Answer to Referee 2

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Dear Referee 2,

we thank you for your comments on our manuscript. Below we present our answers to your comments (italics) accompanied by the changes we performed in the manuscript (blue color).

On behalf of the authors

Daniel Rieger

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?

This is a very good question. Indeed it is known from literature that soiling of solar panels leads to a degradation of their efficiency. Within the PerduS project we have currently started a measurement campaign to quantify the deposition of mineral dust on solar panels and in parallel the self cleaning of the panels by precipitation. Based on these measurements, parameterizations will be derived and included into ICON-ART. This is ongoing research and subject of future publications.

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.

A literature review on dust forecasts and impacts on PV power production has been included. For a detailed answer please see comment 1 to referee1.

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.

Forecast quality strongly depends on the weather situation, as you already suggested. There are different weather situations, where NWP forecasts are afflicted with forecast uncertainties, which also propagate into power forecasts. For solar radiation and power forecasts Köhler et al. (2017) identifies NWP shortcomings concerning the correct representation of shallow cumulus behind cold fronts, Saharan dust outbreaks, and the spatial and temporal resolution of convection as well as low stratus. The question, which problem is the most pressing concerning PV power forecasts depends also on the scales you are interested in.

For a single PV farm, one missed convective event may be critical. In PV power forecasts for whole Germany, such an error would be averaged out or compensated by other errors. As in our example, mineral dust plumes can cover large areas. Mineral dust aerosol influences the atmosphere directly and indirectly and thereby governs not only the atmospheric transparency but also the formation and evolution of clouds and thus the amount of incoming radiation at the surface. A quantitative answer to your question can only be assessed by numerical simulations. Within the project PerduS we intend to investigate this question. Furthermore, the monetary value of PV power forecasts relying on NWP with online-coupled mineral dust aerosols will be assessed.

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?

For a soil tile with lower-than-average residual soil moisture, the difference between the grid average soil moisture and the residual soil moisture is larger than it would be for a resolved soil moisture content. This leads to a higher soil moisture correction factor and, hence, reduced emissions in case of lower-than-average residual soil moisture and vice versa for higher-than-average residual soil moisture. In the end, the total emissions are calculated for each grid cell where underestimations from lower-than-average soil types and overestimations from higher-than-average soil types compensate each other to a certain degree. The error of this is difficult to quantify as it depends heavily on model resolution and source area. For source areas with rather uniform soil and a spatial extent much larger than the model grid cell area, the difference should be very small. Figure 1, figure 2 and figure 3 show the fractions of the most ubiquitous soil types in the Saharan region plotted on the original ICON grid. Generally, high fractions prevail. This underlines our point that the uncertainty related to the assumption of an average residual soil moisture content should be very low.

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).

We agree and modified the sentence according to the suggestion. We could not find a Shao and Lu (2001) publication and think you probably meant Shao and Lu (2000).

The threshold friction velocity is defined as the value of the friction velocity at an equilibrium of aerodynamic, cohesive, and gravitational forces (e.g. Marticorena and Bergametti, 1995; Shao and Lu, 2000).

*6) (P4, Line 27) A soil density of 1500 kg m-3 seems to be rather small. More common is a value of 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.*

This is a mistake in the manuscript. We checked the code and in the code it is indeed $2650\,\mathrm{kg\,m^{-3}}$. The value of $1500\,\mathrm{kg\,m^{-3}}$ is the value of the bulk soil density which is used for a unit conversion of the soil water content. Thank you for pointing that out.

with $A_n = 0.0123$ and $\gamma_n = 3\cdot10^{-4}\,\mathrm{kg\,s^{-2}}$. $\rho_s = 2650\,\mathrm{kg\,m^{-3}}$ is the density of the soil and g the gravitational acceleration.

*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)".*

We agree that this sentence is misleading and we rephrased it according to the suggestion.

For application in a global or regional model, we include correction factors to account for the effects of roughness elements (f_r) and soil moisture (f_η) 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).

The term emission flux was already used in contrast to saltation flux. To further increase the readability, we follow your suggestion and named the two fluxes uniformly saltation flux and dust emission flux.

The resulting saltation flux is then used to calculate a dust emission flux using the parameterization of Alfaro and Gomes (2001).

The following approach to calculate dust emission fluxes applies for one single soil type.

where $C_{white} = 0.7$ is a linear scaling parameter to adapt the dust emission flux to measurements.

By impaction of the saltating particles, the saltation flux leads to the release of small particles creating a dust emission flux.

This leads to the following equation that connects the saltation flux with the dust emission flux of aerosol mode l (Alfaro and Gomes, 2001):

over all saltation particle diameters weighted by their cross sectional areas yields the total dust emission flux of mode l:

As stated previously, a tile approach is used to calculate the dust emission flux.

To consider the subgrid-scale heterogenity of soil properties, the dust emission fluxes $F_{tv,l,i}$ are calculated for each soil type separately and the result is then weighted with the corresponding fraction of the soil to get the final dust emission flux:

The calculation of dust emission fluxes $F_{tv,l,i}$ for individual soil types within one grid element differs by the use of the individual soil particle size distributions $n_{s,i}(d_p)$.

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

We agree, but it is not only difficult for satellite-based remote sensing measurements but also for ground-based remote sensing measurements like sun photometers. Hence, we changed it to 'remote sensing measurements'.

Due to extensive cloud coverage, the comparison with remote sensing measurements is complicated.

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.

We did consider an interpolation, but we preferred to use nearest neighbor values. The reason for this is, that interpolating incoming short wave radiation for example in case of a cloud present at one grid point nearby and no cloud present at another grid point nearby leads to some average value which always contradicts measurements that are either influenced by a cloud or not. Furthermore, we agree that with a horizontal grid spacing of 5 km, the differences between the two methods should be sufficiently small.

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

We have added a statement to clarify this.

The total saltation flux F_{th} is the result of an integration over all saltation particle diameters weighted by the product of the cross sectional area and the number of particles (Vogel et al., 2006). This weighting represents the contribution of particle surface area at a certain diameter to the total soil surface area:

Technical comments:

For brevity, we will not reply to the technical comments and only state the changes performed in the manuscript.

- P2L33 and P3L1: Section (capital "S").

Section 2 provides an overview of the modeling system ICON-ART.

Section 3 summarizes the synoptic situation during the Saharan dust outbreak and section 4 describes the model setup.

- P4L30: No comma after "Equation 2"

Applying Equation 3, we can analytically derive the global minimum of the threshold friction velocity, which is needed in Equation 2.

- P5L1: Independent of

Independent of the size distribution

- 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".

These percentages of kinetic energy are chosen such that particles in the largest mode are emitted first when the threshold friction velocity is exceeded.

- P6L13: heterogeneity

To account for this subgrid-scale heterogeneity, a tile approach is introduced.

- P6L15: except for

defined by the U.S. Department of Agriculture (USDA) except for silt where silty loam is used instead.

- P7L7: corresponding instead of according

the result is then weighted with the corresponding fraction of the soil to get the final emission flux:

- P7L19-21: Sentence seems convoluted, please consider rephrasing.

To account for the influence of mineral dust on radiation, the optical properties of mineral dust are determined offline once. For this, Mie calculations are applied which require the complex refractive index of mineral dust (Mie, 1908) as input.

- P8L25: nucleus

Homogeneous freezing describes the formation of an ice particle without the involvement of a solid ice nucleus (IN).

- P9L17: As mineral dust particles serve as ice nuclei

As mineral dust serves as ice nuclei in ICON-ART it modifies the physical properties of the simulated clouds.

- P9L28: Albedo of the surface or of the PV module?

The surface albedo is required to calculate the reflected diffuse radiation.

Furthermore, the surface albedo and station height are necessary input parameters.

- P10L4: To my knowledge, "ridge" or "upper level high" are sufficient rather than "upper air ridge of high pressure".

On 1 and 2 April 2014, a large part of Europe is influenced by a ridge, whereas an elongated, pronounced trough lies over the eastern Atlantic.

- P13L7: in Figure 4 - P13L8: the spatial distribution

This can be seen in Figure 4 which shows the spatial distribution of mineral dust optical depth at 500 nm (in the following abbreviated with AOD) at different dates.

- 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".

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

While the 5th percentile slightly increases in case of TT, the 95th percentile that indicates the overestimation of SIS is drastically reduced.

- P16L20-21: the part "not only were large contributions of PV capacity are installed" is not clear to me.

These are not only the regions where large contributions of PV capacity are installed (see Figure 1b of Köhler et al., 2017), but also the forecast of incoming solar radiation was challenged by the presence of clouds and aerosols as already discussed in section 3.

- Fig. 8: The green and blue lines are somewhat hard to differentiate. I suggest replacing one of the colors. Caption: "pyranometer observed"

The colors within Fig. 8 have been changed. As in subsequent figures, black is used for observations, red for the FF-case and green for the TT-case. The caption has been adapted: Comparison of observed (black, by pyranometer) and simulated (TT in green, FF in red) surface incoming shortwave irradiance (SIS, a-d) and the resulting computed normalized PV power (e-h) for the stations Mannheim, Meiningen, Trier and Weihenstephan on 4 April 2014, respectively. The normalization is done with respect to peak power.

- P18L2: clouds

As mentioned before, a substantial amount of clouds was present on 4 April 2014.

- Fig. 9 Caption: corresponding difference instead of according difference

Temporal evolution of the difference in normalized PV power between TT and measurements (green line, right ordinate) and the corresponding difference between FF and measurements (red line, right ordinate) on 4 April 2014 at Mannheim (top) and Meiningen (bottom).

- 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

The integrated difference $\mathcal{ID}$ is a measure for the magnitude of the difference between TT and FF without any information on the sign of the difference.

The mean improvement ratio $\overline{\mathcal{IR}}$ is a measure for whether the TT result is better or worse than the FF result

$\overline{\mathcal{CR}}$, $\overline{\mathcal{CC}}$ and $\overline{\mathcal{CI}}$ state the mean percentage contribution of direct radiative, indirect radiative and synergistic interaction effects on the difference between FF and TT.

- 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.

The description of the factorial method at the beginning of the section is more general. The sentences you mention are specific about the factors that were chosen and point to the corresponding equations. We prefer to leave it as it is.

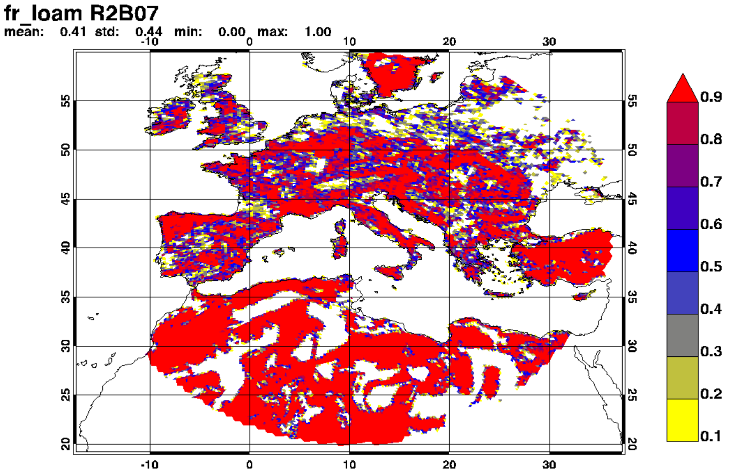

Figure 1. Fraction of loam which is used as input data for the mineral dust emission scheme. The figure was produced for the R2B07 input data, i.e. 20 km effective grid spacing.

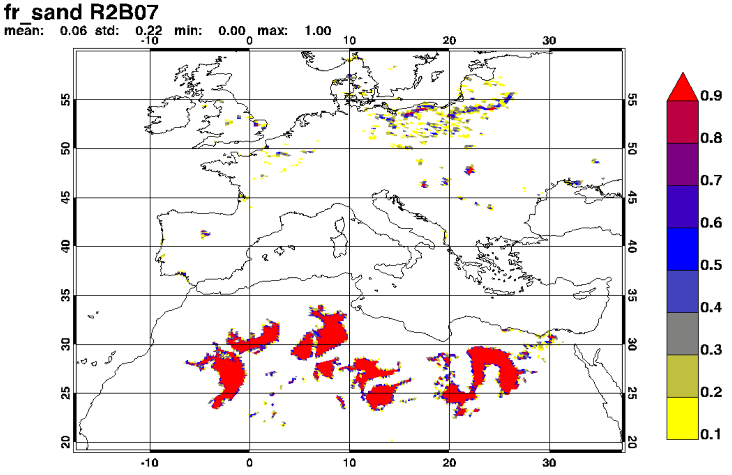

Figure 2. Fraction of sand which is used as input data for the mineral dust emission scheme. The figure was produced for the R2B07 input data, i.e. 20 km effective grid spacing.

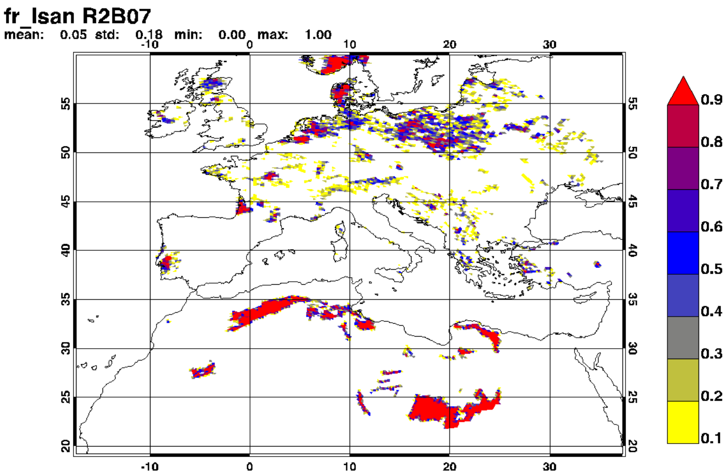

Figure 3. Fraction of loamy sand which is used as input data for the mineral dust emission scheme. The figure was produced for the R2B07 input data, i.e. 20 km effective grid spacing.

References

Alfaro, S. C. and Gomes, L.: Modeling mineral aerosol production by wind erosion: Emission intensities and aerosol size distributions in source areas, J. Geophys. Res.-Atmos., 106, 18 075–18 084, doi:10.1029/2000JD900339, 2001.

Köhler, C., Steiner, A., Saint-Drenan, Y.-M., Ernst, D., Bergmann-Dick, A., Zirkelbach, M., Ben Bouallègue, Z., Metzinger, I., and Ritter, B.: Critical weather situations for renewable energies – Part B: Low stratus risk for solar power, Renewable Energy, 101, 794–803, doi:https://doi.org/10.1016/j.renene.2016.09.002, 2017.

Marticorena, B. and Bergametti, G.: Modeling the atmospheric dust cycle: 1. Design of a soil-derived dust emission scheme, J. Geophys. Res.-Atmos., 100, 16 415–16 430, doi:10.1029/95JD00690, 1995.

Mie, G.: Beiträge zur Optik trüber Medien, speziell kolloidaler Metallösungen, Ann. Phys., 330, 377–445, doi:10.1002/andp.19083300302, 1908.

Shao, Y.: A model for mineral dust emission, J. Geophys. Res.-Atmos., 106, 20 239–20 254, doi:10.1029/2001JD900171, 2001.

Shao, Y. and Lu, H.: A simple expression for wind erosion threshold friction velocity, J. Geophys. Res.-Atmos., 105, 22 437–22 443, doi:10.1029/2000JD900304, 2000.

Vogel, B., Hoose, C., Vogel, H., and Kottmeier, C.: A model of dust transport applied to the Dead Sea Area, Meteorol. Z., 15, 611–624, doi:10.1127/0941-2948/2006/0168, 2006.