Clim. Past Discuss., 3, S611–S616, 2007
www.clim-past-discuss.net/3/S611/2007/

Climate of the Past Discussions

Interactive comment on "How unusual was autumn 2006 in Europe?" *by* G. J. van Oldenborgh

G. J. van Oldenborgh

Received and published: 5 October 2007

Thank you for the detailed review. I will try to address the points in the order they were raised. If I have some spare time after doing the revisions of this paper I will attempt a similar analysis of winter 2006/2007.

The reviewer comments that there should be more explanation and discussion. The paper has been expanded to make it easier to read. As for the number of datasets, I have dropped the low-resolution HadCRUT3 dataset. All plots have been re-made with the latest version of the GHCN/CAMS dataset. I consider the model material at the end essential to the argument that a change in the tail of the distribution is not foreseen by the current climate models.

The circulation patterns were extraordinary, but how does one quantify a return period for a 2D circulation field?

One way to do this is to compute a 'specified temperature', which is a pointwise 0-

dimensional projection of exactly those aspects of the circulation that are relevant for the high temperature (in a linear approach). This quantity can easily be subjected to extreme value analysis. This is exactly the track chosen in this analysis. I have made this point clearer.

I also added some extreme value statistics on circulation directly, and also trends.

1) Quantifying how rare fall 2006 was

This has been clarified somewhat.

2) Extrapolating a pdf (there are two fits in some figures, but not even explained or discussed)

A short discussion has been added.

3) Dealing with data sets such as they are (CRU at low res, GHCN/CAMS at high-res), some gridded&analysed, some at stations (De Bilt)

The CRU dataset has been dropped.

4) Comparing to a proxy data set (this could be dropped; fig.2)

This figure has been dropped.

5) Supposed errors in data sets and how one deals with that (De Bilt, spots in GHCN/CAMS)

Mention has been made, but dealing with this completely falls outside the scope of the paper.

6) The concepts behind Eq(1), relatively easy

OK

7) Concepts behind Eq 2-4, quite involved, debatable and ad hoc

The VSM has been reverted back to the original formulation with vorticity instead of

CPD

3, S611–S616, 2007

solar radiation. This term adds to the explained variance as a proxy for cloud cover.

8) Model simulations (Great work, but delete! To stay focused) Have mercy with the reader! Any of the above gives a reader reason to pause.

I would prefer to keep the model results, in order to make the point that the increase in the tail of the PDF that can be deduced from the observations is not simulated by present-day climate models.

- With respect to the 2nd point...given by how much 2006 was warmer than 2005, and climate cannot have changed that much in one year, the conclusion that the circulation is the culprit (as a generality) is quite obvious.

The point I wanted to make is that the PDF of autumn temperatures appears to have shifted to have a stronger positive tail. If one does not assume that, the observations do not make sense. Autumn 2005 (and probably autumn 2007) did not fall in this tail, by chance. The weather remains capricious.

Specific comments:

-) In some places De Bilt specifically is meant, where The Netherlands is written.

Corrected.

-) Wasn't the previous record for fall a tie between 2005 and 1731?

If you round to 0.1 degree, yes. Amended.

-) Fits in graphs are given but not discussed, fig.3. What is gpd?

Added a short discussion and reference.

-) While inhomogeneities in station data lead to spots, not all spots are due to a lack of QC in the original station data. There are true small scale effects, due to mesoscale circulation, land surface inhomogeneities (themselves changing in space and time), lakes, coastlines and orography. When GHCN/CAMS is thinned out to only supposedly clean

CPD
3, S611–S616, 2007

EGU

GHCN we still find spots. Let's not implicitly impose here a mandatory temperature increase as quality control, that way we accept/reject data according to preconceived notions.

In principle I totally agree. In practice the question of representativeness is a slippery one. At which scale does one call a change in the surroundings a change in climate rather than an inhomogeneity? A change influencing temperatures over 100m is a measurement problem, a change influencing temperatures over 100km a local climate change, but in between there is a grey area in which many of the effects mentioned by the reviewer operate..

To come back to the plots; in the version I used for the plots there were definitely signs of inhomogeneities of the trivial variety. I have redone all plots with the version of October 1, 2007.

-) If CRU is better, why is it not shown at .5 degree res??? Is this the commercial issue? Or because it will take 10 years (if ever) before they have enough data to analyze fall 2006 in that sort of detail.

At the moment the 0.5 degree version of the CRU dataset is only available up to 2002. The GHCN/CAMS dataset is unique in its resolution and timeliness.

-) Eq(1) does not just describe a rise, as stated. The flattening (or decrease of temps in the 1970s) is part of the data underlying A in (1).

OK, adjusted.

-) Cannot truly remove in full climate change via (1) so as to arrive at a residue called weather. The damped nature of regression will make the residue most often of the same sign as the observed anomaly.

I disagree. As long as the uncertainty on the global temperature is much smaller than the residuals, the regression by definition gives rise to residuals that are uncorrelated with the global mean temperature. The regression is damped only when the neglected

CPD

3, S611–S616, 2007

errors on the x-coordinate are large. This is not the case here.

-) Maybe remind us where and exactly how (1) relates to (2)-(4)? Wasn't there a three year running mean somewhere here??? (another twist)

Made more clear.

-) I would also point out that the correlation of the predictors themselves (u and v are correlated, momentum goes poleward, solar rad is determined by circulation etc) is a problem in uniquely determining B, M and A_w and A_s from (limited) data.

The correlations among the circulation indices and between the circulation indices and the rest are typically small ($r < 0.5$ in De Bilt). The main problem is the correlation between the temperature in the previous month and global warming.

-) Where do you find (believable) data to determine B?

I have reverted back to the original formulation, using vorticity. The explained variance is almost identical to the formulation using solar radiation as estimated by the NCEP/NCAR reanalysis, but it is easier to defend, and easier to extend to per-1948 data.

-) I would not include that SW radiation term at all (unless the solar output varies a lot). That term looks even clumsy. An atmosphere full of fronts (clear cold air; cloudy warm air) is not suitable for that approach.

See above.

-) (2)-(4) explain limited amount of variance. The implications for the final conclusions are??? Keep in mind the damping issue.

Adjusted the conclusions to explicitly mention that the model does not describe all of the warming, and nonlinear terms are important as well.

-) Maybe the author should make a case for a very high-res coastal and inland sea

CPD
3, S611–S616, 2007

(Baltic, Mediterranean) SST analysis all over Europe so we could include the effect of July (and history) on SON???

The resolution of the dataset is not the problem. The fact that August was cold cannot be simulated with the simple AR(1) memory model in (1) or (2)-(4). The proper way to do this study would be to run a regional climate model with observed circulation and prescribed SST. This has been done for precipitation in August (Lenderink et al, submitted to Clim. Dyn., http://www.knmi.nl/samenw/regioklim/science/Lenderink_Aug2006_ClimDyn200709submitted.pdf), but not yet for temperature a month later. It must be admitted that Lenderink also complains that he does not have high-resolution North Sea SST.

-) The area of negative in M (9d) is such a minor detail in a paper like this. The interpretation (which may be correct) does not make sense without more discussion.

Deleted.

-) Is Fig.12 truly that exceptional in terms of SLP anomalies?, enough to shatter the previous record. Give us some standard deviations and so on. This relates very much to this paper.

Added lines indicating the number of standard deviations this is from the mean.

Interactive comment on Clim. Past Discuss., 3, 811, 2007.