Global Warming and Climate Data

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Most of the global warming data come from research done at the University of East Anglia, funded by the British Government. All material here is presented under the Fair Use doctrine and as allowed under the UK FOIA.

The following are citations from the climate data modeling program suite used to show global warming. This set of programs constitute a data-mining application, and as such do not represent hard numbers. Instead, they represent inferred trends. I don't expect you to take my word for anything. Instead, I present the researchers' actual words, along with the programs in which they appear. The presentation below is categorized. Some items are highlighted.

To generate this file, I issued a search command designed to find comments in each computer language used in the program suite. (The languages involved are various versions of Fortran and another language that goes by the file extension ".pro").

Geographic Data Removal:

1. All over, the program suite has comments about removing data south of 30N. Each of these programs (`ALL_MAP`, `BOX_MAP`, `COLOR_MAP`, `COMPOSITE`, `COMPOSITE_COL`, `NATURE_COMB`, `NATURE_COMB_LARGE`, `NATURE_FIG2`, `PLOTPATTERNS_DECADE`, `PLOTPATTERNS_DENS`, `PLOTPATTERNS_DIFF`, `PLOTPATTERNS_INSTR`, `PLOTPATTERNS_RWID`, `POLAR_MAP`, `TRI_MAP`, `VAL_MAP`, `YEARLYMAPS`) seem to be mapping programs. According to their comments, we are we only being shown a third of the world for the global warming results.

2. There is also some indication that nothing under 20N is being computed at all, meaning that they're leaving most of Earth out of their calculations:

   `ANALYSE_HEMIBEST` ("*In addition, the near-hemispheric mean of all LAND north of 20N is used.*")
   `EXTRACT_OBS` ("*Compute the northern hemisphere >20N land series*")
   `OBS_LAT_AS` ("*Just keep the northern hemisphere stuff*")
   `OBS_MSLP_AS` ("*Following discussion with Phil, certain regions are removed. We've already removed everything prior to 1922. (1) Remove 80N and 85N (due to Arctic High problems etc.) (2) Remove 15N (3) Set to missing the pre-1950 data at 70N & 75N from 100E to 60W*" and "*(40W if HARSH option is selected)*")
   `OBS_PREC_AS` ("*Just keep the northern hemisphere stuff*")
   `PL_CORR_UNCORR` ("*Compute means over the region north of 50N*")
   `PL_DECLINE` ("*Plots density 'decline' as a time series of the difference between temperature and density averaged over the region north of 50N, and an associated pattern in the difference field.*")

Temporal Data Removal:

1. All over, the program has comments about are ominous at best, talking about cutting out data for one reason or another. Some just omit data, while others specifically omit data they find objectionable:

   `ABDLOWFREQ1GRID, OBSJ04_F7` ("***Get rid of last 6 years (1995-2000), so it stops in 1994***")
   `ANALYSE_REGBEST` ("*Comparison is made by computing correlations over the period 1881-1984*")

BANDALL2NH_SCIENCE_FIXED, BANDALL2NH_YOUNGOLD_FIXED ("*Just keep the 1400 to 1994 portions*")
CALIBRATE_BANDTEMP, CALIBRATE_REGTEMP, CALIB_TDERRS_BANDTEMP, CALPCR_BANDTEMP, CAL_TSD, CAL_TSD_4BAND, PL_LOWFREQ, QUANTIFY_TSDCAL, SHOW_CALIBRATE ("*Get rid of pre-1971 temperatures*")
CALIBRATE_NHRECON ("*stop in 1960 to avoid the decline that affects tree-ring density records*")
CALIB_ERRS_BANDTEMP, CALIB_ERRS_REGTEMP, CALIB_LOWF_BANDTEMP ("*Get rid of pre-1871 temperatures*")
DENSPLUS188119602NETCDF, DENSPLUS2NETCDF ("*we know the file stats at yr 440, but we want nothing till 1400*" and "*we know the file starts at yr 1070, but we want nothing till 1400*")
HOLPT2_FIG2 ("*Plots the grid box coverage of the complete MXD data for the 1697-1976*")
MK_TRENDMEAN_DISTRIB ("*perst=1400*" and "*peren=1900*", meaning period start and end)
MXD_INFILL ("*Only those with complete data over 1800-1964 are kept. Those with complete data over 1800-1976 are used to infill missing values between 1965 and 1976, and those complete over 1697-1964 are used to infill missing values between 1697 and 1799. Any that are not complete over 1697-1976 after infilling are removed, and only the 1697-1976 period is kept and saved.*")
PLOTHISTS_ALL, PLOTMOMENTS_DENS, PLOTEXT_DENS, PLOTEXT_RWID, PLOTHISTS_ALL, PLOTHISTS_DENS, PLOTHISTS_RWID, PLOTMOMENTS_DENS, PLOTMOMENTS_RWID ("***Ignore 1991 and 1992***")
PL_MXDINFILL, PL_MXDINFILL_COL ("*Plots location of MXD grid boxes with complete 1697-1976 data*")
RD_ALLMXD1 ("*we know the file starts at yr 400 or 440, but we want nothing till 1400*" and "*we know the file starts at yr 1070 (or 1071 for trw), but we want nothing till 1400*")
RECON1, RECON_JONES, RECON_MANN, RECON_TORNYAMATAIM ("***stop in 1940 to avoid the decline***")
RECON2 (ditto and "***truncate at 1990***")
RECON_ESPER ("***stop in 1960 to avoid the decline***")

Introduction or Acceptance of Inaccuracy:
These programs all serve to indicate that the model is a rough approximation, and certainly not aimed at being the most accurate representation possible. Time can be traded for memory, crude interpolation is not as good as fine interpolation, a 0.5 degree grid means that objects under 37 miles across are ignored, and using a fixed grid, rather than smaller sizing when better data are available just limit the accuracy (and usefulness) of the results:

SCALEPATTERN ("*has to be omitted and worked around because of lack of memory*")
INTERPOL ("*manages crude interpolation from one grid to another, minimising array sizes while loading all at once approach:*"
KOEPPEN ("*ths was originally used*" and "*it was then modified into the form in GetKoeppen when it was found that on the 0.5deg grid some categories were only used for very small percentage of the land surface; to simplify the classification these were removed*")
ANALYSE_TRWMONCORR ("***THERE IS AN ERROR IN IDL's P_CORRELATE, SO I'VE HAD TO REMOVE OTHER INFLUENCES BY HAND.***")

Now, you may have noticed in this section, and in other sections, the use of the word "correlate". We've all been told that the CRU has been doing climate modeling. This, too, is false. The programs involved are not data modeling programs. When using a data modeling program, you enter the data, use the data to drive a simulation of a process, and deliver a result. To test a model, you use part of the data to predict more data. For instance, you can use data from 1400 to 1900 to predict the result in 1900 to 2000. If it holds, you have a good model. What we have here, though, is data mining. In data mining, you enter a lot of data, and look for relationships between the data. Whatever relationships there are, whether of known or valid connection or not, are considered to be correlators. If you can predict one trend, and it has correlation with other items, then you use the correlation set to make predictions on as much of the system for which you have correlations. For instance, it has been said that men tend to have beards when a Queen reigns in England. That's the kind of answers that data mining gives us.

<u>The Researchers are Not Satisfied with their own Methods</u>:
These programs seem to suggest that the programmers were not satisfied with the accuracy of the algorithms:

`BRIFFA_SEP98_C` ("*Bodge it so that allr(*,*,0)=allr(*,*,1) and allr(*,*,1)=allp(*,*,0)*")
`CALPCR_BANDTEMP` ("*DECIDE WHETHER TO USE THE NSIB AGE-BANDED OR THE NSIB HUGERSHOFF RECORD BECAUSE THE FORMER HAS LARGE ANOMALIES!*")
`COMPUTE_NEFF` ("*Although there are no programming errors, as far as I know, the method would seem to be in error, since neff(raw) is always greater than neff(hi) plus neff(low) - which shouldn't be true, otherwise some information has somehow been lost.*")
`GHCNDISCAN` ("*I'm not convinced by the algorithm for precip, esp for dry regions, but better?*")
`HOMOGENEITY` ("*although this 'works' in the sense of running without execution errors, it does not seem to do a very good job of detecting inhomogeneities, whether with the 'Simple' option turned on or off. This appears to be because there is no indication within the testing procedure itself as to whether the change is gradual or sudde*n")
`PLOTCORR_LAG` ("*SHOULD USE MJJARBAR WHEN IT'S COMPUTED!*")

<u>CO_2 Input</u>:
This program seems to suggest that they are using CO_2 data from Mauna Loa. Mauna Loa is the biggest volcano on Earth, and is likely to have larger CO_2 readings than most other places. For those of you not sure, Mauna Loa is not man-made:

`RD_CO2` ("*Reads in Mauna Loa and Barrow CO2 observations*")

<u>Picking Their Methods?</u>: There is evidence that different models were applied to different data. I can't tell if the purposes were benign or sinister:

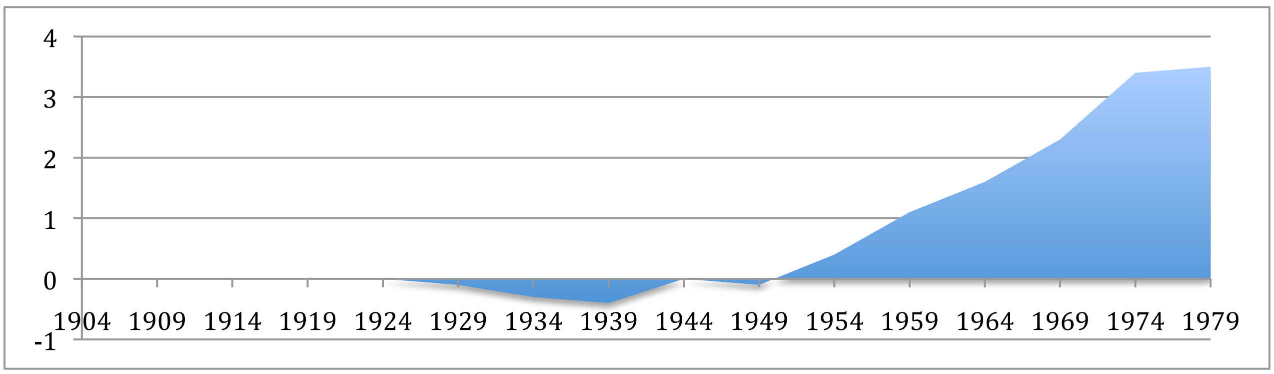
`ANALYSE_CHANGECORR` ("*Correlations over 1901-1950*" and "*Correlations over 1935-1984*")
`CALPCR_BANDTEMP` ("*Model*", "*Period used*", "*Fixed grid to use*", "*0*", "*1683-1981*", "*1950 1800 1700 (1683-99 con, 1700-1900 int, 1901-81 con)*", "*1*", "*1667-1682*", "*1700 (con)*", "*2*", "*1625-1666*", "*1700 (con)*", "*3*", "*1602-1624*", "*1700 (con)*", "*4*", "*1588-1603*", "*1600 (con)*", "*5*", "*1480-1587*", "*1600 1500 (1480-99 con, 1500-1587 int)*", "*6*", "*1402-1479*", "*1500 1400 (1402-1479 int)*", "*7*", "*1982-1988*", "*1950 (con)*", "*8*", "*1989-1991*", "*1988 (con)*", "*9*", "*1992-1994*", and "*1988 (con)*")
`PL_DECLINE` ("*Now fit a 2nd degree polynomial to the decline series, and then extend it at a constant level back to 1400. In fact we compute its mean over 1856-1930 and use this as the constant level from 1400 to 1930. The polynomial is fitted over 1930-1994, forced to have the constant value in 1930.*")

Suspicious (but possibly innocuous) Items:
`MXD_STP_MODES`, `OLAT_STP_MODES` ("*TEMPORARY REPLACEMENT OF TIME SERIES BY RANDOM NOISE!*")
`REEPHAM_EXERCISE` ("*Adjust ALL trees to have mean of 100 and sd of 15*")

We've saved the best (worst) for last…

Some Comments are Just so Brazen as to require a Special Category: Apparently Outright Scientific Fraud:

`BRIFFA_SEP98_D`, `BRIFFA_SEP98_E` ("***Apply a VERY ARTIFICAL correction for decline!!***") The amount of correction applied, as far as I can tell from reading the code, is 0.1° cooler in 1929, 0.3° cooler in 1934, 0.4° cooler in 1939 (see eMail comments about the early '40s "blip" and the planned corrections for that), no correction in 1944, 0.1° cooler in 1949, 0.4° hotter in 1954, 1.1° hotter in 1959, 1.6° in 1964, 2.3° hotter in 1969, 3.4° hotter in 1974, and 3.5° hotter since 1979.



You may recognize this curve as being fairly similar to the "hockey stick". This is input. Other parts of the program apply a 30-year lag to some indicators. If those apply here, we're forcing the jump in the near future, rather than the near past.

`MAPS1`, `MAPS1_MOVIE`, `MAPS1_POSTER`, `MAPS12`, `MAPS15`, `MAPS24`, `MAPS_GENERAL` ("***Uses 'corrected' MXD - but shouldn't usually past 1960 because these will be artificially adjusted to look closer to the real temperatures.***")

`PL_DECLINE` ("***Now apply I completely artificial adjustment for the decline (only where coefficient is positive!)***")

EMail Comments

You've no doubt heard about the eMails, with quotes claimed to be out of context. Here are the quotes, in context. Color highlighting is Sheldon's, and you need read only that for the juicy stuff. If you want the full context, you've got that, too. Headers, footers, and names have been removed, as I don't want to have to deal with any possible lawsuits. I cite the eMail archive IDs, so that you can very easily look them up if you like at the following link or any of a number of others available on the web: http://www.megaupload.com/?d=003LKN94

You may want to scan through the quotes in red first, and then go back for the details. These are in chronological order, with what we consider the best (worst) two at the end.

0841293339

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

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

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

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

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

0887057295

Thanks for the quick response. Responses to responses follows....

(1) I tried the composite GHG plus UIUC SUL on Norm's machine, in just the way you said. However, the results for the USA seem to be identical to those using *only* UIUC GHG input. I'll try again.

(2) You are right in saying one shouldn't scale GHG patterns by GHG+SUL dTs. **However, to be strictly consistent one should never allow GHG patterns to be used alone. So you are *not* being consistent if you allow this---which you do.** The point then is to minimize the extent of the inconsistency.

It is unarguably correct that the global-mean temperature to use is the one containing all forcings (i.e., column 6 in *DRIVE.OUT). The choice then is what pattern(s) to use. If we had no SUL information, we would have to use GHG patterns; as in the original SCENGEN. Scaling these with the MAGICC GHG output would give both incorrect patterns and incorrect global-mean warming. Scaling with column 6 at least gets the global-mean warming correct (within MAGICC uncertainties). You seem to have chosen to get *both* things wrong, instead of just the patterns.

I can see some logic in your method; I just think (strongly) that it is wrong. At the very least, it will be confusing to the user. If the user selects only GHG model patterns, then won't they wonder why the global-mean temperature is inconsistent with MAGICC? To take an extreme case, suppose the full dT is 2degC and the GHG-alone dT is 3degC. Is it better to scale an approximate pattern (i.e., the GHG pattern) by 2degC or 3degC? In my view, GHG scaled by 2degC would be much closer to GHG+SUL scaled by 2degC than GHG scaled by 3degC. **Surely the real issue (given that it is impossible to be entirely consistent in this case) is to get a result that is as close to the 'right' result as possible.** I feel quite sure that scaling by column 6 is best on this basis---

especially given that the patterns are much more uncertain than the global-means. I think this is absolutely beyond doubt.

The bottom line here is that consistency is impossible if one uses only GHG patterns. Column 6 was included deliberately, and after some thought (along the lines noted above).

Of course, it is possible to get column 6 results by adding columns 2, 3, 4 and 5 as they now stand (and as they are in the version that you have). However, one cannot do this with the correct *raw* column 3, 4, and 5 output because of the nonlinear direct forcing effect. It just happens that, in your version, I 'faked up' column 5 as the difference between column 6 and the sum of columns 2, 3 and 4. I did this simply to get the code working; but (as you now know) I never got around to fixing it up until now. In the latest version, column 6 is again equal to the sum of columns 2, 3, 4 and 5 because I scale columns 3, 4 and 5 to ensure that this is so.

(3) **Re HadCM2, again it is impossible to be consistent. What I said before is that the reason for adding these results is simply to make them readily available. I do *not* advocate using them in combination with any other model results. It is, I believe, perfectly reasonable to scale these results with column 6 data. Of course, this 'hides' an assumption about the relative magnitudes of the GHG and SUL components---i.e., it assumes that the HadCM2 relative magnitudes are okay. The point of scaling, however, is to account for other factors that change the global-mean temperature relative to HadCM2 results, such as different sensitivities.**

I agree with you that it would not be an efficient use of time splitting the HadCM2 SUL results into GHG and 'aerosol' component patterns. The whole point of the sulphate part of SCENGEN is to look at the influence of different SO2 emissions patterns. Splitting up HadCM2 wouldn't help here at all.

I also think it would be valueless to hardwire HadCM2 dT results into SCENGEN---again, this would defeat the purpose of including these results. It would introduce an additional inconsistency; since HadCM2 patterns change with time, it would not be logical to scale the 2071-2100 pattern with (e.g.) 2031-2060 dT. Of course, you could argue that it is illogical to scale this pattern with (e.g.) 2031-60 dT from MAGICC; but this is a different issue that I don't think is worth discussing at this time.

(4) Thanks for explaining the UIUC 'other data' problem. I will ask Redacted whether he can provide full global fields for the other variables, since it really would be valuable to include them. If he can give us these data, could you add them to SCENGEN? (re this, see below)

(5) I appreciate your problems with Redacted. As far as I can see, incorporating the revised MAG.FOR code into MAGICC/SCENGEN shouldn't be too difficult. I can, however, get hold of some money to pay for some of Redacted's time to do other work---perhaps $5000 or so. Can we set something up? The contractual side would be easy---just a matter of agreeing a brief statement of work, and having CRU send a bill. If this is useful and possible, then can you check it out with Redacted?

0938018124

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

I am perfectly amenable to keeping Redacted's series in the plot, and can ask Redacted to add it to the plot he has been preparing (nobody liked my own color/plotting conventions so I've given up doing this myself). The key thing is making sure the series are vertically aligned in a reasonable way. **I had been using the entire 20th**

century, but in the case of Redacted's, we need to align the first half of the 20th century w/ the corresponding mean values of the other series, due to the late 20th century decline.

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

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

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

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

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

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

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

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

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

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

Looking forward to hearing back w/ comments,

Redacted

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

Hi everyone

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

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

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

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

0942777075

Once Tim's got a diagram here we'll send that either later today or first thing tomorrow.

I've just completed Redacted's Nature trick of adding in the real temps to each series for the last 20 years (ie from 1981 onwards) amd from 1961 for Redacted's to hide the decline. Redacted's series got the annual land and marine values while the other two got April-Sept for NH land N of 20N. **The latter two are real for 1999, while the estimate for 1999 for NH combined is +0.44C wrt 61-90. The Global estimate for 1999 with data through Oct is +0.35C cf. 0.57 for 1998.**

Thanks for the comments, Redacted.

1018647333's echoed eMail

Hi Redacted, Redacted, etc,

Okay, I am quite happy to give this debate a rest, although I am sure that the issues brought up will still be grounds for scientific debate. I admit to getting a bit riled when I saw the ECS results on the MWP described as "perilous" because I regard that as being an unfair characterization of the work presented. Be that as it may, my reply to Science will be very carefully worded so as not to inflame the issues. Nuff said. Have a good weekend. I certainly intend to do so.

Redacted

Redacted and others,

I thought I too should chime in here one last time...

I'll leave it to you, Redacted and others to debate out the issue of any additional uncertainties, biases, etc. that might arise from RCS in the presence of limited samples. That is beyond my range of expertise. But since this is a new and relatively untested approach, and it is on the basis of this approach that other estimates are being argued to be "underestimates", we would indeed have been remiss now to point this out in our letter.

The wording "perilous" perhaps should be changed, by I very much stand by the overall sentiment expressed by Redacted in our piece with regard to RCS.

One very important additional point that Redacted makes in his message is that conservative estimates of uncertainties, appropriate additional caveats, etc. were indeed all provided in MBH99, and I have always been careful to interpret our results in the context of these uncertainties and caveats. IPCC '2001 was careful to do so to, and based its conclusions within the context of the uncertainties (hence the choice of the conservative term "likely" in describing the apparently unprecedented nature of late 20th century warmth) and, moreover, on the collective results of many independent reconstructions. Briffa & Osborn would have you believe that IPCC '2001's conclusions in this regard rested on MBH99 alone. Frankly, Redacted, I believe that is unfair to the IPCC, whether or not one cares about being fair to MBH or not.

What is unfortunate here then is that Esper et al has been "spun" i to argue that MBH99 underestimates the quantity it purports to estimate, full Northern Hemisphere annual mean temperature. Given the readily acknowledged level of uncertainty in both estimates, combined with the "apples and oranges" nature of the comparison between the two (which I have sought to clarify in my letter to Science, and in my messages to you all, and the comparison plot I provided), I believe it is either sloppy or disingenuous reasoning to argue that this is the case. The fact that this sloppiness also readily serves the interests of the skeptics is quite unfortunate, but it is indeed beside the point!

It would probably also be helpful for me to point out, without naming names, that many of our most prominent colleagues in the climate research community, as well government funding agency representatives, have personally contacted me over the past few weeks to express their dismay at the way they believe this study was spun. I won't get into the blame game, because there's more than enough of that to go around. But when the leaders of our scientific research community and our funding managers personally alert us that they believe the credibility of our field has been damaged, I think it is time for some serious reflection on this episode.

that's my final 2 cents,

Redacted

At 10:21 AM 4/12/02 -0400, Redacted wrote:

Just a few comments here and then I'm done.

Dear Redacted and others, All of our attempts, so far, to estimate hemisphere-scale temperatures for the period around 1000 years ago are based on far fewer data than any of us would like. None of the datasets used so far has anything like the geographical distribution that experience with recent centuries indicates we need, and no-one has yet found a convincing way of validating the lower-frequency components of them against independent data. As Redacted wrote, in the tree-ring records that form the backbone of most of the published estimates, the problem of poor replication near the beginnings of records is particularly acute, and ubiquitous. I would suggest that this problem probably cuts in closer to 1600 than 1400 in the several published series. Therefore, I accept that **everything we are doing is preliminary, and should be treated with considerable caution.**

Therefore, I would guess that you would apply the word "perilous" to everyones' large-scale NH reconstructions covering the past 500-1000 years including those that you have been involved in. Why the sudden increase in caution now? It sounds very self-serving to me for you to call ECS "perilous" and not describe every other large-scale reconstruction in that way as well.

I differ from Redacted, and his co-authors, in believing that these problems have a special significance for the particular implementation of RCS they used, in the light of one of their conclusions that depends heavily on that implementation. As I understand what Redacted have written at various times about RCS, and from my own limited experience with the method, it is extremely important to have strong replication, and **I don't see 50-70 samples probably from 25-35 trees as a big sample.** For reference, most chronologies used in dendroclimatology are based on 10-40 trees, that is 20-80 samples at 2 cores per tree for a single "site", usually a few hectares. Here are two passages from Briffa et al., 1992: page 114, column 1, last paragraph, "For a chronology composed of the same number of samples, one would therefore expect a larger statistical uncertainty using this approach than in a chronology produced using standardization curves fitted to the data from individual trees...............The RCS method therefore requires greater chronology depth (i.e. greater sample replication) to provide the same level of confidence in its representation of the hypothetical "true" chronology." ECS mention this issue.

As I said in my previous email, we hid nothing in terms of the uncertainty concerning the pre-1200 interval. Are you suggesting that we should not have even shown those results? If so, that is ridiculous.

page 114, column 1, third paragraph, there is a discussion of the problems arising from applying RCS when pith age is not known, "In the ring-width data, the final standardization curve probably slightly underestimates the width of young trees and could therefore impart a small positive bias to the standardized ring-width indices for young rings in a number of series. However, this effect will be insignificant when the biased indices are realigned according to calendar growth years and averaged with many other series." The problem here is that this latter condition is not met (in my view), and the "small positive bias" that may be retained could turn out to be important to the most controversial conclusion of ECS (the Medieval question).

I can't speak for Redacted here, but most of the data he used came from Redacted's lab. I believe that pains were taken to estimate the pith offset and that Redacted used this information in his RCS analyses. Redacted would be best to comment here. In any case, Redacted has done a number of experiments in which he has artificially added large pith offset errors into the RCS analysis and the resulting bias is small. So, I do not believe that your "view" is correct.

I also suspect that Redacted and colleagues underestimated both the size and variability of the loss of years at the beginning of records, but the point stands even if this is not so. So far as I can see, ECS do not mention this issue, at least in the context of a possible positive bias.

Are you claiming that the only possible bias is positive? I can show you examples of a probable negative bias using RCS.

The discussion of RCS in the supplementary materials seems to assume good replication.

It was a generic description of the method. The replication is clearly shown in the supplementary materials section as well as in the main paper. If you don't like the replication, that is your opinion. I would love to have more replication as well. Who wouldn't. But we did show the uncertainties, which you seem to ignore in your criticism. Ironically, the ECS estimates of warmth in the MWP are not that dissimilar to those seen in MBH, as ECS Fig. 3 shows. Are the MBH estimates of MWP warmth also similarly biased?

ECS, as Redacted rightly points out, clearly indicate, in both words and diagrams at several points in their paper and in the supplementary materials, that the number of sites and number of samples they used decreases sharply before 1200. Even so, ECS gives prominence (second sentence of the abstract, for example) to the reconstruction in that very period, and makes a comparison with the magnitude of 20th-century warming. All the methods, and their realizations so far, have significant problems. In our letter (Redacted and I) we draw attention to a specific problem with this implementation of RCS that has a special bearing on the reconstruction of a period to which ECS have drawn attention. Hence the strong note of caution about the ECS conclusion on the comparison between the 10th/11th and late 20th centuries. I hope it's clear from this that I don't disagree with the general proposition that all existing reconstructions of hemipsphere-scale temperatures 1000 years ago (or even for all the first half of the second millennium AD) should be viewed as very preliminary. If anyone is interested I attach a short note on the replication in the year AD 1000 of records used in MBH99 to give an idea of what we are up against.

There is obviously a lot more we can debate about here. I will simply stop here by saying that I stand by the results shown in ECS and will say so in my reply to your letter, pointing out that the use of the word "perilous" could be just as easily be applied to MBH.

We all have a lot to do. I see four important tasks - 1) more investigation of the strengths and limitations of methods like RCS and age-banding - for example, how many samples would have been enough in this case, does the RC change through time? and so on; 2) use of tree-ring records where the loss of low-frequency information is least - those with long segments from open stands; 3) the search for tree-ring parameters without age/size related trend; 4) the development of completely independent proxies with intrinsically better low-frequency fidelity.
Cheers, Redacted
The Briffa et al reference is to the 1992 paper, Climate
Dynamics, 7:111-119

Hi Redacted,
OK--thanks for your response. I'll let Redacted respond to the technical issues regarding RC. I'm not really qualified to do so myself anyway. Your other points are well taken...
Cheers,
Redacted
At 12:09 PM 4/11/02 -0400, Redacted wrote:
Hi Redacted,
Thanks for the reply. I too do not want to see anything personal in our disagreements. It would be a shame if it got to that and it shouldn't. I don't think that the science we are talking about is sufficiently known yet to claim the "truth", which is why we are having some of our disagreements. I mainly wanted to clarify some issues relating to some criticisms of the ECS results that I thought were not totally fair. My biggest complaint is with Redacted's contribution to your letter because it really isn't fair to use such words as "perilous". ECS did not hide anything and the uncertainties are clearly indicated in EGS Figs. 2 and 3. So, you can make your own judgement. However, Redacted's opinion does not invalidate the ECS record. If Redacted's statement is correct, than ALL previous estimates of NH temperature over the past 1000 years are "perilous", especially before AD 1400 when the number of series available declines significantly in most records.
Redacted
Redacted,
It will take some time to digest these comments, but my initial response is one of some disappointment. **I will resist the temptation to make the letter to Science available to the others on this list, because of my**

fears of violating the embargo policy (I know examples of where doing so has led to Science retracting a piece form publication). So thanks for also resisting the temptation to do so...

But I must point out that the piece by Redacted and me is very similar in its content to the letter of clarification that you and I originally crafted to send to Science some weeks ago, before your co-author objected to your involvement! If there is no objection on your part, I'd be happy to send that to everyone, because it is not under consideration in Science (a quite unfortunate development, as far as I'm concerned). The only real change from that version is the discussion of the use of RCS. That is in large part Redacted's contribution, but I stand behind what Redacted says. **I think there are some real sins of omission with regard to the use of RCS too, and it would be an oversight on our part now to comment on these.**

Finally, with regard to the scaling issues, let me simply attach a plot which speaks more loudly than several pages possibly could The plot takes Epser et al (not smoothed, but the annual values) and scales it against the full Northern Hemisphere instrumental record 1856-1990 annual mean record, and compares against the entire 20th century instrumental record (1856-1999), as well as with MBH99 and its uncertainties.

Suppose that Esper et al is indeed representative of the fullNorthern Hemisphere annual mean, as MBH99 purports to be. To the extent that differences emerge between the two in assuming such a scaling, I interpret them as differences which exist due to the fact that the extratropical Northern Hemisphere series and full Northern Hemisphere series likely did not co-vary in the past the same way they co-vary in the 20th century (when both are driven predominantly, in a relative sense, by anthropogenic forcing, rather than natural forcing and internal variability). What the plot shows is quite remarkable. Scaled in this way, there is remarkably little difference between Esper et al and MBH99 in the first place (the two reconstructions are largely within the error estimates of MBH99!)!, but moreover, where they do differ, this could be explainable in terms of patterns of enhanced mid-latitude continental response that were discussed, for example, in Shindell et al (2001) in Science last December. So I think this plot says a lot. Its say that there are some statistically significant differences, but certainly no grounds to use Esper et al to contradict MBH99 or IPCC '2001 as, sadly, I believe at least one of the published pieces tacitly appears to want to do.

It is shame that such a plot, which I think is a far more meaningful comparison of the two records, was not shown in either Esper et al or the Briffa & Osborn commentary. I've always given the group of you adequate opportunity for commentary on anything we're about to publish in Nature or Science. I am saddened that many of my colleagues (and, I have always liked to think friends) didn't affort me the same opportunity before this all erupted in our face. It could have been easily avoided. But that's water under the bridge.

Finally, before any more back-and-forths on this, I want to make sure that everyone involved understands that none of this was in any way ever meant to be personal, at least not on my part (and if it ever has, at least on my part, seemed that way, than I offer my apologies--it was never intended that way). This is completely about the "science". To the extent that I (and/or others) feel that the science has been mis-represented in places, however, I personally will work very hard to make sure that a more balanced view is available to the community. Especially because the implications are so great in this case. This is what I sought to do w/ the NYT piece and my NPR interview, and that is what I've sought to do (and Redacted to, as far as I'm concerned) with the letter to Science. Being a bit sloppy w/ wording, and omission, etc. is something we're all guilty of at times. But I do consider it somewhat unforgivable when it is obvious how that sloppiness can be exploited. And you all know exactly what I'm talking about! So, in short, I think are some fundamental issues over which we're in disagreement, and where those exist, I will not shy away from pointing them out. But I hope that is not mis-interpreted as in any way personal.

I hope that suffices,

Redacted

p.s. It seemed like an omission to not cc in Redacted on this exchange, so I've done that. I hope nobody minds this addition...

At 10:57 AM 4/11/02 -0400, Redacted wrote:

Hi Redacted,

I have received the letter that you sent to Science and will respond to it here first in some detail and later in edited and condensed form in Science. Since much of what you comment and criticize on has been disseminated to a number of people in your (Redacted's) somewhat inflammatory earlier emails, I am also sending this lengthy reply out to everyone on that same email list, save those at Science. I hadn't responded in detail before, but do so now because your criticisms will soon be in the public domain. However, I am not attaching your letter to Science to this email since that is not yet in the public domain. It is up to you to send out your submitted letter to everyone if you wish.

I must say at the beginning that some parts of your letter to Science are as "flawed" as your claims about Esper et al. (hereafter ECS). The Briffa/Osborn perspectives piece points out an important scaling issue that indeed needs further examination. However, to claim as you do that they show that the ECS 40-year low-pass temperature reconstruction is "flawed" begs the question: "flawed" by how much? It is not at all clear that scaling the annually resolved RCS chronology to annually resolved instrumental temperatures first before smoothing is the correct way to do it. The ECS series was never created to examine annual, or even decadal, time-scale temperature variability. Rather, as was clearly indicated in the paper, it was created to show how one can preserve multi-centennial climate variability in certain long tree-ring records, as a refutation of Broecker's truly "flawed" essay. As ECS showed in their paper (Table 1), the high-frequency correlations with NH mean annual temperatures after 20-year high-pass filtering is only 0.15. That result was expected and it makes no meaningful difference if one uses only extra-tropical NH temperature data. So, while the amplitude of the temperature-scaled 40-year low-pass ECS series might be on the high end (but still plausible given the gridded borehole temperature record shown in Briffa/Osborn), scaling on the annually resolved data first would probably have the opposite effect of excessively reducing the amplitude. I am willing to accept an intermediate value, but probably not low enough to satisfy you. Really, the more important result from ECS is the enhanced pattern of multi-centennial variability in the NH extra-tropics over the past 1100 years. We can argue about the amplitude later, but the enhanced multi-centennial variability can not be easily dismissed. I should also point out, again, that you saw Fig. 3 in ECS BEFORE it was even submitted to Science and never pointed out the putative scaling "flaw" to me at that time.

With regards to the issue of the late 20th century warming, the fact that I did not include some reference to or plot of the up-to-date instrumental temperature data (cf. Briffa/Osborn) is what I regard as a "sin of omission". What I said was that the estimated temperatures during the MWP in ECS "approached" those in the 20th century portion of that record up to 1990. I don't consider the use of "approached" as an egregious overstatement. But I do agree with you that I should have been a bit more careful in my wording there. As you know, I have publicly stated that I never intended to imply that the MWP was as warm as the late 20th century (e.g., my New York Times interview). However, it is a bit of overkill to state twice in the closing sentences of the first two paragraphs of your letter that the ECS results do not refute the unprecedented late 20th century warming. I would suggest that once is enough.

ECS were also very clear about the extra-tropical nature of their data. So, what you say in your letter about the reduced amplitude in your series coming from the tropics, while perhaps worth pointing out again, is beating a dead horse. However, I must say that the "sin of omission" in the Briffa/Osborn piece concerning the series shown in their plot is a bit worrying. As they say in the data file of series used in their plot (and in Keith's April 5 email response to you), Briffa/Osborn only used your land temperature estimates north of 20 degrees and recalibrated the mean of those estimates to the same domain of land-only instrumental temperatures using the same calibration period for all of the other non-borehole series in the same way. I would have preferred it if they had used your data north of 30N to make the comparisons a bit more one-to-one. However, I still think that their results are interesting. In particular, they reproduce much of the reduced multi-centennial temperature variability seen in your complete NH reconstruction. So, if the amplitude of scaled ECS multi-centennial variability is far too high (as you would apparently suggest), it appears that it is also too low in your estimates for the NH extra-tropics north of 20N. I think that we have to stop being so aggressive in defending our series and try to understand the strengths and weaknesses of each in order to improve them. That is the way that science is supposed to work. I must admit to being really irritated over the criticism of the ECS tree-ring data standardized using the RCS method. First of all, ECS acknowledged up front the declining available data prior to 1200 and its possible effect on interpreting an MWP in the mean record. ECS also showed bootstrap confidence intervals for the mean of the RCS chronologies and showed where the chronologies drop out. Even allowing for the reduction in the number of represented sites before 1400 (ECS Fig. 2d), and the reduction in overall sample size (ECS Fig. 2b), there is still some evidence for significantly above average growth during two intervals that can be plausibly assigned to the MWP. Of course we would like to have had all 14 series cover the past 1000-1200 years. This doesn't mean that we can't usefully examine the data in the more weakly replicated intervals. In any case, the replication in the MWP of the ECS chronology is at least as good as in other published tree-ring estimates of large-scale temperatures (e.g., NH extra-tropical) covering the past 1000+ years. It also includes more long tree-ring records from the NH temperate latitudes than ever before. So to state that "this is a perilous basis for an estimate of temperature on such a large geographic scale" is disingenuous, especially when it is unclear how many millennia-long series are contributing the majority of the temperature information in the Mann/Bradley/Hughes (MBH) reconstruction prior to AD 1400. Let's be balanced here. I basically agree with the closing paragraph of your letter. The ECS record was NEVER intended to refute MBH. It was intended, first and foremost, to refute Broecker's essay in Science that unfairly attacked tree rings. To this extent, ECS succeeded very well. The comparison of ECS with MBH was a logical thing to do given that it has been accepted by the IPCC as the benchmark reconstruction of NH annual temperature variability and change over the past millennium. Several other papers have made similar comparisons between MBH and other even more geographically restricted estimates of past temperature. So, I don't apologize in the slightest for doing so in ECS. The correlations in Table 2 between ECS and MBH were primarily intended to demonstrate the probable large-scale, low-frequency temperature signal in ECS independent of explicitly calibrating the individual RCS chronologies before aggregating them. The results should actually have pleased you because, for the 20-200 year band, ECS and MBH have correlations of 0.60 to 0.68, depending on the period used. Given that ECS is based on a great deal of new data not used in MBH, this result validates to a reasonable degree the temperature signal in MBH in the 20-200 year band over the past 1000 years. Given the incendiary and sometimes quite rude emails that came out at the time when ECS and Briffa/Osborn were published, I could also go into the whole complaint about how the review process at Science was "flawed". I will only say that this is a very dangerous game to get into and complaints of this kind can easily cut both ways. I will submit an appropriately edited and condensed version of this reply to Science.

1054736277

Thanks Redacted, and Thanks Redacted for your willingness to help/sign on. This certainly gives us a "quorum" pending even a few possible additional signatories I'm waiting to hear back from.

In response to the queries, I will work on a draft today w/ references and two suggested figures, and will try to send on by this evening (east coast USA). Redacted indicated that he wouldn't be able look at a draft until Thursday anyway, so why doesn't everyone just take a day then to digest what I've provided and then get back to me with comments/changes (using word "track changes" if you like).

I'd like to tentatively propose to pass this along to Phil as the "official keeper" of the draft to finalize and submit IF it isn't in satisfactory shape by the time I have to leave (July 11--If I hadn't mentioned, I'm getting married, and then honeymoon, prior to IUGG in Sapporo--gone for about 1 month total). Redacted, does that sound ok to you?

Re Figures, what I had in mind were the following two figures:

1) A plot of various of the most reliable (in terms of strength of temperature signal and reliability of millennial-scale variability) regional proxy temperature reconstructions around the Northern Hemisphere that are available over the past 1-2 thousand years to convey the important point that warm and cold periods where highly regionally variable.

Redacted are probably in the best position to prepare this (?). Redacted and I have recently submitted a paper using about a dozen NH records that fit this category, and many of which are available nearly 2K back--I think that trying to adopt a timeframe of 2K, rather than the usual 1K, addresses a good earlier point that Redacted made w/ regard to the memo, that **it would be nice to try to "contain" the putative "MWP"**, even if we don't yet have a hemispheric mean reconstruction available that far back [Redacted and I have one in review--not sure it is kosher to show that yet though--I've put in an inquiry to Redacted at AGU about this]. If we wanted to be fancy, we could do this the way certain plots were presented in one of the past IPCC reports (was it 1990?) in which a spatial map was provided in the center (this would show the locations of the proxies), with "rays" radiating out to the top, sides, and bottom attached to rectanges showing the different timeseries. Its a bit of work, but would be a great way to convey both the spatial and temporal information at the same time.

2) A version of the now-familiar "spaghetti plot" showing the various reconstructions as well as model simulations for the NH over the past 1 (or maybe 2K). To give you an idea of what I have in mind, I'm attaching a Science piece I wrote last year that contains the same sort of plot.

However, what I'd like to do different here is:

In addition to the "multiproxy" reconstructions, I'd like to Add Redacted's maximum latewood density-based series, since it is entirely independent of the multiproxy series, but conveys the same basic message. I would also

like to try to extend the scope of the plot back to nearly 2K. This would be either w/ the Redacted extension (in review in GRL) or, if that is deemed not kosher, the Redacted Eurasian tree-ring composite that extends back about 2K, and, based on Redacted and my results, appears alone to give a reasonably accurate picture of the full hemispheric trend.
Thoughts, comments on any of this?
thanks all for the help,

1096382684

that is a useful way to look at it.

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

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

is that right?

1105019698's echoed eMail

There is a preference in the atmospheric observations chapter of IPCC AR4 to stay with the 1961-1990 normals. This is partly because a change of normals confuses users, e.g. anomalies will seem less positive than before if we change to newer normals, so the impression of global warming will be muted. Also we may wish to wait till there are 30 years of satellite data, i.e until we can compute 1981-2010 normals, which will then be globally complete for some parameters like sea surface temperature.

1107454306's echoed eMail

Thanks Redacted,
Yes, we've learned out lesson about FTP. We're going to be very careful in the future what gets put there. Redacted really screwed up big time when he established that directory so that Redacted could access the data.
Yeah, there is a freedom of information act in the U.S., and the contrarians are going to try to use it for all its worth. But there are also intellectual property rights issues, so it isn't clear how these sorts of things will play out ultimately in the U.S.
I saw the paleo draft (actually I saw an early version, and sent Redacted some minor comments). It looks very good at present--will be interesting to see how they deal w/ the contrarian criticisms--there will be many. I'm hoping they'll stand firm (I believe they will--I think the chapter has the right sort of personalities for that)...
Will keep you updated on stuff...
talk to you later,
Redacted
At 09:41 AM 2/2/2005, Redacted wrote:

Redacted,
I presume congratulations are in order - so congrats etc !
Just sent loads of station data to Redacted. Make sure he documents everything better this time ! **And don't leave stuff lying around on ftp sites - you never know who is trawling them. The two MMs have been after the CRU station data for years. If they ever hear there is a Freedom of Information Act now in the UK, I think I'll delete the file rather than send to anyone.** Does your similar act in the US force you to respond to enquiries within 20 days? - our does ! The UK works on precedents, so the first request will test it.
We also have a data protection act, which I will hide behind. Redacted has sent me a worried email when he heard about it - thought people could ask him for his model code. He has retired officially from

UEA so he can hide behind that. IPR should be relevant here, but I can see me getting into an argument with someone at UEA who'll say we must adhere to it !

Are you planning a complete reworking of your paleo series? Like to be involved if you are.

Had a quick look at Ch 6 on paleo of AR4. The MWP side bar references Briffa, Bradley, Mann, Jones, Crowley, Hughes, Diaz - oh and Lamb ! Looks OK, but I can't see it getting past all the stages in its present form. MM and SB get dismissed. All the right emphasis is there, but the wording on occasions will be crucial. I expect this to be the main contentious issue in AR4. I expect (hope) that the MSU one will fade away. It seems the more the CCSP (the thing Redacted is organizing) looks into Christy and Spencer's series, the more problems/issues they are finding. I might be on the NRC review panel, so will keep you informed.

Redacted is an LA on the Radiative Forcing chapter, so he's a paleo expert by GRL statndards.

Cheers

Redacted

At 13:41 02/02/2005, you wrote:

Redacted--thought I should let you know that its official now that I'll be moving to Penn State next Fall.

I'll be in the Meteorology Dept. & Earth and Environmental Systems Institute, and plan to head up a center for "Earth System History" within the institute. Will keep you updated,

1114607213

Redacted,

Presumably you've seen all this - the forwarded email from Redacted. I got this email from Redacted a few days ago. **As far as I'm concerned he has the data - sent ages ago. I'll tell him this, but that's all - no code. If I can find it, it is likely to be hundreds of lines of uncommented fortran !** I recall the program did a lot more that just average the series.

I know why he can't replicate the results early on - it is because there was a variance correction for fewer series.

See you in Bern.

Cheers

Redacted

Dear Redacted,

In keeping with the spirit of your suggestions to look at some of the other multiproxy publications, I've been looking at Jones et al [1998]. The methodology here is obviously more straightforward than MBH98. However, while I have been able to substantially emulate your calculations, I have been unable to do so exactly. The differences are larger in the early periods.

Since I have been unable to replicate the results exactly based on available materials, I would appreciate a copy of the actual data set used in Jones et al [1998] as well as the code used in these calculations.

There is an interesting article on replication by Anderson et al., some distinguished economists, here [1]http://research.stlouisfed.org/wp/2005/2005-014.pdf discussing the issue of replication in applied economics and referring favorably to our attempts in respect to MBH98.

1212063122

Can you delete any emails you may have had with Redacted re AR4? Redacted will do likewise. He's not in at the moment - minor family crisis.

Can you also email Redacted and get him to do the same? I don't have his new email address.

We will be getting Redacted to do likewise.

I see that CA claim they discovered the 1945 problem in the Nature paper!!

1254751382's echoed eMail

I concede all of your points but add one other thought. It is my grandchildren I worry about and I suspect their grand children will find it exceedingly warm because sunspots will return and carbon abatement is only a game; It wont happen significantly in their lifetime AND IT WONT BE ENOUGH IN ANY CASE. HENCE _WE WILL NEED A GEOENGINEERING SOLUTION_ COME WHAT MAY!

This is a large file in which the UEA people are keeping track and responding to a conversation between several people who are questioning their methods and data. It is a long and interesting conversation that seems to have occurred sometime in October of this year, though at least one of the correspondences is dated in May, 2009. It is worth reading.

The particular person making this statement is a well-known and accomplished physicist, not a climatologist. He sees a certain future, and feels that we must do whatever we need to do, including using geoengineering solutions, to overcome global warming. (He also seems to have an irrational fear of sunspots!)

(Geoengineering: "The modern concept of geoengineering is usually taken to mean proposals to deliberately manipulate the Earth's climate to counteract the effects of global warming from greenhouse gas emissions. The National Academy of Sciences defined geoengineering as 'options that would involve large-scale engineering of our environment in order to combat or counteract the effects of changes in atmospheric chemistry.'" - from Wikipedia)

You can see some of the Geoengineering proposals that have been made at http://www.msnbc.msn.com/id/30112396 and http://www.newsweek.com/id/71691

1254108338

Here are some speculations on correcting SSTs to partly explain the 1940s warming blip.

If you look at the attached plot you will see that the land also shows the 1940s blip (as I'm sure you know).

So, if we could reduce the ocean blip by, say, 0.15 degC, then this would be significant for the global mean -- but we'd still have to explain the land blip.

I've chosen 0.15 here deliberately. This still leaves an ocean blip, and i think one needs to have some form of ocean blip to explain the land blip (via either some common forcing, or ocean forcing land, or vice versa, or all of these). When you look at other blips, the land blips are 1.5 to 2 times (roughly) the ocean blips -- higher sensitivity plus thermal inertia effects. My 0.15 adjustment leaves things consistent with this, so you can see where I am coming from.

Removing ENSO does not affect this.

It would be good to remove at least part of the 1940s blip, but we are still left with "why the blip".

Let me go further. If you look at NH vs SH and the aerosol effect (qualitatively or with MAGICC) then with a reduced ocean blip we get continuous warming in the SH, and a cooling in the NH -- just as one would expect with mainly NH aerosols.

The other interesting thing is (as Redacted note -- from MAGICC) that the 1910-40 warming cannot be solar. The Sun can get at most 10% of this with Redacted solar, less with Redacted solar. So this may well be NADW, as Redacted and I noted in 1987 (and also Redacted later). A reduced SST blip in the 1940s makes the 1910-40 warming larger than the SH (which it currently is not) -- but not really enough.

So ... why was the SH so cold around 1910? Another SST problem? (SH/NH data also attached.)

This stuff is in a report I am writing for EPRI, so I'd appreciate any comments you (and Redacted) might have.

This is another one of those truly incredible smoking guns. This guy is explaining, right here, how he changed input data to get the output curves he wanted. So, we're not just looking at data mining; we're looking at the output of data mining, where the data-set itself is a lie!