**Climate
of the Past
Discussions**

Interactive
Comment

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

Anonymous Referee #3

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{{ I am new to this e-journal, its standards and procedures, and what is expected from me. Here are my first impressions.}}

Since the super extreme nature of the fall 2006 anomaly is among the most important natural phenomena (with an ecological/societal impact), any attempt to clarify why we had such a large anomaly is very VERY important and laudable. Most of the results appear trustworthy (in spite of my many questions - that only shows I am interested) so I would definitely publish. And encourage the same analysis for winter and spring 2006-2007 because they too were equally extreme in terms of historical pdf.

Maybe there are two weak points.

The first is that there are too many different approaches, data sets etc for so short an article. Related to this is a very low text to figures ratio. Put another way: many aspects would need more explanation and discussion to be fully appreciated, including more

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complete figure legends. I suggest deleting some material, for instance the model stuff at the end (does not address the fall 2006 directly).

The 2nd weakness is that in the end we hear that the circulation is the main culprit. I don't doubt this, but since this has always been true the conclusion does not seem very deep. Moreover the paper fails to show that the circulation itself was truly extraordinary in fall 2006. Was it??? Return times for circulation may be harder to imagine or quantify (than for T), let alone that we know how to adjust a time series of 'circulation' for global change (like the A term for T). Nevertheless the author needs to take us where few have gone before. Be our guide, we are all new to this.

With respect to the first point I see the following main issues:

1) Quantifying how rare fall 2006 was

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

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)

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

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

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

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

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.

Don't keep running.

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.

Specific comments:

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

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

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

-) 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 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.

-) 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.

-) 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).

-) 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.

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

-) 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

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problem in uniquely determining B, M and Aw and As from (limited) data.

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

-) 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.

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

-) Maybe the author should make a case for a very high-res coastal and inland sea (Baltic, Mediteranean) SST analysis all over Europe so we could include the effect of July (and history) on SON???

-) 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.

-) 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.

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

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