

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

Anonymous Referee #2

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Review of "How unusual was autumn 2006 in Europe?" by G.J. van Oldenborgh for Climate of the Past Discussions.

General comments

This paper investigates a specific but very relevant event in the current climate change debate: the unusually warm boreal autumn 2006. First off, I must admit this was the first time I came across this journal Climate of the Past along with its sibling Climate of the Past Discussions - I would like to congratulate the people behind it on this forward looking, full of potential web-based initiative - so I'm not entirely sure whether the topic of this paper fits in the framework of the journal. But, is 2006 considered past enough?

I found the topic of the paper very interesting but I think there are several weak points in its presentation. I'll summarise them here and give more specific comments below (I'm assuming the length of the paper is OK):

- * The added value of the paper is not immediately obvious
- * The conclusion does not appear to be particularly deep nor exciting
- * There are far too many leads for such a short paper - a more focussed approach may be beneficial - but then this would very much depend on what your target audience is.
- * Some of the arguments put forward in the paper may be controversial
- * The paper contains considerable amount of redundant information
- * Related to the previous bullet, the paper contains far too much visual material
- * Some parts of the paper could be omitted

In order to gain more insight into the causes of the anomalous event - which is really what the paper is about according to the abstract and the conclusions - I would have found more interesting, and also possibly more appropriate for this journal, a comparison with other autumn events. The previous 2005, for instance, was also particularly warm with several places in Northern Europe with more than two standard deviations above a long term mean (close to the 1961-1990 used in the paper). However, the circulation in 2005 was very different: there was an anomalous high pressure over Siberia and as a consequence that is where the high temperatures were centred - this paper prompted me into some investigations! Even 2004 (also on the warm side) and previous years could have been taken into consideration to better understand the link between global warming and circulation.

In so doing, generic comments such as "implying that it either was a very rare coincidence or some non-linear physics is missing from these (climate) models", not really backed up by the paper, would have been more defensible.

Specific comments:

For simplicity, the specific comments below follow the sequence of the paper rather than addressing the above bullets one by one. Also, not all of the specific comments

below address a specific bullet.

Abstract

Page 812, line 2 - It would be useful to know why the 1961-1990 period was chosen and what difference it would make if the more recent period was included.

Page 812, line 10 - Here and in several other instances in the paper I found very disturbing the expression "under the obviously false assumption". First, I would be generally very cautious with the use of the adverb "obviously" - it certainly doesn't apply in this case. Then, in statistics this concept is normally referred to as "under the null hypothesis" and I would urge the author to adopt this more conventional expression.

Page 812, line 16-18 - I am not convinced the analysis provided sheds enough light on the causes of the anomalous event as most of the listed causes are in fact inter-dependent or too generic. How should we interpret "global warming": is it just increased green-house-gases? Then, global warming and southern circulation may well be related. Again, southern circulation and more sunshine may also be related. Similarly for SST anomalies and circulation or sunshine. The difficulty to draw a clearer line between these causes ultimately is related to the assumptions used to build the very simple model used in the paper (see comments later).

Page 812, line 18-19 - The claim "Climate models that simulate current circulation well" should be better qualified.

1. Introduction

Page 812, line 23-24 - In the Netherlands should read De Bilt. The paper should really distinguish between small scale features from larger ones. In particular, I think that the De Bilt timeseries should be just employed to explain the method for computing the return time, which is the used for the larger scale. And the two things should be clearly separate: the discussion of the severity of the autumn event and the technicalities of the method.

Page 812, line 24 - In the paper it is hardly ever defined at what level the temperature refers to - I assume it is the standard 2m temperature.

Page 812, line 25 - It is hard to read numbers from figure 1 - horizontal lines would have helped. Also, it should have been clarified if figure 1a or 1b is meant when referring to just figure 1.

Page 813, line 1 - No account of uncertainty is given so it is not possible to verify this sentence.

Page 813, line 3-5 - I have no access to Luterbacher et al (2007) but the title of their paper resonates very much with this paper. What is the added value of this paper?

Page 813, line 5 - The legend of figure 2 is not entirely clear.

Page 813, line 9 - It would be useful to comment also on the negative values over Finland, Eastern/South East Europe and the Middle East.

Page 813, line 13 - This remark sounds out of place as is.

2. Observations

Page 814, line 5 - What is "gpd" in figure 3? This is never discussed in the paper so why is it in the figure?

Page 814, line 6 - The approach used to compute return times heavily relies on the normality of the distribution. It is not obvious that the Gaussian distribution is the best choice. Several other possibilities are available and some discussion to this effect should be included along with a proof of normality if this turned out to be the preferred option.

Page 814, line 7-8 - "The return times ... time scale". This remark needs to be better explained.

Page 814, line 8 - The remark that "Global warming has made high temperatures much

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more likely during recent years" sounds tautological to me! The whole sentence could do with some re-thinking.

Page 814, line 15-17 - This may be just a matter of resolution: how would GHCN/CAMS look if averaged over 5x5? In any case, do we really need two very similar datasets?

Page 814, line 22 - This shift sounds more like a red herring to me.

3. Global warming

Page 815, eqn (1) - Has the autocorrelation in the timeseries been taken into account when computing this regression? This is a very crucial point as results could be heavily affected by not considering temporal dependency in the series. Also, no estimate of uncertainty of A is given nor are the values of epsilon shown. Finally, global warming may not be (approximately) linear. In all, too many hidden assumptions underlie this approach and thus it is very difficult to assess its robustness.

Page 815, line 13 - "These are by definition not linearly related to global warming". This sentence doesn't say much: it excludes one possibility and leaves open many more alternatives.

4. Circulation

Page 816, line 2 - I look very favourably to simplified models but, despite its appeal, I think this very simple model (VSM) bears too many unjustified assumptions. For instance, on what basis circulation anomalies and sunshine can be separated? More importantly, these equations assume that the circulation is unaffected by global warming when it is argued that anomalous events are more likely under global warming. If one wants to try to explain the reason for this anomalous event surely it is not very helpful to start off with this built-in assumption, especially when the cause could be exactly the one discarded in the first place.

As for eqn (1) no indication of impact of autocorrelation and magnitude of uncertainty is given. Without this further information I don't think the results in figure 10 can be fully

appreciated.

Page 816, line 14 - A clearer explanation of how M is computed would have been helpful.

Page 816, line 24 - Why are the coefficients not shown over the sea in figure 9?

Page 817, line 4-5 - I am not sure I understood this sentence.

Page 817, line 15-16 - What is it meant by "direction with the highest temperature"?

5. Climate model simulations

I am not sure this section is necessary. It doesn't add much (its conclusions are far from conclusive) and if at all it detracts from the main aim of trying to understand the causes of the anomalous autumn 2006.

Page 818, line 10 - What does it mean "reasonably well"?

Page 818, line 22 - Should this be "underestimate"?

The message of figures 14 and 15 is not particularly evident.

6. Conclusions

The conclusions are very weak and somewhat disappointing. Sentences such as "... persistent southerly wind direction advecting warm air to the north, more sunshine than normal ..." are not particularly illuminating especially when this should be one the main conclusions and it could be drawn by just looking at a couple of easily produced plots (e.g., mean sea level pressure anomaly for SON).

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