Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2015-781-RC2, 2016


Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Implementation of the Bessel's method for solar eclipses prediction in the WRF-ARW model" *by* A. Montornès et al.

**Anonymous Referee #2**

Received and published: 15 February 2016

General comment

Adding solar eclipse parameterizations to NWP models is not new, however, as pointed out by the authors, previous approaches were lacking generality and were usually designed for a particular case, only. The authors use the Bessel's method for the first time in a state-of-the-art NWP model and evaluate its performance. This work is interesting in the light of a growing demand for operational solar radiation variation forecasts (by the solar energy industry), which requires a general approach like the one described here. The paper is well structured and written. The model validation, however, should be improved. No comparison with real measurements regarding the surface layer response is included and only an idealized model setup was used (no cloud-radiation interaction and very coarse horizontal resolution). The paper can be considered for



publication after the following points have been addressed:

Specific comments

Abstract

- apart from providing a basic introduction into the topic, the abstract should focus on results/main findings. The latter is essentially lacking in the abstract. My suggestion would be to replace or extend the last section of the abstract (line 16 - 21, page 1) by the main findings. Currently lines 16-21 are basically a repetition of the introductory lines 17-22 of page 4.

Implementation in the WRF-ARW model

- The advantage of using besselian elements as compared to previous approaches (e.g. Founda et al.) should be described in more detail. Is the new approach more accurate? In what sense is it more general – or – in what sense do former approaches require manual changes to the code or input data, restricting them to single selected eclipse events?
- please elaborate a bit more on technical details of the implementation: what is the overhead/cost for this parameterization, including the setup phase (file read) in percentage of wall clock time? What is the size of the file containing the besselian elements which is read by the model? Is it read once, or opened and (partly) read at each radiation call?

Algorithm validation

The authors mention, that there are some differences between the eclipse tracks computed within the WRF module and the NASA values. However, only very vague explanations are given like “associated with small differences on the code” and “truncation errors due to compiling options”. Even though the differences are relatively small, they seem to be beyond simple truncation errors. To convince the reader of the correctness of the implementation, please point out the reasons for the observed differences more clearly. Do the differences decrease significantly when performing the computations in double precision as compared to single precision?

Results

Given the fact that even global models today use resolutions of $\mathcal{O}10\,\mathrm{km}$ operationally and even higher resolutions in less time-critical scientific applications, the resolution chosen here seems rather coarse. One consequence of this coarse resolution is that the cloud interaction has been switched off, since reproducing the observed cloud structure is impossible anyway at this resolution. This approximation may be OK for qualitative comparisons against the measured GHI values and for evaluating the qualitative response of the WRF model as it is done here. The model setup chosen for validation is, however, very different from what would be used in real applications.

The paper would benefit a lot from at least one fully fledged high-resolution run (i.e. with cloud-radiation interaction switched on) and comparison of the surface layer response against real measurements (surface temperature, wind, . . . ). One may e.g. choose the europe episode, run the model for a shrinked model domain over central europe and do a validation for Lindenberg for which high resolution data should be available.



Technical issues

- Typo in Reference to Chauvenet et al. : Chavuenet $\longrightarrow$ Chauvenet
- Fig. 1: looks a bit crowded due to the use of $6$ different colors. It may improve when using only 3 different colors for A/H/T and 2 different line styles for lat/lon.
- Fig. 2: Please add some additional shading, indicating e.g. the totality zone or $90\,\%$ obscuration area. This may help the unexperienced reader to assess more easily to what degree the various stations are affected by the eclipse.
- Table 1: please add the maximum degree of obscuration for each site.
- page 9, line 29: superfluous ”of”
- page 11, line 27: superfluous ”a” at ”a near-zero . . . “
- page 12, line 5 ”the observer (i.e. position within the model domain)”. Please add the description given brackets already at some previous occurrrence of ”observer” in order to better clarify what is meant by ”observer”.
- page 12, line 9: ”This validation show . . . ” missing ”s”
- page 12, line 32: typo ”shadowm”

Model description:

Please add some information about

- the source of the applied boundary data and its update frequency (so far only the initialization is described)

- the time step
- since a lake-point was selected for comparison with the station PAY, please document whether a lake model/lake parameterization was used by WRF (i.e. is the lake (surface) temperature prognostic, or is it constant (like SST)?

---

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2015-781, 2016.