Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-441-RC1, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of the 4 April 2014 Saharan dust outbreak on the photovoltaic power generation in Germany" *by* Daniel Rieger et al.

Anonymous Referee #2

Received and published: 14 July 2017

The authors assess the impact of including mineral dust in numerical weather prediction on the quality of forecasts of incoming shortwave radiation and photovoltaic power generation in Germany. They do so by implementing a dust emission scheme in their model ICON-ART which produces spatio-temporally varying airborne dust concentrations to replace the dust climatology that had been used in the model so far. Changes in the model's forecast quality are investigated for a Saharan dust outbreak in 2014 which transported dust over parts of Europe, including Germany. Using four different model setups for dust concentration, the authors test the contributions of dust direct (radiation) and indirect (cloud) effects on changes in the forecast quality.

The manuscript is well written and organized, very detailed (almost somewhat too detailed), and a valuable contribution to the research field. In my opinion, the paper is

within the scope of ACP and can be published after minor revisions. My specific comments are the following:

1) How much would the deposition of dust on the solar panels contribute to discrepancies between forecast and obtained photovoltaic (PV) power production? Can this be estimated from modeled dust deposition?

2) Little information is given about existing studies evaluating the effect of improved dust forecasts on PV power production, also including areas other than Germany, and existing operational dust forecasts. I recommend adding a short review to the introduction.

3) How big is the effect of including dust in PV power forecasts for Germany compared to errors due to, for example, clouds (e.g. moist convection in summer)? As a perspective, this would be interesting to know, even if it may be small.

4) (P7, Lines 12-16) The authors use a tile approach to compute dust emission, but calculate the soil moisture correction for threshold friction velocity using only one value per grid, because the modeled soil moisture is only available per each grid cell. My impression is that it would have been more consistent to use a tile approach for moisture correction as well, i.e. using clay content and residual soil moisture for the soil type fractions together with the grid-scale soil moisture. Can the authors give an estimate of the difference/uncertainty related to either of the two approaches?

5) (P4, Line 23) The threshold friction velocity for particle entrainment is estimated based on the equilibrium of aerodynamic, cohesive, and gravitational forces; not only aerodynamic and cohesive forces (e.g. Marticorena and Bergametti, 1995; Shao and Lu, 2001).

6) (P4, Line 27) A soil density of 1500 kg m-3 seems to be rather small. More common is a value of $\sim$2650 kg m-3, which is also what was used by Shao and Lu (2000) in their derivation of u*t. Please give reasoning for using such a small value.

7) (P4, Line 29 “In contrast to Shao and Lu (2000)…”) This sentence is misleading as it seems to suggest that the inclusion of correction factors for surface roughness and soil moisture are an improvement compared to the work of Shao and Lu (2000). The inclusion of such corrections is common in dust modeling and simply not within the scope of Shao and Lu’s theoretical work. I therefore suggest rewording the sentence, for example, as: “For application in a regional model, we include correction factors to account for the effects of roughness elements and soil moisture on u*t (e.g. Shao, 2001)”.

8) The authors frequently use the term “emission flux” throughout Section 2.1. For the sake of clarity, I think it is important to better differentiate between saltation flux (e.g. P5L5), and dust emission flux (e.g. P4L7, P5L23, P6L1).

9) (P13, Line 13) Cloud coverage hinders comparison with satellite-based remote sensing, but not with measurements in general.

10) (P17, Lines 6-7) Model variables at the closest grid points to the pyranometer stations were used as input for PV power estimation. Did you consider interpolating the meteorological variables to the corresponding locations? The difference might be small for a horizontal resolution of 5km though.

11) (P5, Line 16) Why is the saltation flux weighted with the particle cross-sectional area?

Technical comments: - P2L33 and P3L1: Section (capital “S”). - P4L30: No comma after “Equation 2” - P5L1: Independent of - P6L6: energy; also I suggest “kinetic energy are chosen such that particles in the largest mode are emitted first when the threshold friction velocity is exceeded”. - P6L13: heterogeneity - P6L15: except for - P7L7: corresponding instead of according - P7L19-21: Sentence seems convoluted, please consider rephrasing. - P8L25: nucleus - P9L17: As mineral dust particles serve as ice nuclei - P9L28: Albedo of the surface or of the PV module? - P10L4: To my knowledge, “ridge” or “upper level high” are sufficient rather than “upper air ridge of high pressure”.

- P13L7: in Figure 4 - P13L8: the spatial distribution - P13L14-15: Suggest rewording as: “However, in clear sky regions, the amount of modeled mineral dust is of similar magnitude as that observed from satellite”. - P16L6: in case of TT - P16L20-21: the part “not only were large contributions of PV capacity are installed” is not clear to me. - Fig. 8: The green and blue lines are somewhat hard to differentiate. I suggest replacing one of the colors. Caption: “pyranometer observed” - P18L2: clouds - Fig. 9 Caption: corresponding difference instead of according difference - P20L11 ff.: I suggest giving the full names of measures such as IR when they first occur. This eases understanding for the reader. - P20L12: measure for whether - P21L2-4: I suggest moving the first sentence (“The synergistic interaction…”) to the beginning of the Section. The second sentence (“The contributions…”) is then not needed any more as it repeats what has been said in the beginning.

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