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Climate of the Past Discussions



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

**G. J. van Oldenborgh**

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*Is 2006 considered past enough for this journal?*

I was told that the charter even goes up climate projections, although I assume few authors would submit these kind of papers to 'Climate of the Past'.

*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 three main points of the paper have not been discussed anywhere else as far as I am aware (please send me the references if I have missed papers): the anomalies were very improbable even after linearly accounting for the shift in the PDF due to global warming; a quantitative estimate of the effect of the anomalous circulation, and the fact that climate models both underestimate the trend and do not show a change in shape of the PDF and hence do not increase the probability of such an event occurring over the simple linear statistical analysis.

I have rewritten the abstract and conclusions to make this more clear.

** The conclusion does not appear to be particularly deep nor exciting*

See above.

** There are far too many leads for such a short paper - a more focused approach may be beneficial - but then this would very much depend on what your target audience is.*

I agree that I put too much into it and have not written it very clearly. In the revised version some side lines have been cut; the figures of the CRU dataset have been left out and the exposition has been improved.

** Some of the arguments put forward in the paper may be controversial*

I have tried to make the best quantitative arguments, including error estimates on all main results. (An error in the calculation of uncertainty estimates on the return periods was pointed out by reviewer 1 and has been corrected.) If the arguments are controversial because they are incorrect, I would like to see a similarly constructed counter-argument. If they are controversial because they contradict the ideas we had prior to the occurrence of this event, it is the purpose of the paper.

** The paper contains considerable amount of redundant information*

I have deleted the main source of redundancy, the usage of two temperature datasets, by not showing the results in the CRU dataset any more.

** Related to the previous bullet, the paper contains far too much visual material*

See above.

** Some parts of the paper could be omitted*



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I have omitted a few minor points. However, I insist on the inclusion of the model results, as they form an essential part of the second argument.

*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.*

Previous autumn temperatures in Europe fall well within the range expected from natural variability, with return times of less than 250 years without taking global warming into account and less than 25 years in Scandinavia when linearly subtracting the trend up to 2004, 100 years in Murmansk. Given four seasons per year and many grid points on the map, this happens all the time. A $2\sigma$ signal like that does not force a rethink of the way the PDF may be changing like the $5\sigma$ signal in 2006 did.

*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 defendable.*

I have shown that after accounting for the linear shift in PDF due to global warming, the best estimate of the return time is more than 600 years, with a lower bound of 125 years which is only reached if the climate models underestimate the warming trend by a factor more than two. These same models show no evidence for a trend to a more positively skewed distribution as the earth warms. I think this is enough to back up that sentence, and have tried to make the arguments more clear.

*Specific comments*

*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.*

The 1961-90 period was used for convenience as it is the WMO standard and many data (such as the HadCRUT3 global mean temperature) are given in this form. Choosing another period would not make any difference to the analysis, although the actual temperature anomalies would be somewhat different.

*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.*

Aristotle already argued in his Meteorologica that climate changes all the time. Variability exists on all kinds of time scales, so assuming that the climate variability on longer scales does not exist is very uncontroversial in general. The recent rise in temperature has been attributed to global warming in specific in numerous detection and attribution studies, down to the scale of continents (Stott, GRL, 2003) and smaller regions (Zhang, Zwiers, Stott, J. Climate, 2006). Another approach is to assume the detection and attribution has been performed at a large scale, and then connect this with local temperature changes (Turnpenny et al, Climate Research, 2003; van Oldenborgh and van Ulden, Int. J. Climatology, 2003).

In my experience the statistical usage of the phrase 'null-hypothesis' often causes confusion among non-statisticians. I did define the null-hypothesis better in section 2.

*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 interdependent 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).*

I have replaced most occurrences of ‘global warming’ by ‘global temperature changes’, as the model does not make any distinction to the causes, and global warming usually only refers to the last decades of anthropogenic changes.

I did ascertain that there are no trends in the circulation statistics mentioned her, either in the observations up to now, nor in climate model projections of future climate. These results have been described in the text, a more complete analysis will be submitted soon in another paper.

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

This is explained in detail in van Ulden and van Oldenborgh (Atm. Chem. Phys., 2006). I have added added explanation in the text (though not in the abstract).

*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 time series 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.*

Added the qualifier ’at De Bilt’. My colleagues are working on a well-defined Central Netherlands time series. In the meantime, the De Bilt series is the best approximation



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we have. The Netherlands are small enough that the monthly means of the whole country are approximated very well by this time series, at the level of accuracy needed for this paper.

Looking at the larger scales, the correlation between the $5 \times 5°$ grid box in CRUTEM3 that includes De Bilt is correlated $r = 0.97$ with the (non-homogenised) De Bilt observations. I therefore suspect that this grid box is largely taken from this De Bilt series, negating the difference between large-scale grid boxes and point measurements.

The De Bilt time series has been used in order to demonstrate the techniques, but also because it has been homogenised to some extend, increasing my confidence in the results.

*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.*

Yes, added to the text.

*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.*

Added horizontal lines; the references have been fixed.

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

Added the uncertainty and reference to the text.

*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?*

Luterbacher et al only state that the autumn was exceptionally warm. They compare it to a reconstructed time series, and conclude it was extremely likely the warmest one in 500 years, and combined with the winter the closest analogue was in 128/90. They



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complete this with a discussion of phenological effects.

Topics addressed in his paper that were not addressed by the letter of Luterbacher et al are a quantitative discussion of the return time, the return time assuming local warming that is linearly related to global warming, a quantitave discussion of the factors underlying the extreme, and climate model estimates of the changes in the PDF.

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

At the request of the other reviewers this figure has been deleted.

*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.*

[One would expect that some temperatures would be below the highest observed over 500 years.]

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

It was the only impact I could find.

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

Added a short description of the Generalised Pareto Distribution to the text.

*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.*

Two choices are shown and discussed (somewhat more extensively in the revised text), the normal distribution and the GPD. The skewness is negative, hence using the normal distribution underestimates the return times. This was shown by the comparison



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with the GPD extrapolation, which may not be in the asymptotic regime required at 80% but does show higher values in spite of the larger statistical error resulting from the lower number of points available over the threshold (21).

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

OK.

*Page 814, line 8 - The remark that "Global warming has made high temperatures much more likely during recent years" sounds tautological to me! The whole sentence could do with some re-thinking.*

When one shifts a PDF over a small amount compared to the width, the effect is largest in the changes in probability of the tails. Clarified in the text.

*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?*

I have deleted all figures with CRUTEM3, and added one sentence that the results have been checked against that dataset.

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

For a given anomaly, the return time depends strongly on the width of the distribution. Near the Atlantic the standard deviation of temperature is much lower than on the continent, hence the same anomaly has a larger return time, or conversely, one needs a much stronger anomaly in eastern Europe to obtain the same return time.

*Page 815, eqn (1) - Has the autocorrelation in the time series 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.*

The autocorrelation of the remaining whether noise $\epsilon(t)$ is effectively zero (slightly negative). This has been added in the text. The uncertainty of $A$ in De Bilt has been added to the text, it is indicated in the map by leaving all areas with $A$ compatible with zero at 95% blank.

In climate models local temperature scales linearly with global mean temperature. Over the observed record up to 2005 one cannot disprove this. The purpose of this paper is to show that this linear relationship does not seem to hold as well in the observations as it does in the climate models. This has been made more explicit in the text.

*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.*

It was meant as a reminder of Eq. 1 only; deleted.

*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.*

Sunshine has been replaced by vorticity in the revised paper, as it is more homogenous. It adds to the explained variance, hence some part of it is independent of the geostrophic wind.

As stated above, I have checked that there are no trends in these quantities in autumn in the observations.

*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.*

A discussion of the autocorrelation has been added. I am not sure which magnitude of uncertainty could be given beyond Fig. 10 itself, which states that Eqs (2)-(4) explain about half of the variance.

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

$M$ is fitted simultaneously with the other coefficients.

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

Because the dataset(s) used only contain land temperatures. SST has a much larger memory term $M$, so that using the HadCRUT3 dataset that merges SST and land temperatures gives strange results wherever these two are averaged in one grid box, as is the case in a large part of the area of the anomaly due to the large grid box size. I therefore decided to restrict myself to land temperatures.

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

Has been deleted.

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

Clarified to 'advecting the highest temperatures'.

*I am not sure the climate model simulations 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.*

It supports the second argument of the paper, that although the PDF seems to be changing, climate models both underestimate both the shift of the PDF and do not



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show a change in the shape.

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

The definition used in van Ulden and van Oldenborgh (2006) is based on the explained spatial variance of the climatological sea-level pressure fields $E = 1 - \sigma^2_{\mathrm{diff}}/\sigma^2_{\mathrm{obs}}$, where $\sigma^2_{\mathrm{diff}}$ is the spatial variance of the difference between the model climatology and observations, and $\sigma^2_{\mathrm{obs}}$ the spatial variance of these observations. We next demanded that in each month $E > 0$, i.e., the difference between the modeled and observed patterns was no larger than the observed patterns themselves. This gave the list used in his paper, plus lower-resolution versions of the same models.

Updated the text to include the measure used to rank the models.

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

No. The modeled increase is much faster than the observed rise in temperature. It is a weak point of the 4AR that this is hidden behind grey areas around a multi-model mean. These plots can easily be made at the KNMI Climate Explorer (climexp.knmi.nl) except for the MIROC model, for which I do not have permission to redistribute the data.

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

They purport to show that the shape of the PDF of the temperatures in the Netherlands does not change as it shifts to higher values in the models. Combined with the lower than observed model trends his means that the climate models do not offer an explanation for the observed temperature anomalies.

*The conclusions are very weak and somewhat disappointing. Sentences such as "... persistent southerly wind direction advecting warm air to the nor th, 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).*



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The conclusions have been reformulated to show more clearly what is new in this study:

1. Even when taking a shift of the PDF into account the autumn was very unusual; the warm temperatures cannot be simply explained by a uniform warming trend unless this trend is much higher than we thought.

2. Most of the anomalies were due to linear effects of advection.

3. Climate models simulate a lower trend than observed and no change in shape of this PDF, so that they do not give an increase in the probability of extremely warm events over the simple linear statistical analysis.

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