Comments on revised manuscript by Spensberger and Schemm,

Front-orography interactions during landfall of the New Year's Day storm 1992

This paper is much improved from the previous version. In particular the introduction and conclusions are now properly connected and it is clearer what the paper has to say that is new.

I am however still concerned about the argument regarding gravity waves. It seems to hinge on a strip of pink colour upstream of the Norwegian coast in fig 9b, which is the 500 mb field of mass transport perpendicular to isolines at 03 UTC, from the doubled-orography WRF run. The pink strip denotes flow towards lower geopotential. The vertical velocity field from this model run isn't shown, but assuming it resembles that with the original orography, we should compare 9b to 4d. There are clearly no gravity waves in the location of the pink strip in fig 4d, and where gravity waves are found (over the whole Norwegian land mass, not just the Scandes mountains) the mass flux in 9b is away from the cyclone centre. This not what I would expect if the waves were breaking and decelerating the flow. The paper wave s this aside with a sentence I do not understand: 'A IGW-induced deceleration would in turn give rise to a geostrophic adjustment process with an initial acceleration towards the cyclone core, consistent with Figure 9'.

The mass flux structure in 9b does indeed suggest a local trough due to ageostrophic flow in this region, roughly on the scale of the Scandes, reminiscent of lee cyclogenesis, though it's hard to see that in the geopotential. In turn that suggests that the response of the atmosphere to the perturbation of the mountain range is, first and foremost, to set up a deformation on the scale of the mountain range that extends into the upper troposphere. This indeed appears 'simultaneously' throughout the troposphere because the mechanism is gravity wave propagation, not geostrophic adjustment. But the crucial point is that this has nothing to do with the much smaller-scale orographic waves shown in fig.4: it is the response of the fluid field as a whole to the orography.

The hypothesis that the smaller-scale waves transferred momentum to the flow requires that they break – i.e. reduce in amplitude with height or encounter a critical level. The authors could easily have checked that by examining how the wave field varies with height, but they have not done so, preferring instead to speculate.

Section 4.4 starts by discounting the possibility that the response at 500 mb was due to advection, concluding that it can't be. This is complete red herring – of course it's nothing to do with the physical movement of air parcels as the orographic perturbations move (rapidly) through the fluid. Geostrophic adjustment is a process whereby the atmosphere adjusts its wind and pressure fields towards geostrophy when it is forced away from dynamic balance. Again this is irrelevant to a process which is being forced by the orography, which is maintaining the dynamic imbalance.

There are copious books and papers dealing with flow over orography which the autho rs should consult. There they will see how the flow patterns adjust to the presence of the orography, extending up through the atmosphere 'almost-instantaneously'.

I found a few typos:
l.2 Coast
l. 153 Wind field is shown in Fig 3 not 2
l. 165 frontal structure
l.191 The blue circles in fig 4e,f are over the ocean, not over the lee of the Scandes
l.220 'is there' rather than 'there is' (an English subtlety which I can't explain!)
l.334 accelerated
l.353 too