Comments to AMT paper

"Ground-based remote sensing profiling and numerical weather prediction model to manage nuclear power plants meteorological surveillance in Switzerland"

by B. Calpini et al.

General comments

Switzerland has four Nuclear Power Plants (NPP). The authors explain the importance of monitoring and forecasting the nuclear dispersion in case of an emergency (leakage) event through in situ observations and a dispersion model. In the past the dispersion model was fed with in situ observations from meteorological masts at the NPP locations. The masts have been dismantled and the dispersion model input is now obtained from synthetic observations derived from a limited area high-resolution non-hydrostatic model. This latter model is fed with observations from wind profilers and automatic weather stations to yield the best possible description of the atmospheric state. Hereto the model is run in a rapid update cycle mode of 8 runs per day. The quality of the model dynamics has been verified with independent wind observations in two validation campaigns. Finally, a case study of an artificial release of nuclear particles should demonstrate the additional value of using the full four-dimensional model dynamics rather than in situ observations to feed the dispersion model.

The paper is well written and clear. The use of (high-resolution) observations in high-resolution models is still in its infancy and requires much research in the coming years to find the optimal use and density of observations to feed these models. This paper touches these issues for a very challenging (mountainous) region and contributes to further progress in this area. I therefore recommend this paper for publication.

The remainder includes suggestions to improve the paper. In addition, I have some questions to the authors and I look forward to their response.

Specific comments

Page 6. Last paragraph. The authors state that "In the new safety tool, the vertical profile of wind, temperature, and turbulence values at each of the four NPPs are obtained using directly the simulated values from COSMO-2, thus the former meteorological masts have been dismantled"

From the text it is not clear on what basis it was decided to dismantle the meteorological masts. Was it based on the results presented in this paper only? If that is the case, then this is quite tricky, see further comments below. Also, why not assimilate observations from these masts in COSMO-2? From figure 1 they are not too close to the wind profilers (not redundant) and thus might have additional value for the COSMO-2 analysis in particular near the NPP's?

Page 10. The authors find a large discrepancy between model and observations in about 20% of the time. For me that sounds like a very large number and as a decision maker I would have strong hesitation to replace real observations with synthetic model observations with this

consequence. Could the authors elaborate a bit more on the decision to dismantle the meteorological masts and replace it with the new tool, thus introducing a 20% risk of an incorrect dispersion forecast. Is 20% acceptable? If not, what value is acceptable. I guess this refers to the quality index mentioned on Page 11.

To elaborate a bit more on the authors statement on page 10 that "the model be out of phase in time versus the current measured weather condition …". I am wondering about the Quality Control (QC) applied to observations in COSMO-2. In most data assimilation systems QC of observations has several stages, one of these comparison against the model background (short-term forecast). If the observation minus background exceeds a certain threshold then the observation is rejected. This might be the case for phase shifts discussed by the authors for frontal passages. Observations may be rejected (for the wrong reason), thus preventing the model to adapt to the rapid change. Have the authors looked at observation rejection statistics, in particular for these rapid change events?

To reduce the 20% discrepancy between model and observations the authors focus on model improvements, currently mainly a decrease of the model grid size to 1 km (first paragraph on page 10). In general, going to higher model resolutions requires an increase of the observation network density if one aims to resolve the small (km–scale) atmospheric scales. Only decreasing the model grid size does not automatically increase the effective model resolution, i.e., the spatial scales that models can resolve. Nowadays global models have 10-20 km grid size, but their effective resolution is only 150-200 km, i.e., smaller than these spatial scales are not resolved. I do not know the numbers for COSMO, but I would suggest to have a more fundamental look at this by finding an optimal balance between grid size reduction and observation density. In addition, the authors could consider 1-hour cycles to try to solve the phase shift. Other meteorological institutes are currently experimenting with that.

Page 12. Second paragraph. The authors speak of a "three dimensional picture" twice in this paragraph. I guess they mean four-dimensional? In any case, the text does not explain how the CN-MET tool product is used in the dispersion model. I guess the forecast fields are interpolated to the NPP locations to obtain the synthetic observation profiles. This can be done for all forecasts in the range from 1 hour to say 24 hour. When all these hourly synthetic profiles (this is the 4^{th} dimension) are fed into the dispersion model I can understand to result (radioactive release moving into one direction and then moving backwards) presented in the case study of section 4.
If the authors really mean three dimensional then it is always better to use real (mast) observations in their dispersion model instead of synthetic ones, because the representativeness of the latter is always smaller, i.e. lacking small-scale atmospheric features (turbulence) that is underestimated in models but present in the real atmosphere. Could the authors pleas comment on this.

Page 14. Summary. In the 2^{nd} paragraph the authors state "Assimilation of upper-air winds measured within and above the planetary boundary layer improved substantially the quality of the forecasts, … "
This has not been demonstrated in the text. This requires for instance a control or free run, without the assimilation of upper-air winds, for comparison. This was not discussed in the paper, so please remove or reformulate.
In fact, it would be a good starting point to first run COSMO-2 without any observations, but just as a downscaling from COSMO-7. The next step is then the current setup with the additional observations as discussed in the paper and show the (hopefully, but not guaranteed)

improvement. In a next step the authors could consider what additional observations are further needed for further improvements to reduce the 20% discrepancy to an acceptable level. Can the authors please comment on this.

Detailed comments

Page 3. explain a.g.l.
Page 9, first paragraph: follow ⇒ follows
Page 10, third paragraph: shows clear discrepancy ⇒ shows a clear discrepancy
Page 11, last paragraph: dark grey. Remove dark. In the caption of figure 6 dark grey is used for the CHA station.