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Interactive comment on "Calibration of the DSCOVR EPIC visible and NIR channels using MODIS and EPIC lunar observations" *by* Igor V. Geogdzhayev and Alexander Marshak

Igor V. Geogdzhayev and Alexander Marshak

igor.v.geogdzhayev@nasa.gov

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We would like to thank the Anonymous Referee #1 for their insightful comments which helped us generate what we hope is a much improved manuscript. The paragraphs with the Referee's comments below start with ">" symbol, followed by our responses

>Summary. >This study calibrates the EPIC imager channels with corresponding MODIS band calibration using two different methods, that were found to agree. The application of the SBAF and accounting for stray light show that the regression offsets are closer to the true instrument offset of zero in version 2 EPIC. The EPIC absorbing channels were calibrated using lunar targets after adjusting for a small spectral shift.

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The absorbing channel calibrations using this method were compared to ROLO and was found to be within 10%. This paper is ready for review after the following issues have been addressed.

>I agree with the other reviewer. What is the mission of DSCOVR? Why is the calibration needed? What is being retrieved from DSCOVR? Why are the channel spectra so narrow? Must be for trace gas retrieval, such as ozone. I can't believe that there are no DSCOVR publications that can be cited in this paper.

We agree with this comment.

As far as we know the EPIC channels' width is a part of its filter-wheel design.

We have significantly extended the description of the scientific applications of the EPIC data and added multiple references describing these applications in detail. The end of the first paragraph in the introduction section was modified as follows: "Thanks to its position and viewing geometry, the EPIC instrument offers an improved temporal sampling compared to instruments on the sun-synchronous orbit. It samples the entire sunlit hemisphere 10-20 times per day. Compared to other instruments on geostationary orbit, EPIC provides improved coverage in high latitudes hemispheres. It thus has the potential to augment remote sensing observations in such applications as aerosol, cloud, sulphur dioxide and ozone amounts as well as vegetation properties (Marshak et al., 2017a). EPIC data are used for the remote sensing of height and optical depth of dust plumes using oxygen A and B bands (Xu et al., 2017, Yang et al., 2013) and multi-spectral UV SO2 measurements of the sunlit Earth disk (Carn et al., 2016). EPIC measurements are applied to the estimation of leaf area index and its sunlit portion (Yang et al., 2017; Marshak and Knyazikhin, 2017) as well as measuring the ozone, cloud reflectivity and erythemal irradiance (Herman et al., 2017). EPIC measurements were used to observe the terrestrial glint from oriented ice crystals by (Marshak et al., 2017b)."

> Page 1 line 30. Can you also provide the range of the scattering angle for a sunsynch

satellite such as NPP as a reference?

We added the following explanation to the first paragraph in the Introduction section: “For comparison, depending on the season, latitude and scan view angle, the scattering angle for MODIS is typically in a wide range of between 110° and 175°. The Suomi-NPP VIIRS instrument, due to its wider range, covers even larger range of angle including the whole backscattering region.”

> A large scattering angle increases the uncertainty of a retrieval, for example clouds, since it is nearly in direct backscatter. Can you state what retrieval would benefit from such a large scattering angle?

We agree that large scattering angles may present challenges for some retrievals. However they may also be desirable for other applications. We therefor added the following discussion to the top of page 2: “The almost back scattering EPIC observations are a direct consequence of its position at L1. The large scattering angle of EPIC observations is a significant difference compared to the observations from low orbit instruments. The large scattering angles may present challenges for some retrievals. However they may also be desirable for other applications. For example, the position of the water surface glint in the center of the sunlit hemisphere allows better coverage where LEO instruments often see glints as big in the Indian Ocean. Also of note is the lack of shadows for vertically extended scenes. Measurements in the backscattering region allowed to observe and characterize the glint caused by oriented ice crystals in clouds (Marshak et al. 2017b). Availability of these measurements also allow to better characterize BRDF of vegetation (Yang et al 2017; Marshak and Knyazikhin, 2017). Using the back scattering region it is possible to get Leaf Area Index of diffuse and sunlit leaves separately; this is important because they have different photosynthetic rates.”

> Can you also state that a satellite in L1 would have to orbit L1 in order to be in the L1 orbit. Why is the range of SEV decreasing over time? Is the orbit about L1 maintained?

We added the following explanation to the first paragraph of the Introduction section: “The spacecraft actively maintains itself to be in a Lissajous orbit around L1.”

According to the explanations we received from the mission control people the range of the variation of the Solar-Earth-Vehicle angle (SEV = 180o – scattering angle between solar and viewing directions) has been decreasing due to the evolution of its orbit since the launch but is expected to start widening again.”

> Page 2 line 16: The MODIS channel reflectances are not truly reflectances, that is dependent on the solar zenith, but a scaled radiance, that is divided by the solar constant of the channel. The reader needs to be aware of this in Fig. 4

We agree and included the following clarification to the second paragraph in the Data Section: “Note that the MODIS reflectance, as well as EPIC, is the true reflectance multiplied by the solar zenith angle (MODIS Level 1B Product User’s Guide, 2006). We will refer to this quantity as simply “reflectance”.

> Page 4 line 5: The pixel-level homogeneity threshold was set as a function of channel. Can the range of the spatial homogeneity threshold be given as a percentage of the mean pixel value? Was the spatial homogeneity threshold the greatest limiting factor of the number of EPIC and MODIS pairs?

Yes. We modified the end of the second paragraph of the Analysis section as follows: “The relative standard deviation of MODIS and EPIC points included in the regressions was between 0.5% and 1% depending on the channel. In this approach the spatial homogeneity threshold was the greatest limiting factor to the number of EPIC and MODIS pairs.”

> Fig. 4: Can the authors identify the 3 groups of reflectance pairs. Is it clear-sky ocean, clear-sky Saharan desert, and bright clouds. Is the strict pixel-level standard deviation threshold, screening out more bright deep convective clouds or maritime stratus clouds? Each of these scene-types would require differing spectral band adjustment

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factors.

“One may assume that bright pixels mostly represent cloudy scenes, dark pixels are mostly from water and vegetation, and intermediate values represent deserts. However, it would be impossible to classify the pixels with certainty within the framework of the crude approach, where SBAFS for clouds are assumed for bright scenes with reflectance greater 0.6 and a fixed surface classification map is used. This represents a limitation of the current approach and may be responsible for some spread in the regressions.” We added the above comment to the first paragraph of the Spectral correction section. Please also see our response (*)

> Table 2: Do the calibration coefficients that are published assume a zero offset? When comparing M/E ratio, does this represent calibration approach one with a zero-offset?

Using the officially published gain coefficients implicitly assumes a zero offset. We provided offset values to each retrieval team. As far as we know, they were used by the aerosol retrieval team.

We included the following explanation before the last sentence in the Analysis Section: “The differences in the gain coefficients calculated using the two methods given in Table 2 which shows the officially published gain coefficients for the two dataset versions. These coefficients are also available at https://eosweb.larc.nasa.gov/project/dscovr/DSCOVR_EPIC_Calibration_Factors_V02.pdf. When comparing the gain coefficients from the two methods below we do not force the regression through zero. Doing so would reduce the independence of the two approaches as it would effectively ignore the contribution of the dark scenes to the regression.”

> Table 2: can both approach 1 and 2 calibration coefficients also be added to Table 2.

We agree to the request in principle and the Version 2 calibration coefficients for both methods are now available elsewhere in the paper (please see our response (**)). The calibration coefficients for the Version 1 have only limited usefulness since the improved Version 2 data is out and new data is processed by the Version 2 algorithm. We believe that Table 2 provides the official calibration coefficients and adding more values to it may be confusing to the user.

> Fig. 5: Why do you believe that there is a dependency of the EPIC gain with the MODIS/EPIC ratio standard deviation? In order to justify a linear regression based on the ratio standard deviation to find the true ratio. Why do believe this is systematic rather than random?

We added the following discussion to the end of the Analysis Section. A dependence of the M/E ratio on the relative standard deviation of the MODIS pixels may potentially exist because of the different effect of the scene's cloud or surface inhomogeneity on the two instruments due to the different viewing geometry. However, this approach does not assume its existence, as the gain coefficients are obtained from the extrapolation to the "ideal" case of completely uniform scene thus accounting for any potential systematic behavior. If the relation to the standard deviation is completely random the resulting coefficients will be similar to what one would obtain by simply calculating the mean M/E ratio.

> Fig 5: Does the EPIC instrument angular configuration allow for sunglint? I guess since sun-glint is only a forward scatter feature, this would not be the case. Bright sunglint can also exceed 0.6 reflectance.

We agree and added the following sentence to the paragraph before the last one in the Analysis section: "Because EPIC observations are made in the backscattering region the sunglint usually occurs in the center of the image. Bright sunglint can exceed 0.6 reflectance however such scenes are not spatially homogeneous and are screened out by the relative standard deviation requirement"

> Page 6 line 10. How confident are the authors that the bright clouds are deep con-

vective, rather than maritime stratus, which have differing SBAFs.

(*) We agree that it may not be possible to distinguish between the deep convective and maritime stratus clouds using the simple brightness threshold. The cloud identification problem presents itself for the SBAF derivation as well (Scarino et al., 2016). To evaluate the possible effect we rerun the fitting procedure using the “Approximate DCC” SBAF values from Scarino et al. (2016) which represent a collection of bright tropospheric clouds instead the “Precise DCC” classification, used in this paper. We found that the resulting difference in the calibration coefficients was on the order of 0.1% - significantly less than other sources of uncertainty.

> Page 6 line 18. Intermediate brightness scenes. Since there are so few EPIC MODIS reflectance pairs, could not the authors identify the actual scene. It is likely that these scenes are clear-sky deserts, since the deserts are more spectrally red than clouds, have a very different SBAF as shown in Fig. 4, than bright clouds.

We agree that the intermediate brightness scenes are likely to be deserts. One can not completely exclude partial cloud contamination however. We believe this is a limitation of the current approach and a source of uncertainty. Please also see our response to the comment about Figure 4 above and the resulting paper modifications.

> Figure 7. Its good to see the SBAF correction changes the linear regression offset closer to the true space offset of 0.

We agree

> Where are tables 3 and 4?

We have corrected this typo. Table 5 was renamed to Table 3.

> Table 5. Could the authors add to table 5, the actual EPIC gain factors from both methods and a recommendation of which EPIC calibration gains to use?

(**) We agree. The recommended (published) gain coefficients for EPIC version 1 and

2 data are given in Table 2. We now include the gain factors from both methods. For the regression method they are shown on Figure 4. For the M/E ratio method they are now included in the caption of Figure 5.

> Page 7 line 1: are you trying to find seasonal dependence of the calibration method or EPIC sensor seasonal dependency. Can this be more clearly stated. First of all do you expect any sensor degradation of EPIC? The sensor is at L1 where there is so little reflected solar exposure to the optics. Evaluating a seasonal cycle, with one seasonal cycle is difficult. After 2-years than the actual seasonal dependence can be determined with more certainty. It is also interesting that the larger ratio disparities have a seasonal cycle.

We agree with this comment and we modified the beginning of the “Seasonal dependence” as follows: “The length of the available EPIC dataset allowed us to evaluate the magnitude of any possible temporal change in the derived calibration coefficients. Such a change may potentially be due to two distinct factors: seasonal dependence of the calibration method itself or the degradation of the EPIC instrument. With the data covering only one full seasonal cycle it may be difficult to reveal a seasonal dependence of the calibration procedure and thus separate the two factors. However observing no or small temporal change would be an encouraging sign of both the stability of the instrument and the robustness of the calibration method.”

> Page 7 line 18. Regarding, the 0.688 and 0.680_m lunar reflectance difference of 1.6%. Is that the bright portions of the moon or the dark portions? I guess what I am asking do you use the complete lunar disc to get the ratio between the 0.688 and 0.680_m channels. Do you account for lunar phase and libration? ROLO section. Did Tom Stone offer guidance to prepare the EPIC data to be compared with the ROLO model?

We added the following explanation to the section 7“. To calculate the ratios we used both full moon and new moon data separately. To avoid the effects of libration the

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edges of the disk were ignored."

Figure 1 shows that the F(680,688) and F (780,764) ratios for moon observations agree to within 1% and 2% respectively. The smaller size diamonds represent better data fit. It also shows that the new moon observation (circled) agrees well with the rest of the data.

Figue 2 shows the removal of the edge pixels of the moon image (in red, right panel) and examples of the EPIC moon views (left panels)

We extended the following acknowledgement to read: "We are grateful to Tomas Stone for providing the ROLO-derived calibration coefficients for EPIC and for the help in preparing the EPIC data for the use with ROLO model."

> Conclusions, Page 9, line 8. Can you provide the EPIC calibration gains for the 0.764_m and 0.688_m here and some text why you recommend it?

We added a reference to the values reported in Section 7 and included the following sentence: "The values are therefore consistent and may be recommended for use together with the MODIS-derived coefficients for the non-absorbing channels (Table 2).

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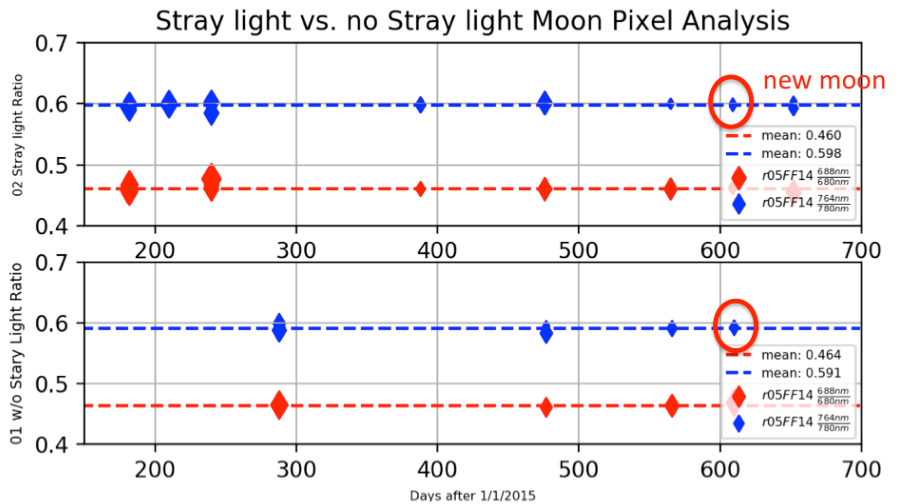
Lunar calibration of the O2 abs. channels

F(680,688)

and

F(780,764)



Courtesy of Marshall Sutton

17 images without the stray light cor. and 36 images with the stray light cor.

The difference in 764/780 ratio < 2%, and the difference in the 688/680 ratio < 1%.

Fig. 1.

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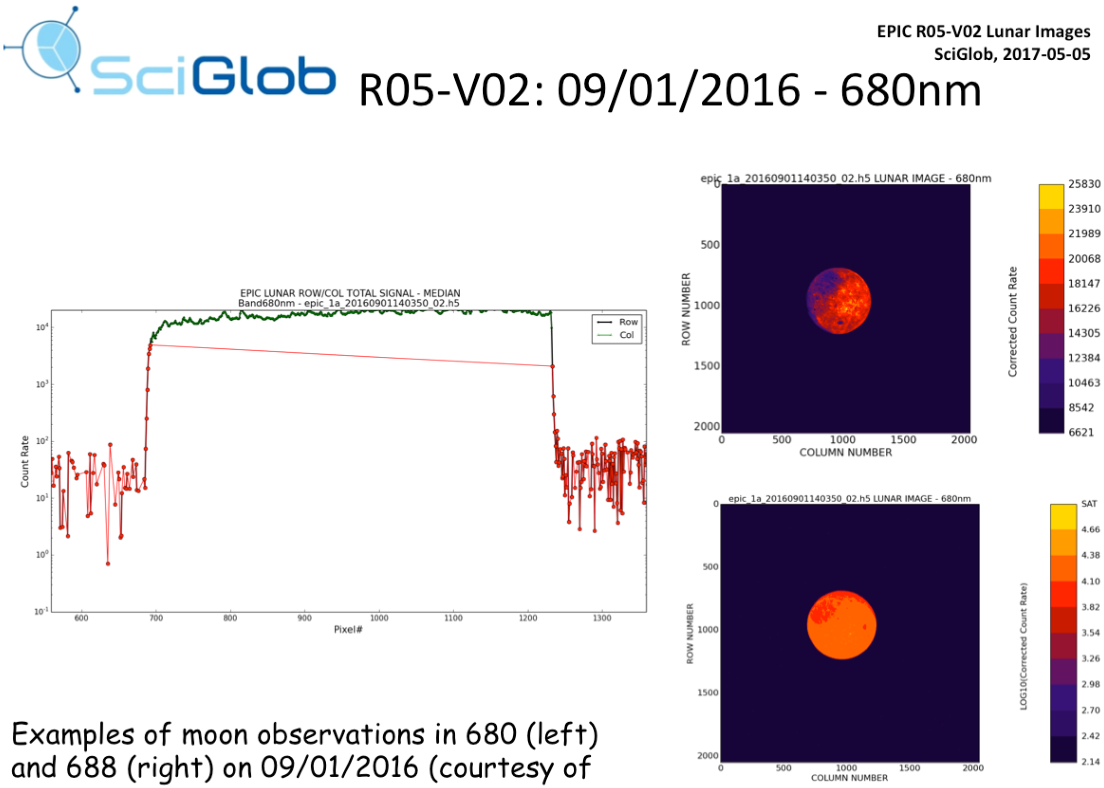

Examples of moon observations in 680 (left) and 688 (right) on 09/01/2016 (courtesy of Matthew Kowalewski).

Fig. 2.