Atmos. Chem. Phys. Discuss., 13, C5443–C5445, 2013
www.atmos-chem-phys-discuss.net/13/C5443/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on “Simulation of the dispersion of the Eyjafjallajökull plume over Europe with COSMO-ART in the operational mode” *by* H. Vogel et al.

Anonymous Referee #2

Received and published: 1 August 2013

The paper describes a simulation of the important problem of quantitatively forecasting the dispersion of volcanic ash, taking the 2010 Eyjafjallajökull eruption as a test case. It is demonstrated to apply the adapted COSMO-ART forecast system in an operational context, with time-lag generated ensemble to infer a probabilistic forecast, with the aspiration to provide information for decision makers on closing air spaces. After tuning, the system aims to demonstrate its potential by comparison with observations. While the subject is clearly of interest and related studies merits publication in ACPD, there are some substantial shortcomings which need to be removed before final publication. The paper clearly addresses important questions for simulating the dispersal

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

of volcanic effluents and is of high relevance to ACP. In this paper tuned simulations of the volcanic events are presented, with the general aspiration to provide guidance for air traffic control. However, by comparison with observations and using other a posteriori information sources like “ source strengths correction factors” from another paper, the method falls short of demonstrating necessary predictive skills in operational mode (see title). Rather, some type of an analysis of the event is provided. Further, probabilities of threshold values were calculated by time lagged forecast ensembles. The way they are constructed by an ad hoc sequence of intermitted forecasts, some account on meteorological uncertainty is given, which are presumably not the parameters of lowest skill in the context of volcanic eruptions. While the variable emission source strength and height are markedly more uncertain, along with sedimentation and wet deposition velocities and microphysical parameters of the particles, a properly designed ensemble with a generation mechanism, which reflects the poor state of knowledge is lacking. In contrast to the explicit claim in the abstract, only lagged forecasts ensembles cannot provide probabilistic information used for decisions with fundamental consequences like closed air spaces. Hence, probabilities or uncertainties provided do not trustworthily reflect the uncertainty of the forecast. Rigorous applications of stochastic simulation principles are yet a matter of ongoing research. Nonetheless, the claim made in this study should at least acknowledge the limited frame of the probability accomplished. To provide substantial conclusions, in their response, the authors are requested âĂć to explicitly define the objective(s) of the paper, âĂć to describe the methods applied in more rigorous terms, âĂć to quantify the predictive skills and limitations of their method quantitatively in operational mode, in terms of the mathematical technique, and âĂć to estimate the fraction of probabilistic information, as part of the full uncertainty, and âĂć to clearly identify the novel steps beyond the state of the art. In its present form the description of the system set-up is not sufficiently complete and precise to allow their reproduction by fellow scientists. The abstract provides a concise, probably too concise summary. It is however too simplistic with judging the simulation in a "nearly perfect agreement", which is of little avail to estimate the model

ACPD
13, C5443–C5445, 2013

skill after the later tuning efforts. There are some flaws in English writing. The authors may consult a native speaker for improvements. Detailed items: 1. In the abstract : What are the “measured data” used for calibration? 2. Page 2, last paragraph: it is claimed what has been adapted in the forecast system during the volcanic eruption. Please add citations of the work, supporting the claim, or remove paragraph. 3. Page 2, 2nd paragraph: The parameters to be forecasted include volcanologic quantities like size distributions and vertical distribution of effective source heights. These parameters, while in the realm of volcanology, cannot be forecasted with usable accuracy for atmospheric simulations. Please reformulate or remove this paragraph. 4. Page 3: 3rd paragraph (1. in section 2): The definition of online vs offline; is there a feedback from volcanic ash loads to the meteorological parameters? If not, the claim of being online is obsolete, as “off-line” configured systems can reproduce the same results. Please explain. 5. Page 4, subsection 2.2: ad the web site of VAAC London. Consider the time delay and limited accuracy and temporally coarse resolution (6 hours) of available ejection heights. Keflavik radar was the most reliable information in those days, but they were not directly available. Explain your configuration set-up for operational use. 6. page 5, 2nd para.: Give a reference of the DWD forecast set-up. Cite the reference paper for COSMO-ART here. 7. page 5, last parag. of section 2: I do not understand (e.g. “hindcast after 9 days”). Please reformulate. 8. Page 5, discussion Fig 5.:”captures . . . quite well”. Could you please be more precise on this? 9. Page 6 discussion of Fig. 3 in relation to Fig. 1 Gasteiger et al 2011. Please combine these information in a single graphics. It is hard for the reader to follow the explanation. 10. Page 6, 3rd parag.: Subsidence would be better discussed by considering the stability of the air and moving of stable layer patterns associated with the high pressure system. A clear distinction of an Eulerian and a Lagrangian viewpoint for rating the subsidence is crucial. See also the discussion in the conclusions page 10, end of 1st paragraph. 11. Figure captions are generally kept too scantily.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 13439, 2013.