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Comment

Interactive comment on “Understanding and forecasting polar stratospheric variability with statistical models” by C. Blume and K. Matthes

Anonymous Referee #1

Received and published: 31 March 2012

Summary

The overall aim of this paper to show that polar stratospheric variability can be forecast to a reasonable extent. The authors make use of 4 different statistical models in an attempt to forecast the winter of 2011/2012. In doing so they predict a sudden stratospheric warming in Jan/Feb.

The paper is a natural progression on current linear techniques, and expands significantly on these that after minor corrections I would recommend it for publication.

Corrections

1. The manuscript attempts to predict an SSW event, when I feel they should refer to this as extreme polar variability, as at not point to they explicitly diagnose SSWs.

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This would also clarify that the prediction of an SSW event in Jan/Feb was actually (partially) correct by the authors. The vortex remained very weak over this period, and in my view was well predicted in this paper. 2. I would include a new figure showing each of the external factors separately (as timeseries). In this way readers will get more of a feel for how they vary. I for instance would be unsure of how the PDO changes on seasonal timescales. I would also show these timeseries before they are normalised. Another note on this point is that are all the external factors needed? For instance TROP2 seems to add little. Perhaps the authors can comment on whether certain forcings could be excluded is computation time became an issue, as they suggest might happen in some cases. 3. Linked to the above comment, the paper has too many acronyms, which are not so easy to remember all at once. If less external factors are used (as in my comment above) then this issue may go away. However if not I would do 1 of 2 things. 1. Add an acronym table at the end of the paper, and state its existence at the beginning. 2. Make the names most obvious, for instance TROP1 could be BLOCKING1. This will make the readability of the paper better. 4. I was surprised to read that the factors representing blocking seemed so insignificant (P10, line 25), especially considering the current literature on the subject. Perhaps the authors could expand on this. Also, how about trying another blocking metric, such as theta on PV2?

Technical Corrections

1. P2, Line 9, please delete etc. 2. P2, Line 20, please change the word 'tremendously'. 3. P2, Line 21, please change propagate to descend, as it is not obvious that anything propagates to the surface (still an open debate). 4. P3, Line 10, please change 'work made by' to 'study of'. 5. P4, Line 3, please expand on what 'trained' means to help guide non-statisticians. 6. P4, line 10, include '(hereafter ERA)' after ERA-Interim.. 7. P4, line 26, spelling: Tompson – Thompson. 8. P5, I would add an appendix explaining how e.g. the AMO is calculated (and other external factors). Or at least point to a paper which looks at most of the forcings (if one such paper exists). 9. P6, line 5, please change 'are sent'. 10. Please expand a little more on Blume et

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al. 2011 as it seems very relevant to this study. 11. I would give more detail about the MLP as it seems more important. I also wonder if any references could be given in section 3. I know of few which deal explicitly with the models mentioned and with the stratosphere, but perhaps Hannachi et al, 2011 and Coughlin and Gray, 2009, both of which used clustering techniques. 12. P8 – last sentence. Considering the size of the 2009 SSW, do you really think it can be explained just in terms of internal variability? 13. P13, line 21. I would suggest that volcanoes do lead to a colder, more stable vortex (depending on what latitude the eruption occurred at. Is the issue here not just a sample size, and the idea of truly random events?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 5659, 2012.

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