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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Determination of time- and height-resolved volcanic ash emissions for quantitative ash dispersion modeling: the 2010 Eyjafjallajökull eruption" *by* A. Stohl et al.

A. Stohl et al.

ast@nilu.no

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We thank referee 1 for the constructive comments on our paper. Below, we repeat these comments in italics and add our answers in normal font.

1. Abstract mentions 'air space closures and for real-time quantitative estimations'. Consider making this "near" real-time (and in conclusions) and explain in body the run-time issues.

We think it is a question of taste whether to call it near real-time or real-time. With the expression real-time we wanted to emphasize that the method would be used (maybe in near-real-time) to provide emission input to real-time forecasts. That means the

method as a whole (including the subsequent dispersion forecast) would actually work in real-time and could look into the future, i.e., it could be used for advance planning of air space closure, etc. Therefore, we would prefer to leave the term "real-term".

We can mention the runtime issues in the revised version of the paper. However, this is rather technical and there are no major real run-time issues as long as a fast computer system is used, which allows running multiple sensitivity simulations at the same time. If the system is set up in a clever way on a reasonably fast computer cluster (e.g., 32 processors), we believe the derivation of emission information (i.e., emission sensitivity calculations and inversions) could be done within an hour of receiving the most actual meteorological data. Actual forecast runs could be started in parallel and be updated after that hour. Updates could even be made in between the times meteorological analyses (and forecasts) are received, using the most recent satellite data.

The major remaining issue of dispersion forecasts is of course that the system could not predict future volcanic activity, i.e., it would have to assume - for instance - persistence in the emission rates. However, this concerns only future emissions, not the ash that's already in the air at the time the system is started.

2. There is little quantitative measurement data available for most volcanic eruptions, world-wide. Consider commenting that this process is most useful only for wellsampled (measured) ash clouds.

The satellite data we used is global (IASI) or is available for a large domain (SEVIRI). Furthermore, other areas are also observed by geostationary satellites. Thus, global coverage is ensured (we could also rely on polar orbiting platforms only, thus ensuring coverage even in the polar regions) and the method can be used for any eruption, regardless of the volcano location. However, we agree that Eyjafjallajoekull provided an ideal test case because of the availability of many data sets for validation. Such a data-rich situation is not often encountered. But the method itself does not depend on any data that are only available in certain locations (e.g., over Europe).

3. Sec. 2.2 – assumed 10% was fine ash. How was this chosen? For any other given volcano/eruption how would you know to use 2% vs. 10% vs. 40%? How much does this matter?

The chosen 10% are admittedly highly subjective. In a real-case scenario we would probably get the order of magnitude from a visual subjective comparison of total ash loadings in model calculations with the first available satellite data. However, given enough satellite data are available (which typically will be the case soon after the eruption on-set), the method is not strongly sensitive to the chosen a priori value. It will in any case provide an improvement over the assumed a priori values, with the a posteriori values being closer to the real ash emissions. So it will always yield an improvement over currently used methods (which are all based on subjective assumptions such as we have used for defining our a priori).

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 5541, 2011.