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

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Answers to reviewers

We are very grateful for the reviewer's comments, which have helped us to substantially improve our paper. Many of their comments were well justified and have been taken into account when revising our manuscript. Specifically we respond to their comments as follows.

Review # 1

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?

The height of the meteorological masts located at the NPPs was 120m at the maximum with *in situ* wind and temperature sensors typically at 10, 60 and 120m agl: such measurements are limited in height (they do not cover the PBL) and they are also "in situ" / local / instead of integrated /averaged/ over the vertical layers of the NWP. They are representative of a local air mass around the NPP and for short time scale after a release event. Only the NWP can adequately define /simulate/ the synthetic vertical profile of each physical quantity above the NPPs and over the Swiss Plateau in the PBL.

Along with these limitations the meteorological masts maintenance which required 100% data availability (!) implied critical aspects in particular for security reasons (changing a wind sensor in harsh environment at 120m above ground!) thus inducing significant cost issues.

These are the 2 reasons why they were dismantled.

These meteorological masts are of interest for short time scale and local atmospheric conditions which make them a valuable input for low altitude observation input for the dispersion model. For this reason each AWS located at the NPP site has been further equipped with a 10m meteorological mast giving already a good representation of the local wind conditions, but avoiding the drawbacks of the 120m masts as mentioned here above.

This information is introduced in the publication.

Finally in case of nuclear release a circle of 5-10km radius defined specifically for each NPP location is used as “entire emergency area”. This cancels the interest of wind forecast at a few km scale around the NPP. This information is not for a paper in the open literature.

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.*

The reasons for dismantling the masts are explained in the previous answer.

The study of the period of time for which outliers are obtained between model and measured results is one example of the study of the performance of the NWP against the independent remote sensing wind profiler measurement: it demonstrates the performance achieved by the new security tool over the first 3’500m asl. This performance is not dependant on the assimilation of the meteorological tower measurements and the reviewer’s conclusion of “*introducing a 20% risk of an incorrect dispersion forecast* “does not apply in this case.

For the dispersion model the input of the older security tool was based on *in situ* meteorological inputs and “wind-classes weather types” (see par 1). Here the major source of error comes from the fact that these observations don’t cover the entire PBL by far. Assumptions such as the stability of the atmosphere, the definition of the temperature inversion height, the estimate of the turbulence of the atmosphere, etc. were all based on *in situ* observations obtained at altitude below 120m agl in order to derive the current wind classes. In this sense the older tool, and the new tool, are in essence different. With the new tool, what we may lose close to the ground is substantially compensated by a gain of information higher within and above the PBL. There is no absolute threshold (eg. better than 20% of the time) value but only a continuous search for the best security tool. The performance analysis of the new tool is based on the validation campaign 2008, 2009, and the case study on October 16, 2009. The 20% discrepancy is probably slightly overestimated, but forecasting in complex topography is a major challenge which was not performed better with the former tool. Moreover, these discrepancies were mainly due to short temporal shifts between model and measurements. The QC checks applied on COSMO 2 inputs are the standards defiend in the COSMO community. Hence Figure 4 is the result of a 3hr update cycle: in case of emergency (eg the October 16, 2009 case), the "on demand" COSMO-2 mode is run every single hour (see par. 2.1). In this case the time shift between model and observation is reduced.

True to say that increasing the model resolution to a NWP 1-km resolution model …may even bring other/ additional discrepancies: such development will be associated with major changes in the physics of the code, but also by reconsidering the observation network required for the new model. This work is planned for the next years, but only mention in the article as a "next step to go".

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 4th 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.

We are effectively speaking about a four-dimensional picture, sorry for the mistake. We modified the text accordingly. There is no interpolation, the forecast fields are directly obtained with the COSMO2 resolution, a synthetic vertical profile is generated for each NPP specifically (located in a grid cell of 2.2x2.2km) at each time step of the NWP: this profile is used as numerical input for the dispersion model. In a later stage of the CN-MET system, implemented in 2010, the Swiss Nuclear Safety Inspectorate ADPIC Lagrangian model has been directly embedded into the COSMO-2 Eulerian model without resorting to wind classes as intermediary information. This information is added at the end of chapter 4. This new scheme does not allow for simulating the past event of

2009 using the dispersion model directly embedded into COSMO-2. Hence in this later version, no predefined wind classes are necessary any more.

Page 14. *Summary. In the 2nd 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.*

It is true that at this point of the analysis no specific sensitivity analysis was performed so far (free run mode, or model performance check with/resp. without specific input data). The sentence *"Assimilation of upper-air winds measured within and above the planetary boundary layer improved substantially the quality of the forecasts, … "also* comes from our internal MeteoSwiss forecasters and users: they have noticed from their every day "forecast practice" a substantial improvement of the wind field forecast over the Swiss Plateau after the CN-MET tool was fully deployed. Since this information is subjective we don't mention it in the text. At this point in the paper we don't prove "from a numerical point of view" this conclusion with a specific study. Therefore we have changed the sentence in the paper accordingly.

Review # 2

1. General comments

There seems to be a misunderstanding on the side of the reviewer, who understood that the old security tool would only be a Gaussian model. In fact, a Gaussian model was applied for a very first estimate, but only until the dispersion model ADPIC would provide more sophisticated predictions. This was already noted in the section about the former safety tool and has now been extended to avoid this misunderstanding. This affects the reviewer's comment on the former safety tool.

Specific comments

2. The former safety tool

The description of the disadvantages of the old system has been amended.

3. Description of the model

The horizontal diffusion in COSMO-2 is an optional 4th-order linear scheme with an orographic limiter. It is designed to filter out unphysical shortwave noise, generally of numerical origin.

In our operational COSMO-2 configuration, this option is switched off in the inner part of the model domain, i.e. no expicit horizontal diffusion is active in the domain of interest for CN-MET.

Regarding wind data at very small scales, see my answer to comment 8.

4. Availability of data and the role of the backup technology

During a certain period, boundary conditions from a previous IFS run can be used. We are also investigating into a backup solution using boundary conditions from the global model of the German Weather Service. If all fails, there are still the 10 m wind towers with turbulence measurements at the NPP sites that can be used for simple estimates.

5. Convection and the model resolution

According to the literature, the explicit representation of the convection implies a mesh-size of as much as 4 km (Weisman et al., 2008) or as little as 100 m or less (Bryan et al., 2003). For Europe, or indeed, the Alps, a mesh-size of the order of 1 km is considered sufficient (Cosma et al., 2002; Chiao et al., 2004).

More pragmatically, the operational verification of COSMO-2 shows an improve timing of the daily convection cycle when compared with a model using parametrized convection (COSMO-7). This is a hint that an explicit representation of the convection on a 2.2km grid already brings some benefits.

Bryan, G. H., J. C. Wyngaard, J. M. Fritsch, 2003: Resolution requirements for the simulation of deep moist convection, Monthly Weather Review, 131, 2394-2416.

Chiao, S., Y. L. Lin, M. L. Kaplan, 2004: Numerical study of the orographic forcing of heavy precipitation during MAP IOP-2B, Monthly Weather Review, 132, 2184-2203.

Cosma, S., E. Richard, F. Miniscloux, 2002: The role of small-scale orographic features in the spatial distribution of precipita-tion, Quarterly Journal Of The Royal Meteorological Society, 128, 75-92.

Weisman, M. L., C. Davis, W. Wang, K. W. Manning, J. B. Klemp, 2008: Experiences with 0-36-h explicit convective forecasts with the WRF-ARW model, Weather And Forecasting, 23, 407-437.

6. Some references to ADPIC

Unfortunately, we do not know of a published description of ADPIC. It has however been evaluated in dispersion modeling experiments, one of which is now added as reference

7. The discussion of the Lagrangian outputs

We agree that the description was not clear and in fact no comparison has been showed in Figure 9. We have changed Figure 9 to ameliorate this and changed the text accordingly.

8. Limitations of the non-hydrostatic model

You are right, COSMO-2 is only able to represent physical phenomena at a scale of about 5 times the grid spacing, meaning about 10 km. This of course excludes all local effects triggered for instance by the the plant towers. To obtain a good representation of these effects a much finer grid and a LES closure would be needed (in addition to many additional improvements).

However, the goal of this study was to show the benefit of using a general purpose NWP model instead of the previous climatological approach. The focus is on a production tool running with a very high reliability around the clock and not on an exploratory method able to use large compute resources.

9. Theoretical basis for dispersion modeling

We think that a discussion of the theory that forms the basis of Lagrangian modeling, although interesting by itself, is too far off the scope for this paper which is oriented toward a specific application.