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

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Response to "Reviewer Comments", Anonymous Referee #2

We are thankful to Anonymous Referee #2 for his, comments, suggestions and ideas. Without any doubt whatsoever, all of them are valuable to improve the quality of our study. In the current document, we will answer each one with more emphasis to those considerations that deal with technical or scientific considerations than those related to the language aspects.

We have decided to give a personalized reply to each referee. However, some points are common for both or they have a full impact on the entire paper. For this reason, we will present firstly a common block and then we will discuss each review point by point.

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Hereinafter, we will use R#1 and R#2 as Referee #1 and #2, respectively.

In order to contextualize the response, the referee's commentary appears before our answer. Each review is quoted in gray. Our response appears with A: (from Authors) at the beginning and in black color. Each one is identified with a label composed by a number and a tag: GC (General Comments) and SC (Specific Comments). For example, SC4 refers to the 4th specific comment. During the discussion, the reader can find some cross-references between responses for R#1 and R#2. For example, **SC7 R#1** means the 7th specific comment of R#1.

Some answers that are also addressed to the Editor when we defend our position but we also think that the position of the reviewer is interesting. These responses are: **SC2 R#1**, **SC7 R#1**, **SC4 R#2**.

Regarding the submission of the revised manuscript, we wait until the final Editor's decision. At that moment, we will finish the last modifications and updates and we will submit the new version.

Common comments

A: The main idea behind the study presented in this manuscript was to discuss a new package for the WRF model (extensible to other General Circulation Models in the future) capable of representing any solar eclipse for any configuration in terms of date, domain size, grid resolution or projection, among others.

Under this framework, the manuscript was divided in three parts. The first part describes the implementation of the Bessel's method in the WRF model (Sect. 2) and it includes a validation of the algorithm by comparing our eclipse trajectories and the NASA's data-set (Sect. 3). The second part includes a validation of four real study cases for the GHI that is the most directly affected variable. The selection of these episodes was based on the availability of BSRN stations. This analysis has two major goals: i) to provide a complementary validation of the algorithm for demonstrating that the degree of obscuration is well determined and it correlates in time with the real data and ii) to show the potentiality of this method for solar energy forecasting applications (Sec. 5.1). Finally, the last part of the manuscript introduces a brief description of the model response to show the applicability of this package for future academia research (Sec. 5.2).

The idea of including solar eclipses in the model born from the last partial and total solar eclipses that occurred in USA (October 23, 2014) and Europe (March 20, 2015), respectively, showing the necessity to incorporate these events into the solar parameterizations for solar renewable energy industry.

The main scope of this manuscript is the validation of the eclipse implementation by means of the trajectory and the GHI. From our understanding, some of the proposals of the referees for extending the study to the meteorological fields are very valuable as well as interesting but they are more appropriated as a future work. By including these extended analyses in the current version may blur the main flow of the study and may lead to a very large text.

The code used in this study has been shared with NCAR and we expect that it will be included in the next release (April, 2016). Consequently, the scientific community will be able to use this algorithm for a deep analysis of the model response and comparing with real measurements.

Further on the proposals for an extended study, both referees agree in the following aspects:

i) They suggested that it is necessary a different approach for the abstract (**SC1 R#1** and **SC1 R#2**) by including the main findings and conclusions as well as some qualitative results. We completely agree with this idea and we will rewrite the abstract. Following the idea provided by **SC1 R#2**, we have modified the last paragraph as:

“This contribution is divided in three parts. First, the implementation of the Bessel’s method is validated for solar eclipses in the period 1950-2050, by comparing the shadow trajectory with values provided by NASA. Latitude and longitude are determined with a bias lower than 5 10^{-3} degrees (i.e., $\sim$550 m at Equator) being slightly overestimated and underestimated, respectively. The second part includes a validation of the simulated Global Horizontal Irradiance (GHI) for four total solar eclipses with measurements of the Baseline Surface Radiation Network (BSRN). The results show an improvement in MAE from 77% to 90% under cloudless skies. Lower agreement between modeled and measured GHI is observed under cloudy conditions since the effect of clouds is not considered in the radiative transfer schemes of the simulations. Finally, an introductory discussion of the response of meteorological variables (e.g. temperature, wind speed) to the reduction of GHI and shortwave heating rate is provided by comparing WRF-eclipse outcomes with control simulations.”

ii) In **GC2 R#1** and **GC1 R#2** both referees indicated that they miss some comparison of the meteorological fields analyzed in Sect. 5.2 with real measurements. We agree with them that this kind of analysis would be interesting. Nevertheless, from our understanding, the best approach is to focus the current manuscript on the implementation of the method and prepare a future study for a better understanding of the atmosphere response with a large number of episodes and comparing with surface and vertical profile measurements.

iii) They indicated that Fig. 1 should be improved (**SC16 R#1** and **SC8 R#2**). We agree that this figure is awkward and useless with the current presentation. We have changed it as indicated in Fig. 1 of this document. We are opened to include new suggestions if necessary.

Response to general comments

GC1 R#2: 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 publication after the following points have been addressed.

A: We appreciate the considerations of R#2 regarding our manuscript. As we indicated in Common comments, we agree that the comparison of the surface fields with real measurements is quite valuable but, in our opinion, this type of validation is more appropriate for a future manuscript with a different scope and based on the current one.

The reason for disabling the cloud-radiation interaction and the coarse grid resolution will be argued in **SC5 R#2**.

Response to specific comments

Abstract

SC1 R#2: 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.

A: This suggestion and **SC1 R#1** indicate that it is necessary a new abstract improving the weak points that you have observed. You can see the new version in Common comments.

Implementation in the WRF-ARW model

SC2 R#2: A schematic representation of besselians elements showing the outline of umbra during eclipse on Earth's surface and its projection on the fundamental plane would be helpful.

A: Generally, the previous approaches used pre-computed solar eclipses or some kind of parameterization based on the eclipse track. The set of values used in the computations were provided by NASA catalogs and hence, they were indirectly based on the Bessel's method.

The advantage of our method with respect to the previous ones is that we incorporate one part of the Bessel's approach (Appendix A) inside the model in terms of the besselian elements. Consequently, the eclipse is evaluated during the simulation and thus it is independent of the grid size, resolution, projection and initialization.

A complementary response to this comment can be found in **GC1 R#1**.

SC3 R#2: 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

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each radiation call?

A: In the following lines we will provide the technical details of the implementation. We have included a new module called module_ra_eclipse composed by three routines,

solar_eclipse	main,
load_besselian_elements	search the besselian elements in run/eclipse_besselian_elements.dat,
compute_besselian_t	compute the besselian elements for a given time.

Moreover, we include five new variables in the Registry:

ra_sw_eclipse	namelist (physics) variable for enabling (1) and disabling (0, default) solar eclipses. No domain dependent
ECOBSC	history 2D variable representing the degree of obscuration at each grid-point
ECMASK	history 2D variable representing the status of the solar eclipse at each grid-point (0- No eclipse, 1- Partial/Penumbra region, 2-Total, 3- Annular)
elon_track, elat_track	coordinates of the path of the eclipse.

At each call of the radiative transfer scheme, controlled by the radt variable, we check if the eclipse exists for that time step. We load the entire file called eclipse_besselian_elements.dat, every time and we check if the eclipse exists. If any episode exists then, we compute the eclipse conditions at each grid-point, if not, we come back to the main flow.

These processes do not require many computational time. In fact, when you compute the mean time for each time-step, the noise produced by the machine (i.e. other processes, programs, etc) has a higher effect than this new implementation.

The code is compatible with both cores: the ARW and the NMM. In the first case, with the solar schemes Dudhia, Goddard, New Goddard, CAM, RRTMG, RRTMG-fast and FLG and in the second one with the default scheme. This new implementation has been tested and shared with NCAR to be included in the next release.

Finally, the file eclipse_besselian_elements.dat is an ASCII file with a size of 4.5 kb. The processes of reading the file every time is not optimal from a programming point of view, but it is a recurrent approach in many parts of the model.

We are not really sure if this kind of information has to appear in the manuscript because it is not a technical report.

Algorithm validation

SC4 R#2: 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?

A: We agree that there are not many details regarding this point. The reason is because the errors are low for mesoscale applications and we considered that this kind of information was irrelevant for the reader.

We compared the computations by using double precision instead of single precision (Figs. 2 and 3). In this case, the bias in latitude becomes zero while in longitude is reduced by 5 (i.e. 110 m as a maximum).

The remaining differences in longitude evaluation are produced by two constants: i) the Earth's eccentricity and ii) the correcting factor for the true longitude considering the non-uniform rotation of the Earth (Eq. 40). Both constants are taken with single precision because we could not find data-sets with more precision. Moreover, by using double precision, the computational time increased and the improvement in accuracy is not significant for most of the WRF applications in which solar eclipses can be enabled. Consequently, we decided to use single precision variables.

Finally, there are higher order effects as the compilation option or even the compiler that can lead to small differences in the results.

This information can be included in the manuscript, but from our perspective is not really relevant for most of the potential readers. We wait until the Editor's decision.

Results

SC5 R#2: Given the fact that even global models today use resolutions of O(10) 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.

A: We completely agree with this statement. Having said this, from our perspective the used methodology is enough for the purposes of this study.

As we explained in the "Common Comments", the scope of this manuscript is to set the basis of a new method for studying solar eclipses with the WRF model. Therefore, we focus the paper on the method and on the validation of the algorithm instead of a deep analysis of the model response.

Consequently, as we are mainly interested in the representation of the eclipse but not in the accuracy of the meteorological fields, we created big domains with coarse resolution. The idea of creating domains covering a large area is to include the shadow during all the episode.We chose a coarse resolution for two reasons. Firstly, but irrelevant, because of the computational cost. Secondly, because we disable the cloud interaction in the radiative transfer and thus, the GHI varies slowly in the horizontal (i.e. homogeneous in the sub-grid).

Of course, this initial study can be improved with higher order modeling approaches such as enabling clouds, increasing the horizontal and vertical resolution or performing a sensitivity tests of the best options for each site. Nevertheless, we think that these ideas are more appropriate for future works because in this study may distort the main scope.

The response to this comment links with **SC6 R#2**.

SC6 R#2: 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.

A: This comment is directly linked with **SC5 R#2** and **SC9 R#1**. Certainly, we agree that this kind of analysis can be very valuable but, in our understanding, they are not appropriated for this study.

The main scope of the manuscript is the implementation of the Bessel's method within the WRF-ARW model and a validation of the algorithm but not a full study of the impact of solar eclipses in the atmosphere and the reliability of the models for modeling this response.

Therefore, these ideas can be developed in future studies more focused on the response of the atmosphere that can be based in the work proposed in this manuscript.

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We propose to include a new paragraph incorporating all these ideas in the Conclusions.

Technical issues

SC7 R#2: Typo in Reference to Chauvenet et al. : Chavuenet -> Chauvenet.

A: It was a typesetting error. We have corrected the reference accordingly.

SC8 R#2: 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.

A: The answer to this comment links with **SC14 R#1**. We agree that this figure must be re-plotted. R#1 suggested a reduction of ticks in the x-axis. You can see the new Figure in Fig. 1.

SC9 R#2: 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.

A: This is an excellent idea. We will include a new version of Fig. 2 adding this information.

SC10 R#2: Table 1: please add the maximum degree of obscuration for each site.

A: This information appears in Fig. 3, but we will include this information in Table 1 to make the manuscript easier to read.

SC11 R#2: Page 9, line 29: superfluous ”of”

A: Of course. We have removed the superfluous “of”.

SC12 R#2: Page 11, line 27: superfluous ”a” at ”a near-zero . . .

A: Sure. The superfluous “a” has been removed.

SC13 R#2: Page 12, line 5 ”the observer (i.e. position within the model domain)”. Please add the description given brackets already at some previous occurrence of ”observer” in order to better clarify what is meant by ”observer".

A: We will provide this information earlier to make the text easier to read.

SC14 R#2: Page 12, line 9: ”This validation show . . . ” missing ”s”.

A: We agree. This part has been reworded accordingly.

SC15 R#2: Page 12, line 32: typo ”shadowm”

A: It is typesetting issue. This word has been reworded.

Appendix B: Model description

Please add some information about

SC16 R#2: The source of the applied boundary data and its update frequency (so far only the initialization is described).

A: We use the ERA Interim Reanalysis with an update frequency of 6 h (i.e. the available output of this model) as initial and boundary conditions. This information will be included in the Appendix B.

SC17 R#2: The time step.

A: We use an adaptive time-step, for this reason it was not included in the model configuration. The first guest is set to 30 s and we set a target CFL condition of 1.2. The time step can not increase more than 60 s because this is the output frequency of the history file. This kind of information will be included in Appendix B.

SC18 R#2: 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).

A: We use the default option, i.e. without lake model. As it is discussed in **SC5 R#2**, the initial idea of this manuscript was not to perform an accurate description of the variables at each site. The description of the treatment of the lake surface will be included in Appendix B as you requested.

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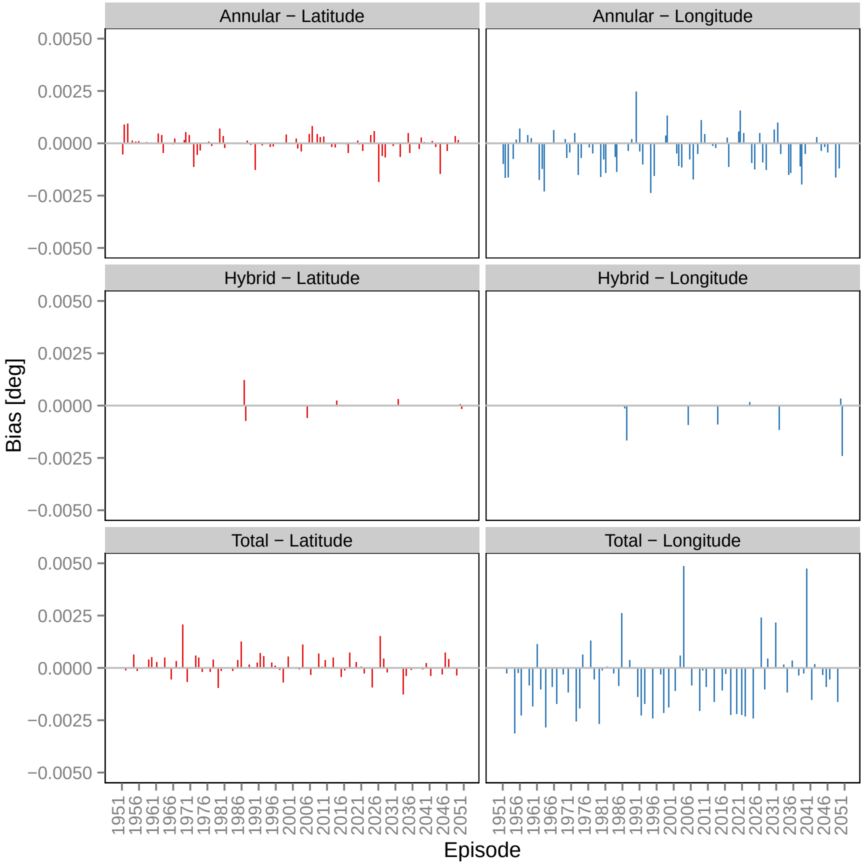

Fig. 1. New version of Fig. 1 in the manuscript.

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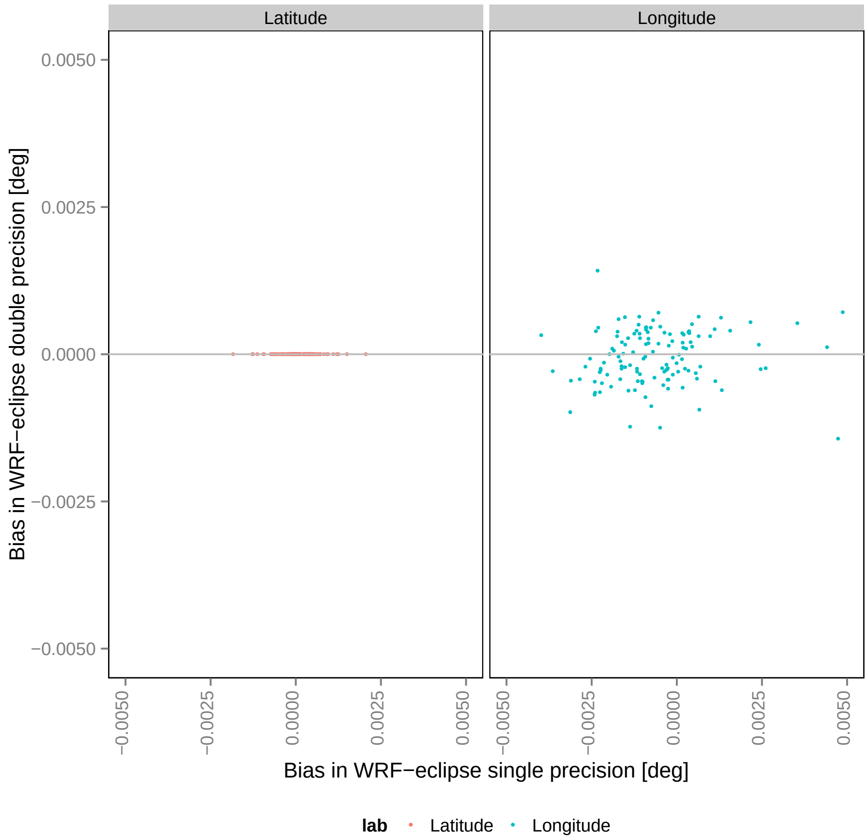

Fig. 2. Scatterplot comparing the errors in latitude and longitude considering single and double precision.

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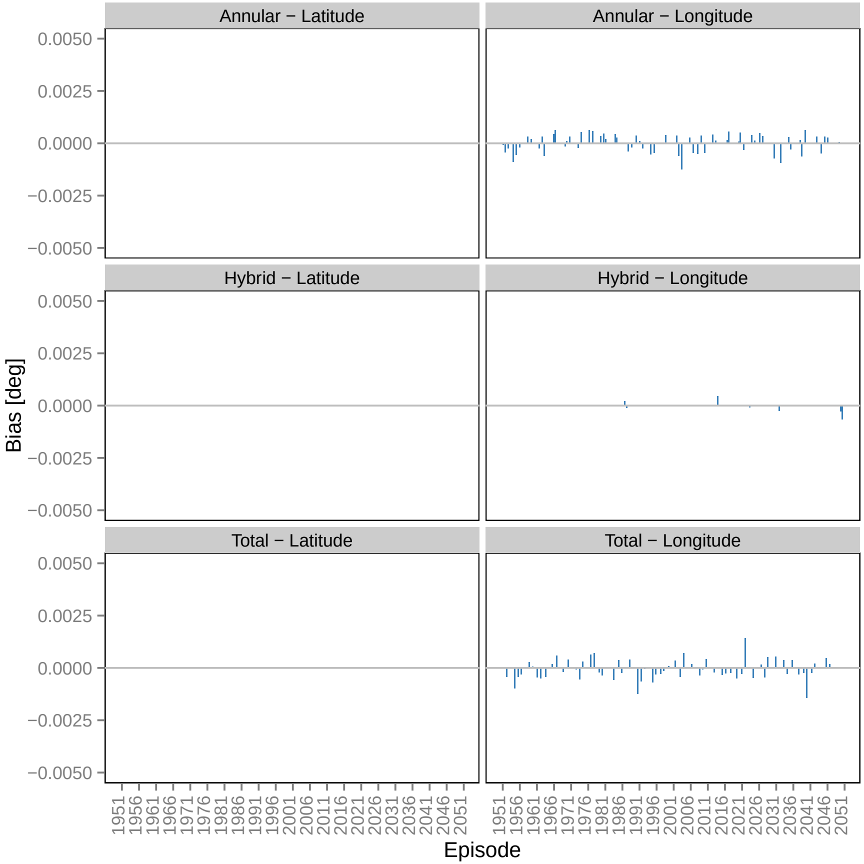

Fig. 3. Equivalent to Fig. 1 in the manuscript using double precision.

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