

From: Ben Santer <bsanter@pcmdi.llnl.gov>
To: ritson@slac.stanford.edu, p.jones@uea.ac.uk, covey@cirrus.llnl.gov, tbarnett-ul@ucsd.edu, k.briffa@uea.ac.uk
Subject: (Fwd) Re: Your Holocene paper with Barnett et al 6.3 1996 page 255
Date: Tue, 14 Oct 1997 16:17:44 -0700

Dear Dr. Ritson,

Your email to Phil Jones suggests that there are serious discrepancies between the ECHAM1/LSG power spectrum that I computed for the 1995 Barnett et al. Holocene paper and the ECHAM1/LSG power spectrum that Curt Covey posted on the WWW. This is not the case. At the time that Tim Barnett, Phil Jones, Keith Briffa and I performed the research that is the subject of the Holocene paper, only 600 years of control run data were available from ECHAM1/LSG. This is stated on page 256 of the Holocene paper. The first ca. 200-250 years of this control integration incorporated a large, non-linear climate drift component. This was manifested both in globally-averaged temperature and in other climate variables (see Santer et al., 1995, JGR 100, 10,693-10,725).

Prior to computing the spectrum I removed the overall (i.e., 600-year) least-squares linear trend. There is still considerable low-frequency variance in the residuals, in part (but not wholly) due to the non-linearity of the drift component in the first few centuries. This residual drift explains some portion of the GFDL-versus-ECHAM1 power discrepancies at timescales of >100 years.

The CMIP project received data from MPI well after the completion of the research described in the Barnett et al. paper. At that time, I believe that 1,250 years of ECHAM1/LSG control run data were made available. My understanding is that Curt did not use the first (drift-contaminated) 250 years of the ECHAM1/LSG control run when he computed the ECHAM1 spectrum displayed on the CMIP WWW page. His analysis relied on the last 1,000 years of the data.

Not surprisingly, neglecting the first 250 years makes a big difference to the computed spectrum. This is particularly apparent at low frequencies, and also in the variance ratio (between periods of 300 and 2 years) that you compute.

I hope this clarifies things. Should you still have residual concerns about our method of spectral analysis (which is standard and follows Jenkins and Watts), I'd be happy to provide you with a copy of the program that was used to generate the spectra.

Sincerely,

Ben Santer

--- Forwarded mail from Phil Jones <p.jones@uea.ac.uk>

Date: Tue, 14 Oct 1997 10:42:29 +0100
To: ritson@slac.stanford.edu
From: Phil Jones <p.jones@uea.ac.uk>
Subject: Re: Your Holocene paper with Barnett et al 6.3 1996 page 255
Cc: bsanter@rainbow.llnl.gov

David,

I can only suggest you contact Ben Santer who did the analysis for Table 1. Ben is generally very busy - his email is bsanter@rainbow.llnl.gov .

Cheers
Phil

At 01:10 PM 10/13/97 -0700, you wrote:

> Two quick questions about your Fig 1, power spectrum of global mean
> averaged temperature.

>
>1) You don't provide units. I would have expected that

>
> <DT**2> Integral(G(f).df)

>
> would be the normalization with G(f) being the power spectrum and DT the
> RMS variance. Obviously this is not what you used. What are your units?

>
>2) I checked your ECHAM1 results for the ratio of the power spectrum at
> a period of 300 years to the value at 2 years against the posted CMIP
> LLNL power spectrum on the WWW. Aside from units the ratios of CMIP
> and yours appear to differ by a factor of the order of 6. As you are both
> using the same data base(?) and Curtis Covey of LLNL said he used Ben Santer's
> program for power spectra this discrepancy seems a little strange. Who is
> right

> or are you both right?

>
> I would check it myself in a matter of day(s) but getting model data bases
> is a bureaucratic nightmare.

>
> Dave

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

---End of forwarded mail from Phil Jones <p.jones@uea.ac.uk>