proxy data.
>
>
>
>Fourth para: leave out second sentence "A frequency-domain...' (too much

>
>technical detail, in a too condensed form to be understandable to a
>general
>reader of EOS).
>
>
>
>Fifth para: very much biased toward the modeling of large-scale, forced
>signal.
>My go at modelling paragraph(s):
>Three types of modelling experiments were distinguished: free
>simulations without
>any external forcing, giving insight into the patterns and timescales of
>
>internally-generated variability, forced simulations and simulations
>constrained
>by the assimilation of proxy data. Examples were presented, where models
>used ranged
>from an energy balance model (EBM), an intermediate-complexity climate
>model (EMIC) to
>atmospheric and coupled General Circulation Models (GCM). Simulations
>with an
>EBM as well as a GCM appear to explain variations over
>century-to-decadal timescales
>in proxy-based reconstructions of the Northern Hemisphere temperature
>over the past millenium, using estimated changes in radiative forcing
>(solar
>irradiance changes, volcanic activity, GHG and aerosol concentrations).
>Discrepancies,
>however, etc.... (a bit long as it is now).
>
>Process-based models of glaciers and sea level were used to generate
>synthetic
>records of these low-frequency proxies on the basis of EMIC and GCM
>simulations,
>using unforced runs as well as orbital and solar-forced runs.
>Over longer timescales simulated glacier lengths and sea level
>variations
>can be used to validate the models response in climatic parameters
>which are not well constrained by existing proxy data, like the
>hydrological cycle.
>In addition, model-data intercomparisons can be carried
>out on the level of the proxy itself rather than on the level of
>reconstructed
>climatic variables. Such process-based models require an understanding
>of local meteorological processes as well as the complicated (physical,
>biological or
>chemical) processes determining the proxy itself. A promising new model
>of
>tree-ring growth was presented.
>
>A new data-assimilation approach to paleoclimatic reconstruction DATUN
>(..)
>was discussed at length....
>This paragraph is not very clear as it is. I can have a go at it,
>but maybe Hans should.
>
>
>
>Para seven: This could be much shorter. Several points are mentioned
>here
>for the first time--> move up te earlier paragraphs (as indicated above)
>

>....currently emphasized high-resolution proxies such as tree rings,
>HISTORICAL
>DOCUMENTARY DATA, corals and ice cores. In addition, low-frequency
>climate
>variability may be reconstructed from low-resolution proxies such as
>borehole
>records, glaciers, foraminifera in marsh cores indicative of sea level
>as well
>as lake and ocean sediments which are not necessarily laminated.
>Process-based proxy models would enable to better exploit the
>information
>contained in proxy records and help to resolve the origin of apparent
>discrepancies between the different data sources. It is also important
>to
>better constrain the histories of radiative forcings prior to AD 1600.
>It was
>strongly felt that there should be an emphasis on developing
>projects....

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

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From: Martin Welp <Martin.Welp@pik-potsdam.de>
To: gberz@munichre.com, tloster@munichre.com, ccarraro@unive.it, juergen.engelhard@rheinbraun.de, guentherr@wwf.de, bhare@ams.greenpeace.org, klaus.hasselmann@dkrz.de, m.hulme@uea.ac.uk, carlo.jaeger@pik-potsdam.de, martin.welp@pik-potsdam.de
Subject: ECF: Agenda of the telephone conference 2 July 2001
Date: Thu, 28 Jun 2001 18:56:00 +0200

Dear member of the ECF steering committee,

The next telephone conference takes place on Monday, 2 July 2001 at 17.00-18.00 CET. The agenda is as follows (it may be modified at the beginning of the meeting):

1. Minutes of previous telephone conference (Draft sent by email on 14.6.2001) (5 Min.)
2. ECF preparatory meeting in Brussels (15 Min.)
    (Agenda, Inputs: project descriptions, Outputs: workplan, sketch of a position paper)
3. ECF as an Association and/or Foundation (15 Min.)
4. Three priority projects (15 Min.)
5. Varia (10 Min.)

Important!! Please check that the telephone number where you want to be called is correct.

Gerhard Berz 089-3891 5290
Carlo Carraro +39-335-6170775

Jürgen Engelhard 02235-77268
Regine Günther 069-79144177
Bill Hare 030-44678765
Klaus Hasselmann 0170-9101601
Mike Hulme (excused)
Carlo Jaeger 0331-288 2601
Martin Welp 0331-288 2619

Reminder:
General information about the ECF can be found at the ECF website:
http://www.European-Climate-Forum.net/
Background documents and internal information (e.g. the programme of the
Brussels meeting):
http://www.european-climate-forum.net/internal/
Your feedback on these sites is more than welcome!

The ECF Flyer is available now! I will send all members of the steering
committee 20 copies. If you need more of them please let me know.

Best regards,
Martin Welp

--
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http://www.pik-potsdam.de/~welp/index.html
http://www.european-climate-forum.net/

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: "Dr. Reinhard Böhm" <r.boehm@zamg.ac.at>, <p.jones@uea.ac.uk>,
<maugeri@mailserver.unimi.it>, <t.nanni@isao.bo.cnr.it>,
<m.brunetti@isao.bo.cnr.it>, <Dietmar.Wagenbach@iup.uni-heidelberg.de>,
<jones@gkss.de>, <widmann@gkss.de>, <storch@gkss.de>
Subject: Re: ALPIPMOD-brainstorming
Date: Fri Jun 29 15:10:11 2001

Hi everyone
I have been through the ideas and offer a few (aptly non organised) comments. First Phil is
away and will not be able to comment until later.
First, the project needs more explicit focus. The call will focused on natural variability
. We are offering a detailed analysis of the variability of climate in the Alpine Region
that focuses on CLIVAR timescales - basically very high resolution and not extending much
beyond a few centuries. The project incorporates instrumental , model and palaeodata . The
inter-relationships between these will be studied to gain an understanding of the nature
and mechanisms of the climate variability - but is this enough. I feel it needs

to be
linked with a strong element of understanding the range of social/economic impacts of this
variability.
Perhaps looking at aspects such as avalanches, forest damage, floods, tourism etc.? I
merely put this out as a straw man . I feel the EC are putting a lot of emphasis on this
aspect of research and incorporating research and researchers in these or similar areas
will be a big plus.
As for the specific points in the brainstorming document -
The Dendro aspect :
I think it is essential to update the Alpine tree-ring chronologies that are available .
This is because they are a proven asset but many questions regarding tree-productivity (in
relation to observed 20th century climate variability) simply can not be addressed without
doing this. Many were collected over 20 years ago. The additional data would then allow new
processing techniques to be employed and vital questions concerning the changing responses
of tree-growth to explored. The most efficient way to do this is to involve several groups
working in the Alps , (Thank you for sending the Thesis by Giorgio Strumia which is
certainly a very impressive piece of work) I would think Rupert Wimmer's group and the
Birmensdorf group would be ideal (Fritz Schweingruber has retired but Jan Esper has joined
them in his place - I can ask them to be involved but this depends on what the group here
think are the priorities and how much we see as the overall budget and institutional
allocations). I should say here that I think we would require money for a single person who
could , if it is agreed, work on aspects of tree-ring processing and relationships with
climate in association with the other tree-ring groups, but also work with the climate and
model data , especially with a view to exploring the statistical inter-relationships and
dynamical associations between the different climate data sets. There is also the French
tree-ring group at Marseille? Perhaps though not all need to partners - ALSO I am thinking
of putting together a European Tree-ring project (or suggesting it as part of a large
European integrated proxy study of Holocene variability) so if this happened there could be
a link between it (involving some of the groups mentioned) and this proposal. The Swiss
might be interested to produce selected site tree-ring density/updating which I think would
be very valuable and I will speak to them without commitment as you ask.
As for some of the climate analysis possibilities mentioned, I very much like the ideas of
detailed ,local climate comparisons with the larger CRU (and CRUder!) data. We are very
interested in the association between time dependence in the relationships between
circulation changes and changes in Temp. and Prec. Also changes in the nature of

climate
   seasonality , and also extreme events (frost frequency , drought, intense rainfall). The
   detailed analyses of these characteristics also compliments the interpretational work on
   the tree-ring and glacier mass balance (and socio economic foci) data.
   As for the glacier work - is not a huge effort already going into this? I think it is
   important but does it fit as well ? The work proposed would have to be distinguished from
   other ongoing efforts - though I do like the idea of linking the geomorphological evidence
   of past glacier change (moraines , pro-glacial sediment data?) with reconstructed glacier
   volume changes , where the reconstructions are based on new long instrumental data , and
   palaeodata (temp. and precip.) used to drive a model of the glacier volume. Our German (or
   Julie) colleagues can point to such work based on GCM output . My colleague here (Sarah
   Raper) has also done this sort of work but using a very simple model to estimate past
   Storglaciaren (in Sweden) volume changes and her results imply that these models must be
   forward driven and not based on simple regression analysis using temperature and
   precipitation to estimate past mass balance.
   The future aspects of the discussion are important - and it is true that the previous EC
   call dealt with modelling and scenarios of future changes. Here, I believe the use of
   models should be strictly limited to understanding natural /current variability and change.
   There is no benefit in going for a 2 year project - I strongly urge 3. I also would find a
   meeting difficult. I am away from 17-29 July, and 11-25 August, and in meetings during
   7-10th July and 26-31st August.
   Phil will be back here next week and will no doubt comment in more detail on the
   instrumental analysis aspects then.
   Very best wishes to all
   Keith
   At 05:13 PM 6/25/01 +0200, Dr. Reinhard Böhm wrote:

      Friends,

      As announced last Friday, we want to open a first round of brainstorming about the
      contents of our project. We have collected what we have received from You so far and
      have it mixed with our own ideas (file Brainstorming-1.doc). It does not have a nice
      structure and there are still a number of question marks, as You will see.
      Please add things where you think something is missing and please feel free to tell us
      which points make no sense, or are too ambitious or simply too much work.
      Please consider also the "how to do it" (state of the art methods, new approaches to
      solve problems, other data than those mentioned, other topics.....).
      Please try also to find Your position in the project, tell us what You would prefer to
      do....
      Please try to consider whether we would have to include other groups in terms of

scientific potential and/or in terms of data (For example: Keith Briffa you mentioned
Fritz Schweingruber as the leading data holder of Alpine tree-ring data. Do you think we
should ask him to join us, or could You use his data also without him being a contractor
of the project? In case You want him in the project could we kindly ask you to contact
him, being much more familiar with him and with the tree-ring topic than we are?)

We would be glad to receive a very short answer from everybody within this week, because
from June 30th to July 15th all the three of us will not be at the institute.
For more detailed considerations and answers You have more time, it would be nice to be
able to study them after our return by mid of July. But please use also the
possibilities to contact the other groups - the sooner we integrate to a group the
better it is.

Our time-table for the rest of the time until October:

July 16th to August 14th: We are at the institute, hoping to bring the project into a
near to final version what concerns the scientific content

August 15th to August 28th: Ice core conference at Kangerlussuaq (Greenland)

August 29th to September 17th: We are at the institute most of the time. We hope this
will be the time to elaborate the EU-shaped complete version.

September 18th to September 22nd: Big events going on in Vienna which may cut down our
time for the project (150th anniversary of our institute, Climate conference DACH-2001
(in German))

September 24th to October: Time reserved for all the things that could not be done yet
in spite of our time table

Could each of You please inform us about Your time table during summer and autumn?

A question to all of You: How do You think about one 2-days meeting this Summer or in
early September? What place do You prefer? If it is Austria we would have two low cost
possibilities: 1): at our institute and 2) (more adventurous): At the
Sonnblick-observatory (You do not have to have Alpinistic experience, we have a private
cable car going up)

Some remaining questions:

Should we try a 2-years or a 3-years project?

Can everybody live with roughly 300.000 Euro (This would result into somewhere between
1.5 and two millions, which we heard is a magnitude preferred by the

commission). Please
consider not only the sum of money but also how to spend it and how to fill it with a
reasonable equivalent in work amount.

What is your feeling about the "Climate variability atlas of the Alps"? Is it good to
have one main deliverable like that or should we better produce a number of smaller
things?

One last technical remark: Please send your comments and mails not only to Vienna, but
also to the other groups (or at least to those You believe would be interested in what
You write). I do not think this would spoil too much our mail boxes and it has the
advantage to include the whole intellectual power of our group into the construction
phase of the proposal.

Looking forward to Your replies, ideas, time tables and anything else

With best regards

Reinhard

--
Dr. Keith Briffa, Climatic Research Unit, University of East Anglia,
Norwich, NR4 7TJ, United Kingdom
Phone: +44-1603-593909   Fax: +44-1603-507784

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From: "Ian Harris (Harry)" <harryharris@btinternet.com>
To: list@norwichgreenparty.org
Subject: Re: [ngp-list] Press Release 'Global Warning' talk establishes West Norfolk Green Party
Date: Mon, 2 Jul 2001 10:24:05 +0100
Reply-to: list@norwichgreenparty.org

<x-flowed>*sigh*

At 9:43 am +0100 2/7/01, Williams, Derek wrote:
>No, it's very dangerous to make predictions like this and IMO doesn't
>help the cause. Even without human activities, natural things like big
>volcanoes can easily disrupt the climate in such a way as to swamp the
>signs of global warming and indeed produce severe weather conditions as
>a casual glance at met records for the past couple of hundred years
>quickly shows (frozen Thames etc)
>

>One example of how this could work: A big volcanoe errupting a massive
~~~~~~~~ hello Dan :-)
>amount of ash could change the albedo of the earth by enough to counter
>the warming effect of the increased CO2 (albedo - and sorry if the
>selling is wrong - measures the reflectivity of the earth, more smoke in
~~~~~~~ *grin*
>the atmoshere reflects more radiation back to space). Cutting the
>forrests down has the same effect, both because of the smoke from the
>burning trees and the resulting cleared ground, which is why on photos
>of building sites the bare earth looks white.
>
>Over simplification does no-one any good.

You're hardly any better, Derek: this is hardly a 'Nature' paper, is it?

You're talking about volcanic events that have a very different
duration than the warming effects we're talking about. Major
eruptions show up very clearly in the tree ring records going back
centuries, but that's because you can pick out a one-to-three year
spike rather than a prolonged cooling effect.

A rudimentary understanding of albedo is all very well, but since the
radiative heat input from the Sun is still poorly understood
(surprisingly) we can't deduce too much. In any case relying on mass
deforestation or a prolonged series of major volcanic eruptions is
hardly an attractive alternative to giving up burning what are finite
resources anyway.

Have a look at http://www.cru.uea.ac.uk/ - particularly
http://www.cru.uea.ac.uk:80/cru/info/warming/. We're looking at an
unprecedented acceleration in temperature, and it's not due to a
sudden lack of volvanic eruptions. Even if it turns out to be
naturally-occurring, who's willing to take that chance? We should be
trying to wean ourselves off of unsustainable energy generation and
use anyway.

Cheers

Harry
--
Ian Harris - "Harry" Telephone: +44 1603 593818
Climatic Research Unit Email: i.harris@uea.ac.uk
University of East Anglia
Norwich NR4 7TJ

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##########

From: Phil Jones <p.jones@uea.ac.uk>
To: Keith Briffa <k.briffa@uea.ac.uk>, "Dr. Reinhard Böhm" <r.boehm@zamg.ac.at>,
<maugeri@mailserver.unimi.it>, <t.nanni@isao.bo.cnr.it>, <m.brunetti@isao.
bo.cnr.it>,<Dietmar. Wagenbach@iup. uni-heidelberg.de>,
<jones@gkss.de>,<widmann@gkss.de>, <storch@gkss.de>
Subject: Re: ALPIPMOD-brainstorming
Date: Tue, 03 Jul 2001 15:01:17 +0100

Dear All,
Here are a few more comments on ALPIPMOD.
Ideas are probably not very well ordered. First, you should try for a 3 year
project
and second, although here for most of the next three months (apart from odd days) I
probably
couldn't justify a meeting. I am intending on resubmitting another proposal to the October
EU round. This one will involve some of the group from ADVICE. It's aim will be to develop
a daily MSLP dataset for Europe and the Atlantic (30-70N by 70W-50E). After the dataset
is produced in the first year, the second and third year will see various analyses
performed
and comparisons of several GCM runs performed at the Hadley Centre. This new project
will probably go to 2.4.1 which will be a different area from yours which will be 2.1.4.
Thus I
would hope that your proposal could be developed over email.
The above dataset would go back to 1850. This is the period which from the IMPROVE
project
is just beyond how far we think we can reliably go back with daily data. Several papers
from
the IMPROVE poject (Moberg et al., 2000 in JGR and several others in press in a special
issue
of Climatic Change) have come to the about 1870 date. We have much earlier data for the 8
sites but ensuring strict homogeneity of the daily series seems doubtful for some types of
extreme measures prior to about 1870. Pressure seems better than temperature. Some sites
are better than others. Monthly is fine for all.
All the IMPROVE and ADVICE data can be used by the ALPIPMOD project. I have a summer
student updating the 51 monthly MSLP sites from ADVICE, amongst other things.
As for your ideas, I think you need some overarching theme. The atlas and CD of all
the data may be one, but it also needs to address some scientific issues which can be
shown to have relevance to the public.
I like the idea of making use of the Alpine orography looking at changes in lapse
rates and
the use of high and low elevation air pressures. The latter is a totally independent
method of
looking at the warming and can be used back to the late 18th century. The Alps have the
longest records of any mountaineous records of any region of the world. Also I am a
strong
advocate of changes in the influence of features such as the NAO (and other circulation
indicators) on surface climate. You can clearly look at these changes over the last 200

years with all the data you have.
Another important issue to a lot of climatologists is the relative surface warming
compared
to the MSU2LT data in the lower troposphere. Although this is hemispheric in extent, we
can
look with the longer Alpine records as to changes in lower level lapse rates over 200+
years.
Related to this tropical ice caps are disappearing at alarming rates in Peru, Tanzania and
in Tibet (Lonnie Thompson's work). Lonnie has calculated that the ice cap on Kilimanjaro
will not be there by 2015 at its present rate of retreat. Lonnie has some local
temperature
series for about 40 years which show a small warming yet the ice caps are going fast. Why?
These ice caps have all been cored and have ice during the MWP times yet some aren't
producing layers now !
My idea is to use the better known histories of the Alpine glaciers to see if they
are
also melting at accelerated rates than simple temperature averages would imply. Keith
mentioned the forward modelling approaches to determine positions in the past (and then
relate these to moraine termini). Do these models still function in the last 20 years?
Lonnie
thinks a lot of the tropical melting is due to sublimation, which isn't accounted for by
the
degree day models. The elevational sunshine records may be important here and with
temperature a particular season may be much more important than the other three.
All the above is just ideas, but getting all the data together (instrumental and tree
ring
as well glacier termini and mass balance) allows us to be able to model the glaciers
better than
anywhere else. All Europeans will be interested in whether Alpine glaciers are going to
disappear and there will be clear impacts on biodiversity at the high elevations and
tourism.
Another impact area is on the use of glacier meltwater and runoff in hydropower
generation.
These are all good issues to use in the social and economic pages that need to be written.
Cheers
Phil
At 15:10 29/06/01 +0100, Keith Briffa wrote:

Hi everyone
I have been through the ideas and offer a few (aptly non organised) comments. First Phil
is away and will not be able to comment until later.
First, the project needs more explicit focus. The call will focused on natural
variability . We are offering a detailed analysis of the variability of climate in the

Alpine Region that focuses on CLIVAR timescales - basically very high
resolution and not
extending much beyond a few centuries. The project incorporates instrumental ,
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palaeodata . The inter-relationships between these will be studied to gain an
understanding of the nature and mechanisms of the climate variability - but is
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enough. I feel it needs to be linked with a strong element of understanding the
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etc.? I
merely put this out as a straw man . I feel the EC are putting a lot of
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As for the specific points in the brainstorming document -
The Dendro aspect :
I think it is essential to update the Alpine tree-ring chronologies that are
available .
This is because they are a proven asset but many questions regarding
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concerning the
changing responses of tree-growth to explored. The most efficient way to do
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involve several groups working in the Alps , (Thank you for sending the Thesis
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Giorgio Strumia which is certainly a very impressive piece of work) I would
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Wimmer's group and the Birmensdorf group would be ideal (Fritz Schweingruber
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but Jan Esper has joined them in his place - I can ask them to be involved but
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depends on what the group here think are the priorities and how much we see as
the
overall budget and institutional allocations). I should say here that I think
we would
require money for a single person who could , if it is agreed, work on aspects
of
tree-ring processing and relationships with climate in association with the
other
tree-ring groups, but also work with the climate and model data , especially
with a view
to exploring the statistical inter-relationships and dynamical associations
between the
different climate data sets. There is also the French tree-ring group at
Marseille?
Perhaps though not all need to partners - ALSO I am thinking of putting
together a
European Tree-ring project (or suggesting it as part of a large European
integrated
proxy study of Holocene variability) so if this happened there could be a link
between
it (involving some of the groups mentioned) and this proposal. The Swiss might
be
interested to produce selected site tree-ring density/updating which I think
would be

very valuable and I will speak to them without commitment as you ask.
As for some of the climate analysis possibilities mentioned, I very much like the ideas
of detailed ,local climate comparisons with the larger CRU (and CRUder!) data. We are
very interested in the association between time dependence in the relationships between
circulation changes and changes in Temp. and Prec. Also changes in the nature of climate
seasonality , and also extreme events (frost frequency , drought, intense rainfall). The
detailed analyses of these characteristics also compliments the interpretational work on
the tree-ring and glacier mass balance (and socio economic foci) data.
As for the glacier work - is not a huge effort already going into this? I think it is
important but does it fit as well ? The work proposed would have to be distinguished
from other ongoing efforts - though I do like the idea of linking the geomorphological
evidence of past glacier change (moraines , pro-glacial sediment data?) with
reconstructed glacier volume changes , where the reconstructions are based on new long
instrumental data , and palaeodata (temp. and precip.) used to drive a model of the
glacier volume. Our German (or Julie) colleagues can point to such work based on GCM
output . My colleague here (Sarah Raper) has also done this sort of work but using a
very simple model to estimate past Storglaciaren (in Sweden) volume changes and her
results imply that these models must be forward driven and not based on simple
regression analysis using temperature and precipitation to estimate past mass balance.
The future aspects of the discussion are important - and it is true that the previous EC
call dealt with modelling and scenarios of future changes. Here, I believe the use of
models should be strictly limited to understanding natural /current variability and
change. There is no benefit in going for a 2 year project - I strongly urge 3. I also
would find a meeting difficult. I am away from 17-29 July, and 11-25 August, and in
meetings during 7-10th July and 26-31st August.
Phil will be back here next week and will no doubt comment in more detail on the
instrumental analysis aspects then.
Very best wishes to all
Keith
At 05:13 PM 6/25/01 +0200, Dr. Reinhard Böhm wrote:

Friends,

As announced last Friday, we want to open a first round of brainstorming about the
contents of our project. We have collected what we have received from You so far and
have it mixed with our own ideas (file Brainstorming-1.doc). It does not have a nice
structure and there are still a number of question marks, as You will see.
Please add things where you think something is missing and please feel free to tell us

which points make no sense, or are too ambitious or simply too much work.
Please consider also the "how to do it" (state of the art methods, new approaches to
solve problems, other data than those mentioned, other topics.....).
Please try also to find Your position in the project, tell us what You would prefer to
do....
Please try to consider whether we would have to include other groups in terms of
scientific potential and/or in terms of data (For example: Keith Briffa you mentioned
Fritz Schweingruber as the leading data holder of Alpine tree-ring data. Do you think we
should ask him to join us, or could You use his data also without him being a contractor
of the project? In case You want him in the project could we kindly ask you to contact
him, being much more familiar with him and with the tree-ring topic than we are?)

We would be glad to receive a very short answer from everybody within this week, because
from June 30th to July 15th all the three of us will not be at the institute.
For more detailed considerations and answers You have more time, it would be nice to be
able to study them after our return by mid of July. But please use also the
possibilities to contact the other groups - the sooner we integrate to a group the
better it is.

Our time-table for the rest of the time until October:

July 16th to August 14th: We are at the institute, hoping to bring the project into a
near to final version what concerns the scientific content

August 15th to August 28th: Ice core conference at Kangerlussuaq (Greenland)

August 29th to September 17th: We are at the institute most of the time. We hope this
will be the time to elaborate the EU-shaped complete version.

September 18th to September 22nd: Big events going on in Vienna which may cut down our
time for the project (150th anniversary of our institute, Climate conference DACH-2001
(in German))

September 24th to October: Time reserved for all the things that could not be done yet
in spite of our time table

Could each of You please inform us about Your time table during summer and autumn?

A question to all of You: How do You think about one 2-days meeting this Summer or in
early September? What place do You prefer? If it is Austria we would have two low cost
possibilities: 1): at our institute and 2) (more adventurous): At the
Sonnblick-observatory (You do not have to have Alpinistic experience, we have a private

cable car going up)

Some remaining questions:

Should we try a 2-years or a 3-years project?

Can everybody live with roughly 300.000 Euro (This would result into somewhere between
1.5 and two millions, which we heard is a magnitude preferred by the commission). Please
consider not only the sum of money but also how to spend it and how to fill it with a
reasonable equivalent in work amount.

What is your feeling about the "Climate variability atlas of the Alps"? Is it good to
have one main deliverable like that or should we better produce a number of smaller
things?

One last technical remark: Please send your comments and mails not only to Vienna, but
also to the other groups (or at least to those You believe would be interested in what
You write). I do not think this would spoil too much our mail boxes and it has the
advantage to include the whole intellectual power of our group into the construction
phase of the proposal.

Looking forward to Your replies, ideas, time tables and anything else

With best regards

Reinhard

--
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239. 0994187098.txt
##

From: "Michael E. Mann" <mann@multiproxy.evsc.virginia.edu>
To: Hans von Storch <Hans.von.Storch@gkss.de>
Subject: Re: EOS report
Date: Tue, 03 Jul 2001 15:04:58 -0400
Cc: "Michael E. Mann" <mann@virginia.edu>, Julie Jones <jones@gkss.de>, Julia Cole <jcole@geo.arizona.edu>, rbradley@geo.umass.edu, jto@u.arizona.edu, weber@knmi.nl, wanner@giub.unibe.ch, tom crowley <tom@ocean.tamu.edu>, k.briffa@uea.ac.uk, Martin Widmann <Martin.Widmann@gkss.de>

<x-flowed>
HI Hans,

Yes--it was the discussion of this in the De Bilt meeting report that led me to think this was envisioned in a broadened version of the DATUN approach. I thought the idea was that you would eventually use a forward biological/physical model to scale up from a given proxy an estimate of say precipitation or temperature for an atmospheric model gridpoint and use that to nudge say the slp or 500 mb field into a particular configuration. This is clearly more ambitious than what you are doing now, and I suppose I was blurring the distinct efforts of Nanne and colleagues with that of yours and colleagues. I makes much more sense at present to only use a statistically-based upscaling of the proxy data. The other possibility remains intriguing, but we are certainly far off from doing that in my opinion as well. I'm actually quite relieved to find out that I was wrong in assuming that this is the direction the DATUN approach was going.

thanks for the clarification,

mike

At 08:32 PM 7/3/01 +0200, you wrote:
>Hi folks,
>"forward models" can only deal with "weather -> proxy", but we need "proxy
>-> circulation". If we had forward models, and we should certainly strive
>to develop such models, we could generate large data sets of consistent
>pairs "weather, proxy" and then derive empirically (neural nets?) the
>needed inverse relationship. (Actually, this method is used at our lab to
>evaluate the informational value of remotely sensed data about water
>quality in coastal seas.) But the inverse relationship is not
>process-based but necessarily phenomenological.
>
>I think the need for forward models was spelled out in the report about he
>De Bilt meeting in 1999 (see EOS paper by Weber and me).
>
>Regards
>
>Hans
>
>At 13:52 03.07.01 -0400, Michael E. Mann wrote:
>>Dear Julie et al,
>>
>>Then I apologize--I thought the idea in DATUN was to at leat eventually
>>incorporate physical or biologically-based models of proxies into the
>>upscaling effort in addition to/in place of statistical upscaling. There
>>was lots of discussion of this, and I recall Hans early on having
>>described to me plans to use physical models of proxies in the process
>>(though I could be mistaken), so I thought that was a planned component
>>of DATUN, and the work that you described (ie, using empirical CCA
>>techniques) was just a preliminary empirical approach. But from what
>>Martin and you have told me, this is not the case, and there is no plan
>>in DATUM to use physical/biological forward models of proxies. If someone
>>out there still believes this is *not* the case please let me know!

>>Otherwise, the wording will be clarified to indicate that it is a
>>"statistical" and not physical/biological model that is used to upscale
>>the proxy information.
>>
>>That simplifies things quite a bit...
>>
>>mike
>>
>>At 07:18 PM 7/3/01 +0200, Julie Jones wrote:
>>
>>>Hi Mike
>>>
>>>I'm getting very confused now!
>>>
>>>If you mean 'forward modelling', by what I term upscaling, this is done
>>>in exactly the same way as most other climate reconstructions,
>>>i.e. calibrating proxy data against climate data using linear multivariate
>>>statistical methods (in this case I use CCA), so has the same errors
>>>inherent in it as other reconstructions where proxy data has been
>>>calibrated against large-scale climate, or climate indices.
>>>
>>>If your idea is that such large-scale climate reconstructions may have
>>>additional uncertainties compared to local empirical models, where proxy
>>>data are calibrated against local climate records, I agree that this is
>>>so - but I think this applies to all such non-local reconstructions, so
>>>should maybe go in the paragraph which discusses reconstructions of
>>>regional climate variability to keep things consistent.
>>>
>>>The additional potential source of error specific to the DATUN method
>>>compared to the other climate reconstructions, whether local or
>>>large-scale, is in the 'nudging' to assimilate the climate reconstructions
>>>obtained as above into the GCM, which should probably go into the text, so
>>>we could perhaps change the end of the paragraph to read:
>>>
>>>......This method is more resistant to biases specific to
>>>purely empirical or model-based approaches but it is relatively untested
>>>using proxy data, and prone to additional uncertainties in the nudging
>>>method used to assimilate the proxy data.
>>>
>>>
>>>Am I on the right track, or have I missed something?
>>>
>>>
>>>cheers
>>>
>>>Julie
>>>
>>>************************************
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>>>phone: +49 (0)4152 871845
>>>fax: +49 (0)4152 871888
>>>************************************
>>>
>>>On Tue, 3 Jul 2001, Michael E. Mann wrote:
>>>
>>> > Dear All,

>>> >
>>> > I am working on preparing a final version of the workshop report based on
>>> > Julie (C)'s revisions, and comments thusfar recieved.
>>> >
>>> > There is one instance below in which it seems especially important
>>> that we
>>> > agree on the wording, so I wanted to give you my revised wording now and
>>> > let you comment on it if you see any problem:
>>> >
>>> > The third approach represents a hybrid of the first two; it
>>> prescribes the
>>> > dynamics of the system using model physics, but aims to reproduce the
>>> > historical climate evolution by "nudging" the model towards reconstructed
>>> > climate estimates. This method is more resistant to biases specific to
>>> > purely empirical or model-based approaches but it is relatively untested
>>> > using proxy data, and prone to additional uncertainties in the forward
>>> > models employed to describe proxy-climate relationships.
>>> >
>>> > I think the latter statement is important because the assumption in the
>>> > forward model is *not* the same assumption as in empirical
>>> reconstructions
>>> > (I take a slight issue w/ Julie J in this regard). The forward modeling
>>> > makes some universal assumptions regarding e.g. tree growth patterns. The
>>> > empirical calibration approach calibrates the individual trees against
>>> > local meteorological/climate records. It doesn't make any universal
>>> > assumptions, though the local calibration may be flawed! In other words,
>>> > we're not saying that one method is better than the other, but the
>>> > potential pitfalls are definitely different! I think this needs to be
>>> > expressed, hence my revised wording. Julie J should let me know if
>>> there is
>>> > a problem w/ this, since she and Julie C spent some time parsing the
>>> > wording on the paragraph in question.
>>> >
>>> > Thanks,
>>> >
>>> > mike
>>> >
>>> > At 07:43 PM 6/28/01 +0200, Julie Jones wrote:
>>> >
>>> > >Hi Julie
>>> > >
>>> > >Yes, that works, although if I could ask for one extra word -
>>> > >
>>> > >...but it is also limited by potential....
>>> > >
>>> > >cheers
>>> > >
>>> > >Julie
>>> > >
>>> > >
>>> > >************************************
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>>> > >************************************
>>> > >

>>> > >On Thu, 28 Jun 2001, Julia Cole wrote:
>>> > >
>>> > > > Hi Julie,
>>> > > >
>>> > > > First, sorry for the author oversight! I did not change that from
>>> > > > Mikes original, which did not have you on it, but he told me you
>>> > > > should be added.
>>> > > >
>>> > > > I like all your suggestions. I would alter the wording of the last
>>> > > > one a bit maybe, to use somewhat fewer words. Does this work? (68
>>> > > > words instead of 78). We are tight on space.
>>> > > >
>>> > > > The third approach represents a hybrid of the first two; it
>>> > > > prescribes the dynamics of the system using model physics, but aims
>>> > > > to reproduce the historical climate evolution by "nudging" the model
>>> > > > towards reconstructed climate estimates. This method is more
>>> > > > resistant to biases specific to purely empirical or model-based
>>> > > > approaches, but it is limited by potential instabilities in the
>>> > > > proxy-climate relationships and is relatively untested using proxy
>>> > > > data.
>>> > > >
>>> > > > cheers, Julie
>>> > > >
>>> > > >
>>> > > >
>>> > > > >Dear All,
>>> > > > >
>>> > > > >Thanks Julie and Mike for your work on the paper. I have just a few
>>> > > > >sentences where I suggest alterations.
>>> > > > >
>>> > > > >1. First paragraph:
>>> > > > >
>>> > > > >'State-of-the-art climate models are also being applied to
>>> analyze late
>>> > > > >Holocene climate sensitivity, upscale paleodata to large-scale
>>> > > > >reconstructions, and simulate proxies themselves'
>>> > > > >
>>> > > > >I suggest changing to
>>> > > > >
>>> > > > >'State-of-the-art climate models are also being applied to
>>> analyze late
>>> > > > >Holocene climate sensitivity, assimilate large-scale climate
>>> > > > >reconstructions from palaeodata, and simulate proxies themselves.'
>>> > > > >
>>> > > > >
>>> > > > >2. Paragraph2, last sentence:
>>> > > > >
>>> > > > >'....patterns of atmospheric circulation, just as meteorological
>>> > > > >information is assimilated into numerical weather forecasting
>>> models (von
>>> > > > >Storch et al. 2000).'
>>> > > > >
>>> > > > >I suggest changing to
>>> > > > >
>>> > > > >....patterns of atmospheric circulation, in a conceptually
>>> similar way to
>>> > > > >the assimilation of meteorological information into numerical
>>> weather
>>> > > > >forecasting models (Weber and von Storch 1999; von Storch et al.
>>> 2000)
>>> > > > >
>>> > > > >   - the Weber and von Storch reference is already in the
>>> reference

>>> > > list.
>>> > > > >
>>> > > > >3. Paragraph 3,
>>> > > > >
>>> > > > >'The third approach represents a hybrid of the first two; it
>>> prescribes
>>> > > > >the dynamical evolution of the system from climate physics but
>>> > > > >is "nudged" toward the observed climate by the proxy data. This
>>> method
>>> > > > >is more resistant to the biases specific to purely empirical or
>>> purely
>>> > > > >model-based approaches, but it is limited by potential instabilities
>>> > > > >in the proxy-climate relationships and by imperfections in the
>>> upscaling
>>> > > > >models, and it is relatively untested using proxy data.'
>>> > > > >
>>> > > > >I would suggest changing to the following (As the upscaling
>>> models are
>>> > > > >produced in exactly the same way as other
>>> > > > >climate reconstructions, so there are no extra imperfections in the
>>> > > > >upscaling models than in other climate reconstructions).
>>> > > > >
>>> > > > >'The third approach represents a hybrid of the first two; it
>>> prescribes
>>> > > > >the dynamics of the system using model physics, but is aimed
>>> > > > >at reproducing the historical climate evolution by "nudging" the
>>> model
>>> > > > >states towards towards climate estimates obtained by the first
>>> > > > >approach. Although this approach also requires the stability
>>> > > > >assumption in the statistical models, it is hoped that it is more
>>> > > > >resistant to the biases specific to purely empirical or purely
>>> model-based
>>> > > > >approaches; it is however relatively untested.'
>>> > > > >
>>> > > > >
>>> > > > >Finally, I've been missed off the list of authors! - and the
>>> address for
>>> > > > >Hans and myself should be GKSS Research Centre, Geesthacht.
>>> > > > >
>>> > > > >Best regards
>>> > > > >
>>> > > > >Julie
>>> > > > >
>>> > > > >
>>> > > > >
>>> > > > >************************************
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>>> > > >
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>>> > > > __________________________________
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##########

From: Jean-Charles HOURCADE <hourcade@centre-cired.fr>
To: roger.harrabin@bbc.co.uk, klaus.hasselmann@dkrz.de,
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ccarraro@unive.it, tol@dkrz.de
Subject: No Subject
Date: Wed, 11 Jul 2001 09:58:13 +0200

Dear Friends,

A few remarks before the meeting of tonight and tomorrow, I am sure that our meeting will make clearer the different objectives of ECF, in particular regarding the articulation between the scientific agenda and activities in direction to stakeholders and policy-makers.

I would like to stress that I will attend the ECF meeting not only in the name of the Cired, but also in view of preparing the involvment of the Institut Laplace in ECF, namely the community of climate modellers, with which we develop a long term research program. I would like to explain hereafter in a few words what should be, in my view the priorities of ECF, in terms of scientific agenda:

Given recent Ipcc experience, the first priority would be to progress in direction to integrated models. Indeed the lessons of the Ipcc are twofold:

- first the Sress scenarios confirm the possibility of generating very different emissions growth scenarios over the long run, but the consistency between the Storylines and the numerical scenarios remain uncertain; this uncertainty and vagueness reveals a more fundamental limitation of the state of the art of economic modelling over the long run, in particular to provide an explicit picture of linkages between structural changes (infrastructure transportation, urban forms that govern the energy content of final consumption, industrial structure and the so-called dematerialisation), innovation and both macro and micro economic drivers (productivity, growth and price-signals). This makes very difficult to detect where are the real bifurcations, the real policy-parameters and to make much progress in the understanding of the timing of policy responses,
- second the sections on 'damages ' have make some progress but remain weak in terms of the social and economic implications. More precisely they deal mostly with impacts on physical parameters (sea-level rise), in a few cases adress impacts on humans (tropical diseases), but all this does not give a comprehensive picture of social and economic damages (once discounted the effect of adapatation),

One of the scientific objective of ECF should be to be prepared to provide in a few years for a convincing contribution in future exercises like the SRES and in the future Ipcc rounds. This passes first through two parallel efforts:

- on long term economic modelling where the limitations of existing tools are obvious depiste real progress; this relates basically to three challenges:
    - a macroeconomic framework insuring the consistency between prices and quantities at any point in time without necessarily resorting to the modelling tricks relying on the conventional neo-classical growth theory; these 'tricks' assume indeed perfect foresight, efficient markets and the absence of strategic or routine behaviours; New conceptual frameworks about endogenous growth theory allow for such a move, but there is a gap between advances in pure theory and empirical modelling,
    - the endogeneisation of technical change and more precisely to develop this endogeneisation in such a way that the information coming from sectoral models in energy, transportation or agriculture is not lost (this comes back to the bottom-up/top-down controversy); note that one key challenge here is to progress in direction to transportation and agriculture
    - an explicit treatment of expectations and uncertainty; one key issue indeed is that the stabilisation of expectations over the long run is the main driver of technical change, consumption patterns and structural adaptation.

- on 'coupling' economic and climate models: here there are two routes, either to develop coupling methods between large-scale models or to develop interface compact modules, reduced forms of large scale models. Both routes are valid, however, in the following years, to develop integrated models made up with reduced forms of larger models seems more promising; thanks to tractable and numerically controlable models, in will be easier to reveal the key mechanisms at work and to introduce uncertainties. This will pass through progress in the representation of carbon cycle (including sequestration) in such models and, more importantly in the representation of damages and adaptation, which rises rather fundamental conceptual issues that explain what seems to be the second priority in my view.

The second prority relates to the joint question of damages and precautionary principle:
- part of the agenda is covered by Mike Hulme's paper and I will not elaborate here on other dimensions I would link to include and how to assess a cost. I will simply insist of the fact that we need to set up a taxonomy of damages in economic terms, this means as resulting not of the climate transformation per se but from the joint effect of inertia and uncertainty (to pass to Riviera to the beaches of Normandy in not a cost in itself in a world restabilized around a new climate equilibrium; what matter are the transition costs and the generated variability of climate). Moreover I would insist for adopting deliberately a worldview because, fundamentally, climate change will generate a new human geography, and not to be restricted to the European subcontinent,
- this should lead to develop in parallel stochastic decision modelling tools to disentangle the many dimensions and views about the precautionary principle and, I take some risks in saying that, in a symmetric treatment of climate damages and nuclear risks (we cannot avoid to try and put some rationale in this discussion which is one of the reason for the failure of the EU tax in 1992 and of COP6, and which will be an 'hidden' division line within the EU)

The third priority should be the topic 1 made by Klaus. For me the two first modelling efforts I described briefly are outmostly important to bring new insights for responding the question of the instruments. However, we have, before waiting for the acheivement of a new generation of models (which will respond to point 2 and 3 of Klaus's paper), it matters to develop in parallel a specific programm on international coordination architecture given the failure of COP6 and the lack of understanding of economic and social implications of the selection of this architecture (coordination through prices or quantities, full agreement or partial expanding coalition, issue linkages, perceived equity etc ....). This workprogramm should build on advances on the role of economic and non economic instruments in fostering innovation, and on the distributive static and dynamic implications of such instruments.

These are very brief remarks, simply to give you some ideas about my current perspectives.

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From: Edward Cook <drdendro@ldeo.columbia.edu>
To: Tim Osborn <t.osborn@uea.ac.uk>
Subject: Re: N(eff) and practicality
Date: Tue, 24 Jul 2001 08:49:14 -0400
Cc: Phil Jones <p.jones@uea.ac.uk>, Keith Briffa <k.briffa@uea.ac.uk>

Hi Tim,

Thanks for the remarks. We can certainly spend some time talking through some of the points raised. I guess I am still finding it difficult to believe that an rbar of 0.05 has any operational significance in estimating Neff. It is kind of like doing correlations between tree rings and climate:

a correlation of 0.10 may be statistically significant, but have no practical value at all for reconstruction. The same goes for an rbar of 0.05 in my mind. I agree that what I suggested (i.e. testing the individual correlations for significance and only using those above the some significance level for estimating rbar) is somewhat ad hoc and not theoretically pleasing. However, it is also true that correlations below the chosen significance threshold are "not significantly different from zero" and could be ignored in principle, just as we would do in testing variables for entry into a regression model. This would clearly muddy (a nice choice of words!) the rbar waters, I admit.

In terms of the problem I am working on (computing bootstrap confidence limits on annual values of 1205 RCS-detrended tree-ring series from 14 sites), it is hard to know what to do. Certainly, using Neff will result in almost none of the annual means being statistically significant over the past 1200 years. I don't believe that this is "true". Other highly conservative methods of testing significance result in a very high frequency of similarly negative results, i.e. the test of significance in spectral analysis that takes into account the multiplicity effect of testing all frequencies in an a posteriori way (see Mitchell et al. 1966, Climatic Change, pg. 41). If you use this correction, virtually no "significant" band-limited signals will ever be identified in paleoclimatological spectra. So, this test has very low statistical power. I think that this is the crux issue: Type-1 vs. Type-2 error in statistical hypothesis testing. The Neff correction greatly increases the probability of Type-2 error, while virtually eliminating Type-1 error. So, truth or dare.

Consider one last "thought experiment". Suppose you came to Earth from another planet to study its climate. You put out 1,000 randomly distributed recording thermometers and measure daily temperatures for 1 Earth year. You then pick up the thermometers and return to your planet where you estimate the mean annual temperature of the Earth for that one year. How many degrees of freedom do you have? Presumably, 999. Now, suppose that you leave those same recording thermometers in place for 20 years and calculate 20 annual means. From these 20-year records, you also calculate an rbar of 0.10. How many degrees of freedom per year do you have now? 999 or 9.9? What has changed? Certainly not the observation network. Does this mean that we can just as accurately measure the Earth's mean annual temperature with only 10 randomly placed thermometers if they provide temperature records with an rbar of 0.00 over a 20 year period? I wouldn't bet on it, but your theory implies it to be so. Surely, one would have more confidence (i.e. smaller confidence intervals) in mean annual tempertures estimated from a 1000-station network.

Cheers,

Ed

>Ed,
>
>re. your recent questions about Neff and rbar etc...
>
>I've thought a bit about these kind of questions over the past few years,
>but have never completely got my head around it all in a satisfactory way.
>I agree with what Phil said in his reply to you. Also, your idea of
>subsamping 40% of the cores at a time sounds reasonable, though I don't
>think it would be possible to write a very elegant statistical
>justification! Anyway, I just wanted to add a couple of points to what
>Phil said:
>
>(1) Even for very low rbar, the formula certainly works for
>idealised/synthetic cases (i.e. with similar standard deviations and

>inter-series correlations etc.). For example, I just generated 1000 random
>time series (each 500 elements long) with a very weak common signal,
>resulting in rbar=0.047. n=1000 was the closest I could get to n=infinity
>without waiting for ages for the correlation matrix to be computed! The
>formula:
>
>neff = n / (1 + [n-1]rbar)
>
>which reduces to neff = 1 / rbar for n=infinity gives neff = 20.83. For
>such a low rbar, neff seems rather few? The mean of the variances of the
>1000 series was 1.04677. If I took the "global-mean" timeseries (i.e. the
>mean of the 1000 series, then it's variance was 0.05041. The ratio of
>these variances is 20.77 - almost the same as neff! If our expectation
>that neff should be higher than 20.83 was true, then the variance of the
>mean series should have been much lower than it was. It should be easy to
>try out similar synthetic tests with various options (e.g. shorter time
>series, sets of series with differing variances, subsets with higher common
>signal (within-site) combined with subsets with weaker common signal
>(distant sites) etc.) to test the formula further.
>
>(2) I agree that rbar is computed from sample correlations rather than true
>(population) correlations.
>(a) For short overlaps, the individual correlations will rarely be
>significant. But the true correlations could be higher as well as lower,
>so rbar could be an underestimate and neff could be an overestimate! Maybe
>you have even fewer than 20 degrees of freedom!
>(b) I did wonder whether the sample rbar might be a biased estimate of the
>population rbar, given that the uncertainty ranges surrounding individual
>correlations are asymmetric (with a wider range on the lower side than the
>higher side). But I've checked this out with synthetic data and the rbar
>computed from short samples is uncertain but not biased.
>(c) Just because rbar is only 0.05 does not mean that you need series 1500
>elements long to be significant - that would be the case for testing a
>single correlation coefficient. But rbar is the mean of many coefficients
>(not all independent though!) so it is much easier to obtain significance.
>Not sure how you'd test for this theoretically, but a Monte Carlo test
>would work, given some assumptions about the core data. For 100 cores,
>each just 20 years long, a quick Monte Carlo test indicates that an rbar of
>0.05 is indeed significant - therefore rbar=0.05 in your case with > 100
>cores, many of which will be > 20 years long, should certainly be significant.
>
>Looking forward to your visit! We can discuss this some more.
>
>Tim
>
>
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##########

From: "Stephan Singer" <SSinger@wwfepo.org>
To: <bill.hare@ams.greenpeace.org>,<baldur.eliasson@ch.abb.com>, <klaus.hasselmann@dkrz.de>, <tol@dkrz.de>, <ccarraro@helios.unive.it>, <gretz@mail1.tread.net>, <hourcade@msh-paris.fr>, <GBerz@munichre.com>, <ola.johannessen@nrsc.no>, <Carlo.Jaeger@pik-potsdam.de>, <Martin.Welp@pik-potsdam.de>, <Ottmar.Edenhofer@pik-potsdam.de>, <schellnhuber@pik-potsdam.de>, <juergen.engelhard@rheinbraun.de>, <m.hulme@uea.ac.uk>, <ccarraro@unive.it>
Subject: response
Date: Fri, 17 Aug 2001 15:56:33 +0200

Dear Mr Hasselmann,

thanks for the draft position of the ECF. I do believe it is very good first approach to position the needs of a science-based climate policy in the future. I do particularly like the quasi-goal of a long-term 0-emission target supported by the scientific community.
However, there are a few amendments I like to propose:

a) I do not agree at all that the focus on the short term "dictared by the 10 year Kyoto horizon has tended to obscure longer term issues".

In the contrary, if we were to agree on longer-term and deeper targets - what we all want I suppose - there must be a starting point somewhere in the next years. I do agree that the 1 CP targets are moderate and will be diluted by all kinds of loopholes. But given the economic and political nature of this treaty, more is/was not reachable by the international community. I prefer an unperfect agreement coevering the globe (almost!) as a starter over an perfect agreement that will never be agreed upon.
And - probably more important - the recent Bonn agreement will give the signal to the main polluters that the atmosphere is not a free sewer any more. At best, they won some time - but the ultimate message is, that the train towards deeper targets has started. This may impact future industrial investment and legislative decision making much deeper than the targets of the 1 CP itself as it provides some basic certainty.
Having said this, the next important discussion round on a political level will resume about "adequacy of commitments" of the next CPs. that is the build-in logic of both the treaty and the Convention. Here countries will start to address targets for 2013-2018. Thus, there is an approach to the long-term issues. It is a transient process over time. And, please believe me, almost everyone I talked to in the past who complained about the "short-term" focus of the treaty as opposed to a long-term global strategy had not in mind to strengthen environmental effectiveness - these voices mostly reflected the desire to fully delay any early action after all. And without early action and without short term focus we will never get to the longer-term targets.
In short, I believe, a scientific approach should foster the architecture of the KP and that of the Convention and the need for further target-setting processes in the future by all parties - and that is intrinsincally embedded in the process.
In that respect, it is probably scientifically correct to state that the "Kyoto reductions have negligible impacts on global warming" but it would be politically naive to conclude that this means Kyoto is only "symbolic". It is much more.

b) I have problems with the focus on solar as the sole beneficiary of a 0-emission society. Still, I still like to focus on those measures that are not implemented yet and can provide the bulk of future emissions reductions mostly cost-effectively - that is energy efficiency in its various forms and various applications. And renewables are those who benefit most from energy efficiency as each renewable kWh provides more service, km or goods.

Generally, I like a broader approach to renewables. It is not "one takes it all" solar what will save the world from climate change. We need many forms of renewables according to the cultural, political and economical circumstances in the various regions. In some it may be solar thermal power or PV, in others it is off-shore wind, and in many rural areas it may be biomass or geo-thermal energy. And let us not forget the challenge of producing hydrogen from renewable sources as another ultimate fuel.

c) How dow we deal with equity? I believe it has to be addressed in one way or the other - and I mean much more than the usual GHG emissions per capita approach. This would include compensation/adaptation funding for poor and vulnerable developing countries - but also how to deal with targets for (certain) developing countries in the next CPs.

best regards
Stephan Singer
WWF International

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##

From: Klaus Hasselmann <klaus.hasselmann@dkrz.de>
To: "Stephan Singer" <SSinger@wwfepo.org>, <bill.hare@ams.greenpeace.org>, <baldur.eliasson@ch.abb.com>, <tol@dkrz.de>, <ccarraro@helios.unive.it>, <gretz@mail1.tread.net>, <hourcade@msh-paris.fr>, <GBerz@munichre.com>, <ola.johannessen@nrsc.no>, <Carlo.Jaeger@pik-potsdam.de>, <Martin.Welp@pik-potsdam.de>, <Ottmar.Edenhofer@pik-potsdam.de>, <schellnhuber@pik-potsdam.de>, <juergen.engelhard@rheinbraun.de>, <m.hulme@uea.ac.uk>, <ccarraro@unive.it>
Subject: Re: response to response
Date: Sat, 18 Aug 2001 13:39:00 +0200

<x-flowed>

>Dear Stephan (I suggest we use the anglo-saxon first-name form, coupled
>with "Sie" if we slip into German)

I agree with all of your points and hope you will contribute to finding the right language in our position paper to reflect both the need for long-term goals and the value of at least starting off with something one can build upon. One of my motives was to help keep the door open for those who wish to join the process later without too much embarassment. I also agree that we need to investigate all technological options. I am certainly not an expert in this field and am willing to learn from those who see more Global Mitigation Potential in some of the currently proposed technologies than I do.
  With best regards
Klaus

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##########

From: Mike Hulme <m.hulme@uea.ac.uk>
To: "Matilda Lee" <matildalee1@hotmail.com>
Subject: Re: Request from The Ecologist magazine
Date: Tue Aug 21 09:41:10 2001

See comments embedded from me below .... I would appreciate receiving a copy of the
magazine when published. Thank you.
My affiliation is provided below.
Mike
At 15:15 14/08/01 +0000, you wrote:

Yes-very much so! Your response would be greatly appreciated. Thanks!

From: Mike Hulme <m.hulme@uea.ac.uk>
To: "Matilda Lee" <matildalee1@hotmail.com>
Subject: Re: Request from The Ecologist magazine
Date: Tue, 14 Aug 2001 16:08:55 +0100
Been away on holiday - is this still relevant?
Mike
At 10:10 03/08/01 +0000, you wrote:

Dear Sirs:
The Ecologist, a London-based internationally recognized environmental magazine, will be publishing a Special Edition on Climate Change in September. For this edition, we believe it would be extremely useful to gather the opinions of the top climatologists on an issue for which there is growing interest by those concerned with climate change.
This issue is addressed in Article II of the United Nations Framework Convention on Climate Change, which states:
"The ultimate objective of this Convention and any related legal instruments that the Conference of the Parties may adopt is to achieve, in accordance with the relevant provisions of the Convention, stabilization of greenhouse gas concentrations in the atmosphere at a level that would prevent dangerous anthropogenic interference with the climate system. Such a level should be achieved within a time-frame sufficient to allow ecosystems of adapt naturally to climate change, to ensure that food production is not threatened and to enable economic development to proceed in a sustainable manner."
Furthermore, the need to address the issue of atmospheric concentrations was recently reaffirmed by Michael Zammit Cutajar, Executive Secretary of the UNFCCC, who stated at the closing session of the IGBP in Amsterdam on 13 July 2001,
"I believe that the political process on climate change would be greatly assisted by agreement on a target for atmospheric concentrations, at least an intermediate target. This would give a sense of where the whole international community should be heading and a basis for apportioning responsibility for getting there."
We would be very appreciative if you would send a return email with your response to the following questions for publication in The Ecologist

Special Edition on Climate Change.
-At what levels do you think we should aim to stabilize carbon dioxide
concentrations in the atmosphere and why?

I do not believe we have any sure basis for establishing what a 'non-dangerous' level
should be. This is so for several reasons:
- what is 'dangerous' depends on what measures are taken to adapt to climate change.
550ppm may be 'safe' in one assumed future world but 'dangerous' in another.
- the concept of 'danger' is not one that science can pronounce on. Such a level has to be
negotiated via a social and political process. This negotiation has also to take place in
the context of other risks that society is exposed to, i.e., we may be prepared to run a
higher risk with climate change if it means we can divert greater resources to reducing
global poverty.
- the basis for establishing 'danger' is contested. One could argue that 'dangerous'
climate change is change in climate that leads to the death of just *one* person; or argue
that some benefit/cost ratio should be used; or argue that if a sovereign state is
extinguished (e.g. a Pacific atoll nation) then that is the definition of 'dangerous'.
Thus you can see that I do not believe we can arbitrarily choose 550ppm or 650ppm, as done
by many scientific pronouncements (including the IPCC and others), and claim that is our
target. This can only be done by using the instruments of social and political discourse
on an international scale.
What we can say is that the higher the concentration of CO2 reached the greater the likely
risks associated with that concentration will be. But this is a relative argument, not an
absolute one.

-What does that level equate to in terms of percentages of emissions
reductions and by what date should we aim to reach that level?

So you see this second question I cannot answer. What we need to be doing, while we debate
the first question, is to put in place measures/mechanisms/processes that will now, and in
the future, give us greater flexibility of choice about different energy systems that have
different carbon ratings. The process is more important that the targets, as the Kyoto
negotiations have amply demonstrated.
In 10 years times, what we regard as 'dangerous' climate change will be very different from
today - and different again in the year 2020. We therefore need an emissions reduction
strategy that is flexible and reflexive to the changing demands of society.

We are aware that there is currently no consensus within the scientific
community on what an appropriate level for atmospheric concentrations is.

Indeed not - and there never can be. This question is not appropriately answered by

science - it has to be answered by society! This is a very important point to
get across.

Our aim in this endeavour is to share with our readers the values
considered relevant to this debate to illustrate why a consensus is
difficult to achieve.

Exactly so - and in the end it is a matter of risk assessment and risk
management. And
with most matters of risk, it is the perception by different individuals that
matters more
than any quasi-objective estimate of risk. Temperamentally I take more risks
than does my
wife - my concept of dangerous climate change is likely therefore to be quite
different
from hers. Writ large and across the nations of the world, this is the problem
of climate
change management.

Thank you in advance for your consideration.
Sincerely,
Matilda Lee
The Ecologist

____

____

Get your FREE download of MSN Explorer at [2]http://explorer.msn.com/intl.asp

References

1. http://explorer.msn.com/intl.asp
2. http://explorer.msn.com/intl.asp

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##

From: Rob Swart <Rob.Swart@rivm.nl>
To: wigley@ucar.edu
Subject: Re: TGCIA scenario recommendations
Date: Mon, 27 Aug 2001 11:39:11 +0200
Cc: m.hulme@uea.ac.uk, parryml@aol.com, Rob Swart <Rob.Swart@rivm.nl>, steve smith
<ssmith@pnl.gov>, s.raper@uea.ac.uk, Tsuneyuki MORITA <t-morita@nies.go.jp>,
tim.carter@fmi.fi

Dear Tom,

Thanks for your message and papers. The problem is clearly one of the
science-policy interface. If science cannot demonstrate that it makes a
difference in terms of avoided climate change and impacts if GHG
concentrations are stabilised, why bother? Currently a Danish guy, Björn
Lomborg, is making the headlines again (Guardian, New York Times,
Economist), TV programmes, etc.) telling the public (and policymakers) not
only that there aren;'t any environmental problems, but also, even if
climate change may be real, it does not make any sense at all to do
something about it, since efforts to control GHG emissions are expensive
and the mitigation would not make any difference at all anyway in terms of
avoiding negative consequences. Very popular message. Now clearly,
scientists should clearly explain what they can say about this issue. My

expectation would indeed be that comparing climate changes resulting from reference cases and from stabilization cases would not be distinguishable until well into the 2nd half of the century (like in the GRL paper), but if this is so, so be it. 2050 seems a lot closer now in 2001 (2050 is THIS century and our childrens' lifetime) than it was in 1999 (when 2050 was something of the next century and some abstract next generations). It is a matter of communication skills to get the message across about the long timescales and inertia of the systems involved, and the difficulty of identifying the climate change signal in the noise of natural variability. I would be curious what your opinion is about the UK work of Nigel Arnell, Martin Parry, John Mitchell and others, analysing the (significant) avoided impacts of 550 stabilisation from an IS98a reference. Another strategy of concerned scientists may be not to do these analyses at all in order to avoid a possible result that the differences between reference and stabilisation can not be demonstrated in a scientifically credible and unambiguous way and hence climate policy action may be obstructed. To me, this does not seem to be the honest way to go.

I am not sure what this all implies for the planned recommended stabilization runs. Your points about the climate sensitivity and non-CO2 gases are well taken. I am not sure the sulfur emissions in the proposed post-SRES scenarios would make a lot of difference, since already in the SRES base cases sulfur emissions are pretty low, and these would only be slightly different (usually lower) in the stabilisation cases. You suggest "carefully constructed idealized scenarios". Do you mean carefully constructed from the climate system point of view in order to get "distinguishable results", or carefully constructed from the socio-economic point of view so as to analyse real-world consistent and plausible futures (the latter is what Morita's exercise tried to achieve)? My answer would be: both.

I'd like to reflect a little bit more on this and since I am a scenario expert rather than a climate expert, await reactions from people more expert in the area of climate modelling, like Sarah, Mike and Tim, and Martin himself as chair of the TGCIA.

Thanks again,

Rob

Tom Wigley

<wigley@ucar.	To:	Rob Swart <Rob.Swart@rivm.nl>

edu>	cc:	parryml@aol.com, tim.carter@fmi.fi,
m.hulme@uea.ac.uk,
s.raper@uea.ac.uk, Tsuneyuki MORITA
<t-morita@nies.go.jp>, steve smith
25-08-01	<ssmith@pnl.gov>, (bcc: Rob Swart/RIVM/NL)

01:47	Subject:	Re: TGCIA scenario
recommendations
Please

respond to

wigley

Rob and others,

The key thing with doing stabilization runs with AOGCMs is (as Rob says) that the different cases "would have to be distinguishable from one another". This is the crux of the problem (in fact, it is a non-trivial problem even to define what is meant by "distinguishable from one another").

A few years ago we decided to try to do some matched no-climate-policy and (550ppm) stabilization runs where the two scenarios had some semblance of realism. (It turns out that the only similar work is that done by the Hadley Ctr, but the scenarios they used are highly idealized.) Our runs were also idealized in that we only changed CO2 -- in the best scientific tradition of changing only one thing at a time to assess sensitivities. The first results of our exercise (using CSM) are in Dai et al., J. Climate 14, 485-519, 2000. A number of things were clear from this. First, one cannot tell much from single realizations of the two cases -- ensemble runs are essential. Second, as we already knew from running simple models, the no-policy and stabilization runs diverge only slowly. Even after 50 years, the two are only just distinguishable at the global-mean level; so, clearly,differences at the regional level (especially for precipitation) would not be detectible above the noise of natural variability.

So our next step was to do ensembles of 5, this time using PCM instead of CSM (this paper is in press in BAMS -- for a pdf preprint, look at www.cgd.ucar.edu/cas/adai/). Even then, for ensemble means, the separation between the no-policy and stabilization cases is slow. So I devised an extended no-policy case out to 2200 (50 years beyond where the CO2 level stabilizes in the stabilization run), and we extended some of the runs out to 2200. This work is in press in GRL (and downloadable from the above site). Additional important results come from these experiments. One important result is that, even for precipitation, the *patterns* of change are not detectibly different between the no-policy and stabilization runs. A second important result is that, for most of the world the intra-ensemble differences are similar to or greater than the underlying signals of change. Distinguishing the no-policy and stabilization runs therefore presents a much greater challenge than any of you probably realize.

There are two issues to keep in mind, however. The first is that PCM and CSM have quite low climate sensitivities. So, will things be different if one used a more sensitive model? I suspect not in any major way. The reason is because inter-annual variability tends to be higher in more sensitive models, so the signal-to-noise ratio may not change much. This also applies to the intra-ensemble noise, since the root cause of these intra-ensemble differences is the internal variability of the model.

The second issue is that we have only changed CO2 in our experiments. We know that attempts to stabilize CO2 via emissions reductions also affect SO2 emissions -- so perhaps the no-policy and stabilization cases might be more distinguishable if one accounted for these concomitant SO2 effects? I have addressed this issue at the global-mean level in a paper on stabilization that I will attach to this email. (A more extensive analysis is in another paper, with Steve Smith as my co-author, that I

am not ready to share with anyone just yet.) My judgment, as someone
with quite a lot of experience in this area, is that having full spatial
details will not make the problem any easier; since, as the spatial
scale is reduced so the noise increases.

My recommendation from all this is that, first, you read the attached
paper (and I would welcome feedback on this) and the three
above-mentioned Dai et al. paper. Then, you might want to re-consider
what your strategy should be. In my view, I do not think we as a
community are at the stage where we can blindly develop paired no-policy
and stabilization scenarios and simply feed them into AOGCMs to see the
consequences. I believe that carefully constructed idealized scenarios
(perhaps based on what Morita is doing) will provide much more useful
information. You are already probably well aware of the need to do
ensemble runs, and I don't need to remind you how computationally
expensive this can be.

I hope these comments, and the papers, are useful. I'm sorry that it is
impossible for me to come to the Barbados meeting, but I am willing to
help in any way that I can.

Best wishes (and good luck), Tom.
****

Rob Swart wrote:
>
> Dear Sarah, Tom, Tsuneyuki, Martin, Mike and Tim,
>
> Back from holidays I found your email exchange. Let me first apologize
that
> I did not inform Sarah about this TGCIA action. I remembered from the
> IPCC-TGCIA meeting ? apparently wrongly - that Mike and/or Tim would
inform
> Sarah, as they would be in touch with her anyway (I did not even have
> Sarah's email address at the time). Let me also reiterate the reason for
> Tsuneyuki's invited proposal. In order to have comparable GCM results
> available and impact studies based on these results at the time of the
IPCC
> Fourth Assessment Report, and taking into account that GCM teams are
> unlikely to perform dozens of runs, the IPCC-TGCIA (chaired by Martin)
> intends to recommend a limited set of both baseline and stabilization
> scenarios for such runs. In this way, impact modellers in the coming
years
> could base their analysis on different runs from different GCMs for the
> same socio-economic scenario(s). Evidently, teams are free to run
whatever
> scenario they think interesting, but comparability would be preferable,
and
> many teams have proven responsive to IPCC-TGCIA recommendations in the
past
> as I understand it.
>
> The TGCIA has reached agreement on which 4 of the 40 SRES baseline
> scenarios would be most interesting (see meeting report: 4 scenarios
(A1FI,
> A2, B1 and B2) for 3 time periods 2020s, 2050s and 2080s). The next
> question was: since a (maybe "the") core policy question is what the
> benefits (or avoided impacts) would be of stabilizing GHG concentrations
at
> various levels, and since impact analysis should be based directly on GCM
> results rather than on results from simple climate models/IA models, it
> would be useful to also recommend a limited set of stabilization cases.
To

> make this a sensible effort, all the cases would have to be
distinguishable
> from one another from a GCM viewpoint. This may allow for combining
various
> scenarios which may be very different socio-economically, but would give
> very similar climate results for this century, such as the B1 and 550,
and
> the 650 and B2 cases. The stabilization cases would be selected from the
> following table, of which the cells contain available (post-SRES)
scenario
> runs:

	450 ppm	550 ppm	650 ppm	750 ppm
A1T				
A1B				
A1FI				
A2				
B1				
B2				

>
> It was suggested to select 2-4 cases from the more than 70 scenarios runs
> in the post-SRES programme co-ordinated by Tsuneyuki. Tom, it may well be
> that your "post-WRE" work serves the same purpose, but the rationale for
> selecting post-SRES cases would be: consistency with the SRES narratives
> and numbers of the IPCC, and the much-acclaimed multi-model
characteristics
> of the (post-)SRES work. To downsize the 70-odd cases to 2-4 cases and
not
> burden Sarah too much, it was suggested to have one model (MAGICC) run a
> subset of some 10-15 cases which seemed to make sense. Please also note
> that not all 70-odd cases are useable, either because they do not have
all
> relevant GHG gases, or there have been questions about the
> consistency/quality of their assumptions, e.g. a correct simulation of
the
> SRES base case by teams participating in post-SRES but not in SRES
(right,
> Tsuneyuki?). More importantly, Tsuneyuki used his intimate knowledge of
all
> cases and their distribution over base cases and stabilization levels to
> recommend 13 cases. This selection was discussed with me and Naki during
a
> brief meeting in Washington in June and seemed to be a very appropriate
> one.
>
> I noted the remark by Sarah that mean climate change results would be
> rather be model-independent (for a given climate sensitivity), while
> Tsuneyuki notes the large differences in the post-SRES work. These
> differences may not have to do with different approaches with respect to
> the carbon cycle or radiative forcing calculations, but rather with the
> freedom modellers had (or rather: took) in selecting the time path
(beyond
> 2100) towards stabilization/time horizon, and the changes in emissions of
> non-CO2 GHG in the stabilization analyses which focused primarily on CO2
> stabilization. This would need to be clarified in detail for the runs to
be

> selected, and I suggest that only those runs are further used for which the
> authors provide sufficient information on these issues.
>
> Concluding, I would like to ask Sarah, if she would be willing to take the
material provided by Tsuneyuki and perform the required calculations for
> the 13 cases (radiative forcing, global mean temperature and sea level
> rise, right, Mike/Tim?) within the next 1-2 months. The results would be
> discussed electronically in a small group (the addressees of this message)
> in October/November and a preliminary proposal based on these discussions
> would be the input for a discussion on this issue during the next TGCIA
> meeting in Barbados, in November. Tom's recent work may be useful for this
discussion as well, and I wonder if the mentioned (draft) papers could be
> distributed to this group or even the full TGCIA.
>
> Kind regards,
>
> Rob
>
> Dr. Rob Swart
> Head, Technical Support Unit
> Intergovernmental Panel on Climate Change Working Group III: Mitigation
> P.O. Box 1
> 3720 BA Bilthoven
> Netherlands
> tel. 31-30-2743026
> fax. 31-30-2744464
> email: rob.swart@rivm.nl
Attachment Converted: "c:\eudora\attach\ASPEN11.DOC"

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Attachment Converted: "c:\eudora\attach\9-550TEM1.XLS"

246. 0999293834.txt
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From: Mike Hulme <m.hulme@uea.ac.uk>
To: Klaus Hasselmann <klaus.hasselmann@dkrz.de>, Carlo.Jaeger@pik-potsdam.de, Martin Welp <Martin.Welp@pik-potsdam.de>, schellnhuber@pik-potsdam.de, Ottmar.Edenhofer@pik-potsdam.de, tol@dkrz.de, ccarraro@helios.unive.it, ccarraro@unive.it, juergen.engelhard@rheinbraun.de, baldur.eliasson@ch.abb.com, hourcade@msh-paris.fr, ola.johannessen@nrsc.no, gretz@mail1.tread.net, bill.hare@ams.greenpeace.org, SSinger@wwfepo.org, guentherr@wwf.de, gberz@munichre.com
Subject: Re: ECF position paper
Date: Fri Aug 31 17:37:14 2001

Klaus,
A few belated comments on your 1st draft ...... which is looking promising:
a. we need to be careful about using concepts/terms such as 'unacceptable' global warming.
As I think Richard Tol says, we do not have any sound basis for determining what constitutes 'dangerous' climate change. Is it one life lost? a nation-state inundated? or some more utilitarian exceedance of a benefit/cost ratio? Does every citizen on the planet have a vote or just each government? We should draw attention to the rather flimsy basis upon which notions of safe or dangerous, tolerable or unacceptable climate change are debated. In the end of course there are lots of things we may view as 'unacceptable' (war for example), yet they happen and we survive. I think this is an area rich for research and we could draw out some of the dimensions.
b. later on you use the idea of balancing abatements costs vs. the risks of climate change. I think we need to use the language of risk here and to draw upon insights developed by risk analysts (academic and professionals) about how we frame the climate change problem in risk terms. The differential perceptions of risks, inc. climate ones, therefore becomes central in addressing point a.
c. the proposed ECF project on changes in extreme weather is of course a necessary first step towards the quantification of climate risks. This should be one of the justifications for work in this area. It is also the case that better understanding of these changes will yield insights into how adaptation does or should proceed, at both environmental systems and institutional systems levels.
d. re. nuclear energy in a climate protection portfolio, the ECF should be bold and should question and expose assumptions made on both sides of the debate about the up and down-sides of this technology. It is rising higher on the UK agenda and there will be some challenging times ahead in this country about its rightful place and role.
I look forward to seeing the second draft,
Mike
At 14:24 11/08/01 +0200, Klaus Hasselmann wrote:

Dear colleague:

I was requested on the 6.August telephone conference by the ECF skeleton board and the
members of the former ECF steering committe to coordinate the writing of an ECF position
paper, as agreed upon at the ECF meeting in Brussels on July 12.
It was proposed that we complete the position paper and present it to the press about a
week in advance of the Marrakech COP 7 meeting in November this year.
I suggest the following timetable:
1) preliminary agreement on the structure and contents of the paper by the end of this
month,
2) production of first draft in September,
3) detailed discussion of first draft on 2nd October in Potsdam (an additional day ahead
of the 3-4.October ECF meeting, which was proposed on 6.August to discuss the details of
the various projects agreed upon at the Brussels meeting)
4) completion of the paper in October.
5) November: presentation of the paper
I would hope that apart from the 2nd October meeting we can achieve our task by e-mail.
But a meeting may be necessary in September. If so, we should try to combine it with one
of the other project meetings that will be taking place in September.
Everybody is invited to participate. Please feel free to copy this mail to other ECF
members or potential members who I may have missed.
It has been suggested that the position paper should be short, about 5 pages, plus some
appendices if necessary.To get the discussion going, I propose the attached structure as
straw man. Please note that many of the points I have listed are my own views, and I
will by happy to - and expect to - modify them based on your responses.
With best regards
Klaus

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e-mail: klaus.hasselmann@dkrz.de

247. 1000132513.txt
##

From: "Michael E. Mann" <mann@virginia.edu>
To: Ed Cook <drdendro@ldeo.columbia.edu>
Subject: Re: Esper/Cook paper
Date: Mon, 10 Sep 2001 10:35:13 -0400
Cc: "Malcolm K. Hughes" <mhughes@ltrr.arizona.edu>, "Michael E. Mann" <mann@virginia.edu>, Crowley_Hegerl <tcrowley@nc.rr.com>, jto@u.arizona.edu, rbradley@geo.umass.edu, Jan Esper <esper@wsl.ch>, srutherford@gso.uri.edu, p.jones@uea.ac.uk, k.briffa@uea.ac.uk

<x-flowed>
Hi Ed,

Just to reiterate one more key point---Superimposing the two series and their uncertainties is not the whole story (although it is a definite improvement over just showing the two reconstructions on top of each other w/ know assessment of uncertainty). However, doing the above still only poses the question:

apple +/- [uncertainty in apple] =? orange +/- [uncertainty in orange]

As we discussed in a previous email exchange (based on the correlations you calculated between instrumental series w/ the trend removed) , the two reconstructions should probably only share about 60% or so variance in common in the best case scenario, where there is no uncertainty at all, owing simply to the differing target regions/season...

So we need to be very careful w/ the following statement which you made in your previous email:

"If so, this would not mean that the series are not significantly different from each
other. One can't dismiss the highly systematic differences at multi-centennial timescales quite so easily."

I'm not sure you can justify that statement based on sound statistical reasoning!

I agree w/ your following statement "Why these differences are there is the crux question."

However,I hope the discussion will accurately reflect the fact that the leading hypotheses to be rejected in answering that question are 1) random uncertainty in the two series owing to differing data quality and sampling, etc. can explain the difference and 2) systematic differences owing to differing target region and seasonality can explain any residual differences after (1).

That may be a tough standard to beat, but it *is* the approach that Tom, Phil, Keith, and I have all been taking in addressing the issue of whether our different reconstructions are or are not inconsistent and the conclusion has in general been (see e.g. IPCC which was really a consensus of many of us, though admittedly only I was a lead author) that, despite notable differences in the low-frequency variability, the different reconstructions probably cannot be considered inconsistent given the uncertainties and differences in seasonality/spatial sampling. I have a hard time understanding why the same standard should not be applied to comparisons w/ your current reconstruction?

Does your RCS reconstruction really not fall in the mix of all the other reconstructions? Is it truly an outlier w/ respect to Phil's, Tom's, MBH, and other existing N. hem reconstructions that are based on different seasonality and regional sampling???

We've probably had enough discussion now on this point, so I'll leave it to you to discuss the results in the way you see most fit, but I really hope you take the above points into account, in fairness to the previous work...

I look forward to seeing the final manuscript in one form or another, in any case,

cheers,

mike

At 08:10 AM 9/10/01 -0400, Ed Cook wrote:

>I do intend to put in a new Fig. 5 that will compare the mean RCS with MBH,
>including each series' confidence limits. This will be done on low-pass
>filtered data (probably 40 year because of what Mike has sent me). I am
>sure that there will be significant overlap of confidence limits,
>especially prior to AD 1600, when they are quite wide in MBH. If so, this
>would not mean that the series are not significantly different from each
>other. One can't dismiss the highly systematic differences at
>multi-centennial timescales quite so easily. Why these differences are
>there is the crux question.
>
>Cheers,
>
>Ed
>
> >Dear Ed and Jan,
> >I have a couple of general comments, and then some specific little things
> >that
> >may be helpful. It is possible that some of the answers to my questions
> >may be
> >in the two manuscripts in review or in press (TRR and Dendrochronologia) to
> >which you refer.
> >   It seems that your results are consistent with the general shape and
> >some of the detail of the MBH99 series, apart from departures before 1200
> >and in
> >the 19th century. As the two datasets are largely, but not completely,
> >independent, this is an important result. At the time when your
> >replication is
> >weakest, there appear to be differences between the linear and
> non-linear RCS
> >curves and the MBH series. Before about 1200 your dataset is dominated by
> >material from four sites, I think - Polar Urals, Mongolia, Quebec and the
> >Taimyr
> >Peninsula. It therefore seems to me that it is important to make the
> >kinds of
> >direct graphical comparisons that Mike suggests of both your series and
> >the MBH
> >series (superimposed and with their confidence limits shown). Perhaps the
> >differences you note are not robust, and then there would seem to be little
> >reason to seek climatological explanations. I suggest that the graphical
> >comparison Mike suggests will be important since it should allow some
> >assessment
> >of the extent to which MBH and others have or have not underestimated
> >temperature in the AD 1000-1400 period, if your arguments hold up.
> >   I think that a reasonable reader would have some questions about this
> >particular application of the RCS approach. Maybe an expansion of the
> >footnote
> >might help. How does the determination of the form of the regional
> >standardization curve itself depend on replication within each sampled
> >population? Do we know that the regional standardization curve does not vary
> >with time? Or, do we know that the regional standardization curve does
> >not vary
> >with climate on multicentennial timescales? If so, how? Is it not quite
> >possible
> >that the level of the part of the curve for, say, trees between ages 100
> >and 300
> >is set by climate in the early life of the tree, or that it is itself
> >directly
> >determined by contemporaneous temperatures? A number of these questions
> >occur to
> >me because I have been struggling with RCS in the Yakutia material I have
> >been
> >working on with Gene Vaganov. We have a very good situation for the

> >application
> >of the method, with a couple of hundred samples for which we have pith - no
> >estimate needed. Even so, the resulting chronology, once calibrated, gives
> >impossible temperatures in the early part of the millennium. They imply mean
> >early summer temperatures of up to 18 degrees Celsius, which, at 70 degrees
> >north would have led to massive ecological and geomorphological change.
> I can
> >find no evidence for this. I would not be at all surprised if an
> >examination of
> >the Taimyr material you used were to show the same thing. I say this
> >because I
> >know Mukhtar Nuarzbaev's RCS chronology from the Taimyr shows these very
> high
> >levels at precisely the same time as the Yakutia material. Perhaps Mukhtar
> >and I
> >are misapplying the RCS method - a real possibility at least as far as I am
> >concerned. Alternatively, there is some problem with RCS that we have yet to
> >identify.
> >We are all stuck with a more fundamental problem, which is that we have no
> >way
> >to calibrate multicentennial variations. You have used one method of
> >producing
> >chronologies with greater low frequency variability, one that has some very
> >appealing characteristics. There are other ways the same objective could be
> >reached, but we do not have a simple way to choose between them in most
> >cases. I
> >do think it would be interesting to compare the RCS for the Sierra Nevada
> >material you used, if it contains enough samples to do that, with the Great
> >Basin upper forest border network, as highgraded to only contain samples
> with
> >minimum segment length of 500 years, and very conservatively detrended.
> >
> >Here are some specific points:
> >In the penultimate line on page 2 you refer to 1,205 tree ring series
> from 14
> >locations. Some readers will for sure be confused by the word "series" in
> >this
> >case - how about "core samples" or "radii" or "trees"?
> >Page 3 - I need to check this, but I think the segment lengths in the
> >relevant
> >series in the MBH99 analyses are much longer than 400 years.
> >Page 5 - The differences of timing in high values between the linear and
> >non-linear chronologies are actually quite striking. I think if you and I
> >were
> >looking at a couple of subsamples from a single site we would put these
> >differences down to inadequate sample depth.
> >Page 6 - you talk about the two series (RCS and MBH) disagreeing strongly,
> >but
> >at the moment there is no basis available to the reader to see how strongly.
> >This comes back to Mike's suggestion of a direct graphical comparison with
> >confidence limits, etc.
> >
> >Hope this helps, Cheers, Malcolm
> >
>
>
>==================================
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>Lamont-Doherty Earth Observatory
>Palisades, New York 10964 USA
>Email: drdendro@ldeo.columbia.edu

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>==================================

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248. 1000140042.txt
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From: "Malcolm Hughes" <mhughes@ltrr.arizona.edu>
To: "Michael E. Mann" <mann@virginia.edu>, Ed Cook <drdendro@ldeo.columbia.edu>
Subject: Re: Esper/Cook paper
Date: Mon, 10 Sep 2001 12:40:42 -0700
Cc: "Malcolm K. Hughes" <mhughes@ltrr.arizona.edu>, Crowley_Hegerl
<tcrowley@nc.rr.com>, jto@u.arizona.edu, rbradley@geo.umass.edu, Jan Esper
<esper@wsl.ch>, srutherford@gso.uri.edu, p.jones@uea.ac.uk, k.briffa@uea.ac.uk

Dear Ed - Didn't Keith Briffa also come up with a more marked LIA
than MBH99 in his age-band work? If this turns out to be right, it
should eventually be easier to find the sources of the differences
between the reconstructions, just by virtue of there being not only
many more tree-ring data for that period, but also more other, data,
such as documentary. Cheers, Malcolm
Malcolm Hughes
Professor of Dendrochronology
Laboratory of Tree-Ring Research
University of Arizona
Tucson, AZ 85721
520-621-6470
fax 520-621-8229

249. 1000154718.txt
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From: Ed Cook <drdendro@ldeo.columbia.edu>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: Re: the real message
Date: Mon, 10 Sep 2001 16:45:18 -0400

Hi Keith,

You probably haven't seen the newest version, which has not yet been
submitted, but I CLEARLY state that several of the data sets/sites used in
the paper have been used before and I reference all of the relevant papers.
I never implied anywhere that this was the first successful use of RCS. I
also reference your Quat. Sci. Rev. paper and your Age Banding paper. I
also state in the concluding section that what has been shown is not new,
but it is somewhat novel (the separation of the data into RC curve classes
and the regionalization of the data on the scale described) and
informative. I stand by that completely. So, the version I am working on

covers (hopefully) some of your concerns/complaints. I will do my best to be "fair" before I finally submit it. However, this is a Report to Science (~2500 word limit), so I can't do the kind of review of the literature and detailed discussiion of results that would be possible in more normal size papers.

Sorry for sounding a bit testy here. I've been fielding a whole raft of questions, comments, and criticisms from Mike Mann, Tom Crowley, and Malcolm Hughes. Some of them useful, many of them tiresome or besides the point. I never wanted to get involved in this quixotic game of producing the next great NH temperature reconstruction because of the professional politics and sensitivities involved. All I wanted to do was demonstate with Jan that Broecker was wrong, something that you have obviously done a few times before but in journals that Broecker and others don't follow closely (I guess. I should also say that the amount of ignorance about tree rings in the global change/paleo/modeling community is staggering given what has been published. Like it or not, they simply don't read our papers.). In so doing, it seemed reasonable to compare the RCS chronology against the hockey stick because that is the series that Broecker was railing against. That is why I didn't bother to compare the series against all the other records produced by you, Phil, and others. Jan originally did that, but I chose to restrict the comparison to tighten the focus of the paper. More reference to your results is clearly justified, so maybe I was wrong here.

This all reinforces my determination to leave this NH/global temperature reconstruction junk behind me once I get this paper submitted. It's not worth the aggravation. However, the paper is something that I need to do for Jan. And I still think it is a good paper.

Cheers,

Ed

>What I really mean is that you have written this paper implying that you
>are getting low-frequency NH temperatures out of tree-ring data for the
>first time- using the RCS. You set up this question then use a lot of data
>in your analysis and the RCS as though they have not been analysed like
>this before and then show you get more of a LIA than Mann ,   while
>ignoring the fact that I have already produced calibrated summer
>temperature curves (in the Science Perspective piece) from RCS ring width
>data in Sweden , Urals , Taimyr   and (in the JGR paper) using banded
>density - which both show more low frequency than MBH. The real question is
>whether MBH use data in tropical and mid latitudes that supress what is
>really a high latitude summer signal in their northen predictors ? I just
>don't think you are being very fair here- despite how many times you cite
>me (perhaps the citations should anyway reflect the useful contributions
>to a particular area even if they number more than a token couple)
>that's off my chest now
>cheers
>Keith
>
>--
>Professor Keith Briffa,
>Climatic Research Unit
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>
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==================================
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==================================

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##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: Ed Cook <drdendro@ldeo.columbia.edu>, "Michael E. Mann" <mann@virginia.edu>
Subject: Re: Esper/Cook paper
Date: Mon Sep 10 20:34:13 2001
Cc: "Malcolm K. Hughes" <mhughes@ltrr.arizona.edu>, Crowley_Hegerl <tcrowley@nc.rr.com>, jto@u.arizona.edu, rbradley@geo.umass.edu, Jan Esper <esper@wsl.ch>, srutherford@gso.uri.edu, p.jones@uea.ac.uk

Ed
I still believe you are not showing sufficient comparisons with series besides the MBH ; necessary to demonstrate the true extent of "new" information in this work. At the very least this needs to acknowledge that other (and other tree-ring-based) series are out there , that use at least some of the data you employ , and use the RCS method to process may of their constituent series - i.e. the Northern chronology series shown in my QSR paper. What is similar and what is different in your series and this one?
You give the impression here that you are using the RCS and new data to demonstrate the possibility of getting more low frequency signal from tree-ring data - but then you base this on a comparison with MBH only. Surely what is needed here is to establish WHY MBH don't get as much LIA for example . By not showing that other tree-ring data that have also shown a LIA , and not exploring why MBH does not (despite using some of the same -and note -already RCS standardised data) is perhaps confusing rather than clarifying the issue.
When we discussed this here, I also suggested the need to show separate "north" and more "south" curves ,separated in your data set, to try to get at least some handle on the independent expression of the centennial trends in a region south of the over-exploited northern network . At the very least it should be clearly stated that many of the site data used here and in previous work (see our Science perspectives piece) are common and other series already produce more low-frequency signal than is implied in MBH .
Sorry for this rushed comment but I wanted to get this point over as we had talked about it

before but you don't seem to have taken it on board.
cheers
Keith
At 02:51 PM 9/10/01 -0400, Ed Cook wrote:

Hi Mike et al.,
Okay, here is an overlay plot of MBH vs. RCS, with RCS scaled to the 1900-1977 period of MBH, and with 95% confidence limits. This has been done for the 40-yr low-pass RCS data to be consistent with the low-pass MBH series you sent me. The 95% confidence limits of the RCS are also scaled appropriately. Since correlations with both instrumental and MBH are O(0.95) after even 20-year smoothing because of the trend, the RCS limits are effectively based on the bootstrap 95% limits of the 14 chronologies. Assuming that the original RCS C.I.s are reasonably accurate (which I think they are), what is apparent (to me anyway) is that the confidence limits of MBH are uniformly narrower after AD 1600. Prior to that, they are comparable to RCS back to ca. AD 1200 where RCS C.I.s get bigger. Of course this is an odd comparison because the confidence limits are not derived the same way. However, I do think that they are somewhat informative nonetheless. What is also apparent is the much great amplitude of variability in the RCS estimates. This is consistent with the understanding that extratropical temperatures are more variable than tropical tempertures, which supports the idea that the MBH record does have more tropical temperature information in it. The other interesting thing about expressing the RCS data this way and overlaying it on MBH is the appearance that MBH is missing the LIA rather than the MWP, at least on multi-centennial timescales. This turns some of Broecker's criticism of the "hockey stick" on its head. I'm not sure where all this leads.
Any comments and further suggestions are welcome as long as they come in by tomorrow. I am definately submitting the paper within a day or two.
Cheers,
Ed
==================================
Dr. Edward R. Cook
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Tree-Ring Laboratory
Lamont-Doherty Earth Observatory
Palisades, New York 10964 USA
Email: drdendro@ldeo.columbia.edu
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==================================

--
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References

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

251. 1000242208.txt
##

From: Rashit Hantemirov <rashit@ipae.uran.ru>
To: Keith Briffa <k.briffa@uea.ac.uk>
Subject: INTAS
Date: Tue, 11 Sep 2001 17:03:28 +0500
Reply-to: Rashit Hantemirov <rashit@ipae.uran.ru>

Dear Keith,
below is the list of the Ekaterinburg team members with brief description.

=========

Stepan G. Shiyatov, Prof., Dr., head of the Laboratory of Dendrochronology, leader of Ekaterinburg team, took part in collecting subfossil wood in the Yamal Peninsula, cross-dating ring-width series, developing and analysing the multimillennial ring-width chronology. He has also carried out the work on evaluation of changes in composition and structure of forest-tundra ecosystems in Polar Urals.

Rashit M. Hantemirov, Dr., took part in collecting subfossil wood in the Yamal Peninsula, cross-dating ring-width series, developing and analysing the multimillennial ring-width chronology. He has also developed and analysed juniper chronology in Polar Urals.

Valery S. Mazepa, Dr., took part in treatment of individual ring-width series and analysing of the Yamal long chronology. He has also carried out the work on estimating of changes in woody biomass in Polar Urals.

Alexander Yu. Surkov, technician, took part in collecting, preparing and measuring the subfossil wood from Yamal Peninsula

=========

Finances(Eu):

Labour	Overhead	travel/sub	equipment	consum	other	total
12500	1250	7900	2950	400	0	25000

For any case - how many got each team member (Eu):

Shiyatov S.G.	4000
Mazepa V.S.	3800
Hantemirov R.M.	3700
Surkov A.Y.	1000

Best regards,
Stepan G. Shiyatov
Rashit M. Hantemirov

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252. 1001695888.txt
###
##########

From: Keith Briffa <k.briffa@uea.ac.uk>
To: t.osborn@uea.ac.uk
Subject: [Fwd: Rapid Climate Change]
Date: Fri Sep 28 12:51:28 2001

Date: Thu, 27 Sep 2001 11:32:30 +0100
From: Simon Tett <simon.tett@metoffice.com>
Subject: [Fwd: Rapid Climate Change]
Sender: simon.tett@metoffice.com
To: k.briffa@uea.ac.uk, sandy.tudhope@ed.ac.uk
X-Mailer: Mozilla 4.75 [en] (X11; U; HP-UX B.11.00 9000/782)
X-Accept-Language: en
Dear Keith/Sandy,
please don't pass on or discuss further -- this is the email I got from Phil Newton. So with some reluncance I get to put up a strawman. I will go with what we discussed in London but some nice graphics (or any thoughts) would be helpful -- do you have any you can send me.
Simon
Date: Fri, 21 Sep 2001 16:02:14 +0100
From: Philip Newton <ppn@nerc.ac.uk>
Subject: Rapid Climate Change
To: sfbtett@email, a.j.watson@uea.ac.uk
Cc: Meric Srokosz <MAS@soc.soton.ac.uk>,
Catrin Yeomans <CVY.DST.Swindon@wpo.nerc.ac.uk>,
Judy Parker <JMP.DST.Swindon@wpo.nerc.ac.uk>,
Nigel Collins <NRC.DST.Swindon@wpo.nerc.ac.uk>,
Neville Hollingworth <NTH.DST.Swindon@wpo.nerc.ac.uk>
Message-id: <md5:867B0102E7BAE34BCAE86F2E32B8167E>
MIME-version: 1.0
Content-type: multipart/mixed; boundary="Boundary_(ID_5Sy4P7Icy2zVEqcBr4S8jA)"
Dear Simon, Andy,
Many thanks for agreeing to each give an informal presentation to the Steering Committee on the first afternoon of the meeting.
As I mentioned on the phone, what I'm after is for each of you to look at the Abrupt proposal and Prescient proposal/draft-science plan (attached as WORD documents), stand well back, and put forward some ideas for how one might combine them into a single coherent programme. The intention is to lay the foundation for some discussion, both Monday afternoon and evening, in advance of the formal Steering Committee meeting item that will deal with developing a single science plan. All SC members will have the attached documents in their papers.
I'll summarise the few constraints we have at the start of the Monday session, so you won't have to revisit the history; by the time we get to you, all will know that we have the task of coming up with a single plan, and the events leading up to that circumstance.
The constraints as I see them are:
The Rapid Climate Change programme has a budget of £20m. The Abrupt proposal was written to £16.9m, and the STB decided to invest £17.0m in thermohaline-related rapid

climate change. This proposal contained both palaeo and modelling components
(as well as
modern observational/process work), and a strong complementarity and close
working
relationship with Prescient was always envisaged by the writers. The Prescient
proposal
was written to £8m, and the Prescient draft science plan (following reduced
award) was
written to £4.5m. The STB did not have a discussion about how the science of
the two
programmes should be combined, but the nature and chronology of
events/discussions imply
that the STB decision to spend £17m on thermohaline-related work should be
respected. I
do not see that this has to be translated as an inexorable shackling of the
£4.5m
Prescient science aims, given that a good fraction of the Prescient draft
science plan
seems to be potentially relevant to thermohaline-related climate change, and
that there
is notionally £3m of the £20m that is not tied to thermohaline-related work,
and there
is a strong palaeo/modelling element to Abrupt.
So much for constraints. I do not want to give the impression that we are after
a
ring-fencing of Prescient and Abrupt monies and aims within Rapid. I would
hope that
there is scope for a much more integrated (in the sense of both palaeo/modern
and
obs/model) and coherent programme than that. One potential conflict, in the
modelling
context, seems to be the apparently regional approach of Abrupt cf the global
approach
of Prescient. I suspect (but may be wrong) that there is a scientific debate to
be had
as to whether an Atlantic-centric approach is sufficient to consider
thermohaline-related climate change over NW Europe, or whether a more global
treatment
is required.
On practicalities, I've got you down for 20 minutes each, and have set aside
half an
hour for discussion straight afterwards. Please let Catrin Yeomans
(cvy@nerc.ac.uk)
know your audio-visual needs.
Get back to me if you need further clarification.
All the best,
Phil
Dr Philip Newton
Head of Marine Sciences Team
Science Programmes Directorate
Natural Environment Research Council
Polaris House
North Star Avenue
Swindon
SN2 1EU, UK.
Tel: +44 (0) 1793 411636
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[1]http://www.cru.uea.ac.uk/cru/people/briffa[2]/

References

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

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From: "R K Pachauri" <pachauri@teri.res.in>
Subject: TERI launches TerraGreen, an e-magazine on the environment
Date: Wed, 28 Nov 2001 16:40:00 +0530

TERI is proud to announce the launch of TerraGreen (http://www.teriin.org/terragreen/), an e-magazine that will bring you news about energy, environment and sustainable development from India, once every two weeks.

TerraGreen was formally launched on Wednesday, November 28, 2001 by Mr. C. M. Vasudev, Secretary, Department of Economic Affairs, Government of India in New Delhi.

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You are receiving TerraGreen because you have shown interest in TERI's research, multifarious activities or numerous publications over the years. Your address is saved in TERI's central database of e-mail addresses. If you should prefer not to receive this e-mail in future, please let us know. To do this, please scroll down to the end of this e-mail.

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TERRAGREEN
News to Save the Earth

Issue 1, 15-30 November, 2001

Letter from the editor

Here is the first issue of TerraGreen, an e-magazine that will bring to you the most significant shakeouts in India's energy, environment and sustainable development scenarios. For concerned individuals across the world looking for reliable news and information in these fields from India has often been an uphill task. TERI has worked for over quarter of a century to disseminate information from these very fields. Taking that mandate forward, TerraGreen will bring you analytical, unbiased and straightforward reportage. In the wilderness of the Internet you will soon learn to rely on TerraGreen for news, views and information. So, welcome to the wilderness. Enjoy.

For full text click on: http://www.teriin.org/terragreen/issue1/letter.htm

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News of the fortnight

What's happening in our green horizons and elsewhere? TerraGreen's news updates bring you the latest in environment news.

This issue's headlines

Pepper and people power
Periyar Tiger Reserve, Kerala- The India Ecodevelopment Project brings a much-needed economic fillip to the lives of Mannan and Paliyan tribals through pepper cooperatives. Find out how it all happened at http://www.teriin.org/terragreen/issue1/news.htm#pepper

Sunny through the clouds
New Delhi- Anybody for the sun? Soft loans for setting up solar water heaters in group housing societies from IREDA. Visit http://www.teriin.org/terragreen/issue1/news.htm#sunny to also find out about sun-powered electricity in the high, cold reaches of the Himalayas, for villagers in Leh and Kargil.

Of Birds and War
Afghanistan- The terror of war and bombings in Afghanistan is spreading far. So hangs the fate of India's winged migratory friends -- the Siberian crane, shoveller ducks, the crested poacher and Arctic tern, to name a few. At http://www.teriin.org/terragreen/issue1/news.htm#birds read about these avian anxieties.

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The Long Story

Let the Gentle Giants Be
Veraval, Gujarat- Fahmeeda Hanfee's first-hand report on the huge but vulnerable whale shark, and on a milestone that is something of a first in the official protection for marine life in India. Hanfee analyses the pros and cons at http://www.teriin.org/terragreen/issue1/feature.htm#f1

The Water Harvest
Kalakhoont-Madhya Pradesh, Sangani-Gujarat- Arnab Ray Ghatak's inspiring report of villagers (http://www.teriin.org/terragreen/issue1/feature.htm#f2) who looked beyond governmental apathy to drill water from parched lands on their own and are now reaping a golden harvest.

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In Conversation

At a time when a lot of people across India are grappling with power shortages, Mr Suresh P. Prabhu, Union Minister for Power, talks to TerraGreen's executive editor in a one-on-one.
http://www.teriin.org/terragreen/issue1/interview.htm

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Centrepiece

No one Need Go Hungry
Dr. L. C. Jain, Chairman, Industrial Development Services, economist and Gandhian, Dr L C Jain, unfolds a simple blueprint to change the bizarre food security situation India faces today - of rotting foodgrains and starvation deaths. Read more about Jain's views at http://www.teriin.org/terragreen/issue1/essay.htm. He laments that if Gandhi were to be around today and learnt of this cruel irony, he would invite an assassin to end his life.

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Reviews

Get the latest on your green reads. This week: Subhadra Menon reviews Brenda Cranney's The Mountain Women of Himachal Pradesh. Plus more short reviews at http://www.teriin.org/terragreen/issue1/reviews.htm

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People in Action

Ever wondered how to reach people working at the grass-roots? To be able to make a difference? Let nothing stop you, contact them to work alongside, or just to help. Go to: http://www.teriin.org/terragreen/issue1/people.htm

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Forthcoming Events

Check out our green calendar http://www.teriin.org/terragreen/issue1/events.htm for the fortnight.

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Factfile

At http://www.teriin.org/terragreen/issue1/facts.htm check out some interesting facts about the environment around us.

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CONTACT

Reach the executive editor of TerraGreen at
http://www.teriin.org/terragreen/contact.htm

FEEDBACK

Need to reach us at TerraGreen with comments or suggestions? The second issue of TerraGreen is in the pipeline, do mail us at terragreen@teri.res.in or please fill the form at http://www.teriin.org/terragreen/feedback.htm

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http://www.teriin.org/terragreen/about.htm

ABOUT TERI

If you would like to know more about TERI as an institute and our other publications visit us at http://www.teriin.orga

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Visit www.teriin.org/dsds/ for full audio and
video coverage of the Delhi Sustainable
Development Summit, 7-9 Feb. 2001.

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From: Tim Osborn <t.osborn@uea.ac.uk>
To: Myles Allen <m.allen1@physics.ox.ac.uk>
Subject: RE: RE: Tyndall proposal
Date: Wed Dec 12 09:29:29 2001

At 00:03 12/12/01, you wrote:

> Hi Tim and Phil,
> I'm afraid I missed their deadline -- I'm presenting at the Royal Society meeting on IPCC tomorrow, and that had to take priority. If Simon is interested enough to bend some rules quietly, I could certainly get him an outline proposal by Friday, but if not, it'll have to wait until their next call. It's frustrating, but it can't be helped. NERC just have too many calls. As Simon points out, the Tyndall Centre's style may be a more top-down, regulatory approach anyway, and good luck to them. Politically negotiated emission targets may work, but I have to confess to having doubts. Perhaps I have spent too much time talking to Dick Lindzen to believe in central planning any more.

Myles, by "Simon" do you mean Simon Shackley? I don't think he'd be able to bend the rules since the proposals have to go direct to the Tyndall Centre's administrator. As you say, they are being more directive (is that a word?) in what they want this time round, and since your idea isn't central to what they think they want I doubt whether they'd be prepared to bend the rules. Hope the Roy Soc goes well - I hear they're charging 100 quid to listen to you - a bargain!
Tim

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From: Keith Briffa <k.briffa@uea.ac.uk>
To: Ed Cook <drdendro@ldeo.columbia.edu>
Subject: Re: Science paper
Date: Mon Dec 17 15:13:14 2001
Cc: esper@wsl.ch

Ed (and Jan)
Frankly I am a bit surprised at your and Jan's response to my letter.

I thought I had explained clearly what I was writing to Science and did only that. After
some not too little experience in reviewing for Science and Nature , I returned what I
considered to be a very positive response, one which I knew Science would interpret as a
call to publish important results. This is precisely what they have done and no more could
have been expected. As to the sentiments and opinions expressed , they are objective and ,
in my opinion still correct. They are to be interpreted as a request for re-thinking the
logic and rationale of the presentation. I do not see why they require more than some
re-phrasing. Though I will admit that they ask for some minor (entirely justifiable) work
to include the correlations with summer seasonal data. I simply would not like to see you
write a paper that puts out a confused message with regard to the global warming debate ,
leaving ambiguity as to your opinion on the validity of the Mann curve and implying that
your series is a annual record , when I do not believe that you think it is. To get Science
to consider a rewrite is surely what you would have hoped for , and satisfying my remarks ,
in small or large measure , will not be the determinant in getting this published. Indeed ,
it may well be that the tone of my letter could have convinced them that this was important
work that should be published (though with some provisos) despite what other reviewers may
have thought. What did the other reviewers say?
If you think I was too negative then I am sorry that we don't agree entirely - but that at
least is the normal !
I would not like this affair to ruin my Christmas , as it surely will if it is the cause of
our falling out . As for your message Jan , I prefer to think you were trying to calm
troubled waters , though you seem peculiarly adept at doing the opposite where I am
concerned, I prefer to ignore the remark about "not wanting to let this curve into Science"
(a response might only injure the prospects of any further collaboration) but I will say
that it goes without saying that Ed can have his opinion , just as even I can have. I would
never consider myself stupid enough to imagine I could ever influence your response to
Science by anything other than reasoned argument. If this is not accepted then ,at least Ed
I am sure knows that, I would not let this stand in the way of this paper.
Ed, I am sorry to hear about your condition and I do know how debilitating it is. Useless
as it is , you have all my sympathy and best wishes for a rapid recovery.
I am likely also guilty of short temper and extreme frustration at the moment because of
conflicts between family and work , both sides demanding more time and both being
increasingly ill served by me. Somewhere in the middle I feel increasingly suffocated of
late and in moments of sane reflection can see that much of the trouble could perhaps be

lessened if one had time to be more considered in ones actions - but the moments of quiet
reflection are invariably harder to find.
I am totally confident that after a day's rephrasing this paper can go back and be
publishable to my satisfaction by Science. I am equally confident that this interchange
was a waste of yours and my time . To the extent that I am culpable , I am truly sorry.
Keith
At 09:23 AM 12/17/01 -0500, you wrote:

Hi Keith,
First, I need to apologize a bit for what I wrote to you. It was a bit over the top and came out during some serious physical discomfort that I am experiencing now from a bout of shingles(? I'll find out from the doctor today). It is all rather painful and depressing. So while I still think that we have very real differences of opinion on the paper, I would hope that we can accept at least some of these differences as part of the scientific debate process and not let it affect us negatively or personally.
Paul Krusic came by yesterday and brought with him several parcels from the lab, including the paper from Science. The editor will not accept the paper as submitted, but will consider it after revision. Obviously, this is as good as we should have expected. I will do whatever I can to satisfy the reviewers comments, including yours, but probably can not rewrite it in a way that will satisfy all of your concerns. At that point, it will be up to Science to decide how to proceed.
Regards,
Ed
==================================
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[1]http://www.cru.uea.ac.uk/cru/people/briffa[2]/

References

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