

Interactive comment on “Front-orography interactions during landfall of the New Year’s Day Storm 1992” by Clemens Spensberger and Sebastian Schemm

Anonymous Referee #1

Received and published: 11 December 2019

This paper re-examines the notorious New Year’s Day storm that struck Western Norway on January 1 1992, and led Grønås (1995) to introduce the ‘poisonous sting at the end of the tail’ to the meteorological literature, now referred to as a sting jet. The main focus is on the effect the Scandinavian mountains had on the thermodynamic structure of the fronts in this storm, although the paper also examines whether the mountains also affected the development of the strong winds that struck the Norwegian coast (the answer to this is a resounding no – the same winds would have developed regardless of the mountains). The paper is an interesting study but needs considerable polishing to be a useful addition to the meteorological literature.

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My general criticism of the paper is that the concluding section does not link back to the introduction, and to the existing literature on the effect of orography on fronts. As a result it is impossible to see what is new in this paper and what corroborates (or contradicts) previous results (other than the null effect on the CCB wind maximum). The summary and conclusions need re-writing to place the current results in context, and should concentrate on verifiable results rather than speculation (e.g. 1.319-20, 328-32). This is a well-established area of research in meteorology. Only if the authors can show a genuine novel result should this paper be published.

Specific comments 1. Section 3. The figures that accompany this section show fields from both NORA and WRF, but the text does not make it clear which model field is being discussed. I would have thought that the reanalysis would be closer to reality than a free-running model so the synoptic discussion should be confined to NORA, making it a little easier to follow. I'm not sure why you need all the WRF graphs as their only purpose as far as I can tell is to satisfy the reader that the WRF simulation looks sufficiently similar to the reanalysis. Section 3.4 is far too superficial to require 13 figure panels (figs 2, 3, 4).

2. Line 256-260. The authors propose that IGWs are responsible for the effect of orography on mass transport at 500 mb. This need not be so: the mountains change the thermodynamic fields at the lower levels and therefore the height field at 500 mb. Mountain waves can only impart momentum to the flow if they break, and as they are fixed relative to the topography their effect would be to slow the winds at 500 mb. That would disturb the geostrophic balance, suggesting a flow towards low pressure, which is the opposite of that shown in fig 9b. In the absence of any evidence this paragraph is pure speculation, quite probably wrong, and should be removed.

3. The same unwarranted speculation continues in the first paragraph of the next section, which should either be removed or solid evidence be presented for this conjecture.

Typos

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Line 71 resolution of 10 km

Line 180 'warm-air seclusion suggests that . . . cyclone core may be dynamically linked'
– correlation does not prove causation

Line 225 fronts fronts

Line 265 vertical wind no longer shows

Interactive comment on Weather Clim. Dynam. Discuss., <https://doi.org/10.5194/wcd-2019-9>, 2019.

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