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Title: The climatology of synoptic-scale ascent over western North America: a perspective on storm tracks

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Recommendation: Minor Revision

GENERAL COMMENTS:

The authors present a new perspective on storm track behaviour over North America by identifying storm systems with synoptic-scale ascent. They explore this methodology first with respect to an individual case-study then construct a climatological analysis using the ERA-Interim dataset. Their analysis discusses the seasonal (winter) mean, intra-seasonal variation, and ENSO-response in the storm track over North America. Given the difficulties with defining and interpreting any single-level "storm track" measure (i.e., either dependent on upper or lower tropospheric variations), I found this an interesting paper to read.

The presentation is good and I believe that the proposed methodology could potentially be applied usefully and widely. I do, however, have some concerns about the details and, in particular, its sensitivity to the "depth" of the storm track (outlined in detail below). Assuming that these concerns can be addressed, I would recommend this paper for publication.

MAJOR COMMENTS:

1 – Storm track depth

As noted by the authors, there are many plausible definitions of "storm tracks". These range from diagnostics focussing on near-surface features to those concerned with upper tropospheric disturbances (~line 65-85). As further noted in the paper (line 81) the spatial disagreements between the different metrics occurs because they are, in general, capturing different aspects of storm track behaviour. Despite this, one tends to have a concept of a true "storm track" perhaps identified as an area with repeated occurrances of vertically deep baroclinic wave structures – that is, synoptic features that extend through the troposphere rather than simply shallow surface cyclones or wobbles in the upper-tropospheric circulation. I therefore believe that the approach adopted by these authors is very interesting because it potentially offer a more sophisticated tool for identifying these deep cyclones than is common in the climate literature.

I do, however, have some concerns about the details of the methodology and its interpretation. In particular, the method relies on a calculation of the time-mean ascent rate (ϖ) at each grid point (eqn 5). This time-mean is constructed (~line 191) using the maximum ascent rate in the vertical (ω^m, i.e., the maximum value recorded in the 5 vertical levels from 700 hPa to 300 hPa, line 166). Therefore at different locations the mean ascent rate may perhaps be principally associated either with relatively shallow ascent (ω^m typically occuring at, say, 700 hPa) or rather deeper ascent (say above 500 hPa). In the current framework no distinction appears to be made between these cases.

One is therefore left wondering about the nature of the storm track identified by ϖ over the southern US and its applicability to continental regions more generally. In particular, over the interior of the continent there is likely to be less atmospheric moisture available and, as a consequence, storms and ascent might be rather shallower as there is less latent heat release (the methodology used also ignores diabatic heating from condensation, lines 182-184). Indeed, in a more idealised setting, GCM experiments by Brayshaw et al (2009, JAS) showed that an intense-but-very-shallow storm track was generated in the central and eastern part of extratropical continental interiors but that deeper storm tracks (appearing at both the surface and tropopause) were generally restricted to ocean basins.

I would therefore like to see more detail on precisely what is being detected by their methodology over the continental interior. If the storms in the interior are indeed shallow, does an individual storm system usually go on to deepen when it reaches the continental coastline? If so, then the regions identified by ϖ over the continent might legitimately be considered as extensions of the main oceanic storm tracks but, if not, then I would suggest that this new diagnostic technique is perhaps identifying different storm track types/depths over land and ocean.

The authors may have already looked into this question, but I think it would be helpful to either have more discussion of individual synoptic events (as well as the very extreme Tax-Day storm) or, preferably, a composite climatological-analysis of the level at which ω^m occurs. It would also be worth examining the impact of replacing ω^m with a single mid-tropospheric value of ω: would this produce a significantly different picture if the level was varied from 700 to 500 to 300 hPa? Finally, it would be interesting to see the same diagnostic applied to a wider geographical region including the main Northern Hemisphere storm tracks that lie over the oceans: does the new method adequately capture their structure as well?

As an aside, the authors may find the ongoing IMILAST storm tracking intercomparison project of interest as they develop their work (http://www.proclim.ch/imilast/index.html).

2 – Mean ascent vs. synoptic scale ascent

Figs 4 and 5 show the climatological-mean “synoptic scale” ascent, its frequency of occurrence, and the mean ascent magnitude during identified events. However, in terms of the large-scale time-mean flow one might expect there to be ascent downstream of the southern portion of the mountain range along the western US (Brayshaw et al, 2009 JAS – see their fig 15). How much of the pattern in Figs 4 and 5 is associated with the transient systems compared to the time-mean flow?

MINOR COMMENTS:

1. I found the description of the Tax-Day storm a little hard to follow because of the references to names of individual states in the US. Perhaps State name abbreviations could be added to Fig 1 for the benefit of non-US readers?
2. Section 4b refers repeatedly to Fig 11, but I think it means Fig 13?