Review "Status and future of Numerical Atmospheric Aerosol Prediction with a focus on data requirements" by Benedetti et al.

This manuscript aims at providing current status of aerosol prediction models, and at reviewing requirements for aerosol observations used by these models. The number of centers providing aerosol forecasting at regional or global scales have been growing significantly over the last decade. It is therefore a useful initiative.

The paper is very interesting and well written. However, there is a strong disparity in quality between sections. The sections at the beginning of the documents are either excellent or very good with only some suggestions listed below. But starting with Section 3.4 things gets too specific. They either focus on the research limited to a specific location, or discuss in details a specific model. Some sections contain incomplete information related to aerosol modeling. As the paper aims at incorporating as much models as possible, I would recommend synthesizing key elements of these models rather than going into the details of strengths and weaknesses of a few models and field campaign. I have also provided below General comments, which may help improve the manuscript.

General comments:

1. It would be useful to introduce mathematically aerosol prediction as an initial and boundary conditions problem as opposed to aerosol projection, which is essentially a boundary condition problem. This will help understand that data assimilation is particularly important for aerosol prediction, while for future projection emissions scenarios are the key factor.
2. At the core of any transport model, there is an advection solver. Models use different solvers, with some creating spurious waves. These numerical oscillations are generally smooth out with a diffusive scheme, creating numerical (unphysical) diffusion. These drawbacks are too important to be ignored, and I would recommend addressing them. An example of discrepancy generated by advection schemes has been discussed by Ginoux (2003). He showed that poor representation of dust size distribution in models was primarily due to the numerical solver of sedimentation.
3. A source of error considered in data assimilation is the inconsistency between simulated and observed variables. This is discussed in the manuscript but what is missing is the description of the dependent variables of the prognostic equations in these models. You should mention that dependent variables of these equations are mass/number concentrations, as it will clarify the discussion, while observations are mostly optical properties. Passing from one to the other necessitates assumptions and consequently error.
4. Is ocean data assimilation not important to be mentioned for seasonal to sub-seasonal aerosol prediction? How could you make any correct aerosol prediction without representing the right phase of large-scale oscillation such as ENSO or NAO? Maybe you should add a sentence about this without developing as it is beyond the scope of the paper.

5. Emission of several aerosols depends strongly on vegetation. For example, biomass burning will obviously depend on the amount of biomass, dust emission is drastically reduced in presence of any vegetation cover, and the emission of biogenic organic precursors depends on vegetation cover. It may be valuable in this paper to include data requirements for vegetation cover, as new model developments often increase the level of interactions between vegetation and aerosols. Evans et al. (2016) showed that dust variability in Australia is amplified by dynamic vegetation in agreement with satellite observations. Also, are there any recommendations to validate land model results used for aerosol prediction?
6. An additional application of aerosol forecasting model is to provide support during field campaign. The model provides direct information on aerosol optical thickness and concentrations for effective flight planning, while feedbacks from measurements constantly evaluate the model for successful model improvements (Chin et al., 2003).

SPECIFIC COMMENTS:
Section 1.2. I would suggest adding some sentences related to above comments 1 to 3.
Section 1.3. You may want to mention the use of forecasting model to support field campaigns (see above comment 6).
Section 3.2.2. Last paragraph. Increasing resolution does not necessarily mean better model skills. It may request new tuning of parameters of subscale processes (e.g. orographic gravity wave drag), as well as larger ensemble runs due to large variability. I wish I could propose a reference related to aerosol, but Kapnick et al. (2018) discusses such issues for the prediction of snow over the western US.
Section 3.4. This section on dust and the following on sea-salt are much shorter than the previous section related to biomass burning. Is there a justification for it? Section 3.4. No discussion on dust sources, which is the base of any dust study and modeling. All dust models use a form or another of preferential dust sources defined by Prospero et al. (2002) and adapted for global models (Ginoux et al., 2001; Tegen et al., 2002; Zender et al., 2003; Ginoux et al., 2012). These source functions were necessary because soil properties from global inventories (e.g. FAO) were and still are unrealistic in arid and hyper-arid regions. Although, model representing the physical processes of dust emission have been around for a long time (e.g. Marticorena and Bergametti, 1995; Shao, 2001), they have to be adapted to accommodate major discrepancy in soil texture datasets, the driver of dust emission. There is the interesting work of objectively compare different dust source inventories (Cakmur et al., 2006). It may be adequate to perform similar exercise with more recent inventories.
Section 3.4. Not one word on soil texture, soil moisture, vegetation cover, and mineralogy, although these are key elements to simulate dust emission, distribution and optical properties. I would recommend including them in a paragraph with references. On the other hand, there is a discussion on the difficulty to represent sub-scale dry and wet convection. These are important processes for dust emission, but it may be better to discuss boundary layer parametrization in a “meteorological” section. Why are you mentioning 3 field campaigns? And these ones in particular, are the others less important?

Section 3.4. Satellite data. You mention IASI but there are more than 2 groups working on retrieving dust from the data. Geostationary satellites have their own quality for aerosol prediction, and SEVIRI has been quite useful to retrieve dust sources (Schepanski et al., 2007), or detect haboobs (Ashpole and Washington, 2013). Also, I would mention the promising results from GRASP algorithm (Chen et al., 2018).

Section 3.5. There is no mention of the temperature dependency of sea salt emission. Most models are now considering it, specifically the parameterization of Jaegle et al. (2011)

Section 3.6. This section is detailing removal processes of one model (NAAP), but they are generally treated quite differently in other models. It reads as a technical report of the NAAP model. Also, it seems that important processes are missing, such as in cloud scavenging, Bergeron process, etc. It would be more useful to learn about the method to parameterize the different physical processes rather than learning what is useful or not to run NAAP.

Section 3.6. Line 703-705. I would mention the work by Yu et al. (2017), which allows evaluating dust deposition by combining MODIS and CALIOP data.

Section 4.2.2 Line 790. Reference(s) would be useful.

Section 4.2.5. This section is again focusing on model (CAMS) to discuss its problems. Why should I care about this model if I am not using it?

Line 874: There is method to derive aerosol speciation from AERONET (Schuster et al., 2005), and more recently there are promising possibilities with GRASP algorithm (Torres et al., 2017)

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