

Interactive comment on “The Arctic vortex in March 2011: a dynamical perspective” by M. M. Hurwitz et al.

Anonymous Referee #2

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This paper examines the correlation of the unusually low temperatures in March 2011 (and in March 1997, the only previous year on record with a cold period lingering similarly late into spring) with several phenomena (ENSO, QBO, sea surface temperatures) that have been previously shown to have an effect on lower-stratospheric temperatures. The analysis is sound and interesting, as far as it goes, and is suitable for publication in ACP after some minor revisions/clarifications. The main limitation I see in this paper is in that "as far as it goes", i.e., it doesn't go very far. Why only examine March, when the 2010/2011 winter really appears much more anomalous when you consider the entire winter? Why examine only an average over a calendar month in which the temperatures were changing rapidly – what makes that an appropriate averaging period? And isn't the state of the lower stratosphere (vortex strength/structure, temperatures) earlier in the winter relevant to how the other processes examined may affect it by March?

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How well does classifying whole winters as QBO-easterly or westerly, or as having or not having a La Nina condition really characterize the potential influence of those phenomena on a single month? I'm not suggesting that the authors need to address all of these questions in a major revision, but it would be good to at least acknowledge the scope of the study and put it in the context of the larger picture.

Specific comments:

Page 22114, line 19 to Page 22115, line 2: The authors couldn't have known this before, but now that it is out, Manney et al, 2011, Nature would be an excellent reference for the unprecedented Arctic ozone loss in 2011.

Page 22115, lines 6-13: I think it would be useful in this paragraph to note explicitly that these are statistical relationships, that is, they don't imply that, eg, a cold vortex will *always* be associated with a westerly QBO phase or an El Nino condition.

Page 22115, lines 14-21: This would be a good place to give some of the context mentioned in the general comment above.

Page 22115, line 27– 22116, line 12: The NCEP-2 reanalysis has been shown to a poor choice for studies of the polar lower stratosphere (for any stratospheric studies for that matter). The NCEP-1 and NCEP-CPC are also analyses that have been shown to have serious deficiencies for stratospheric studies. (E.g., Manney et al, 2005, JGR; 2005, MWR; several others both before and since these). Why weren't more "modern" reanalyses used (e.g., the MERRA reanalysis using GEOS-5.2.0, or the ECMWF's ERA-Interim reanalysis)?

Page 22116, line 22, "In March...more persistent" sounds a bit odd, since more persistent applies to time evolution rather than a particular time period.

Page 22117, line 5, The breakup dates we are discussing are most commonly in late March or April, which is "spring", not "late winter".

Page 22119, lines 2-3: Please explain (and give references for) why "March lower

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stratospheric temperature and February planetary waving driving should be most influenced by SST variability in mid- to late winter".

Page 22119, lines 15-18: Please explain how this relates to the results of, e.g., Orsolini et al, 2009, QJRMS; Nishii et al, 2010, GRL.

Page 22120, lines 5-7: Do you come to the same conclusion about the heat fluxes in 1997 vs 2011 if you look at different time periods or levels above or below 100hPa? Likewise, were the March 1997 polar cap temperatures lower than those in 2011 at levels throughout the lower stratosphere (results in of Manney et al, 2011, Nature suggest this might not be the case)?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 22113, 2011.

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