# Response to reviewers

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## Introduction

We are grateful to Seiji Kato for further comments on the manuscript "Impact of reflected shortwave anisotropy on satellite radiometer measurements of the Earth's energy imbalance" (egusphere-2025-829). The original comments are reproduced in black below, with our responses in blue.

A full record of the exact modifications in the revised document are available as a manuscript with tracked changes, uploaded separately.

## Comments by Seiji Kato

The authors wrote: However, because we specifically consider the global mean values, these can be translated to equivalent global mean values at an alternative control altitude by a straightforward (radius1/radius2)2 conversion factor thanks to conservation of energy.

The point I want to make is nothing to do with energy conservation. To illustrate the point, I use one example. Suppose a nonscanner footprint contains very dark surface and one small bright spot. The irradiance inferred from the non-scanner measurement depends on the location of the bright spot. The irradiance is different if the bright spot is located at the nadir or off the nadir. Also how irradiance changes with moving the altitude of the nonscanner depends on the location of the bright spot. An extreme case is when the bright spot is located near the edge of the field-of-view. Lowering the nonscanner altitude leads to moving the bright spot outside the field-of-view. This example illustrates that we need to know scene type to estimate the irradiance by change the altitude to a reference level. This is caused by the fact that we cannot measure the irradiance leaving Earth everywhere instantaneously. Only we can measure irradiances leaving a highly spatially and temporally nonuniform object (Earth) piecewise. I believe that the question that this paper is addressing is that whether one can neglect complexity of scenes within nonscanner filed of views and assume isotropic radiances to measure net irradiance to within a sufficient accuracy (better than 0.5 % in shortwave). Because scene within field-of-view is the central issue of this study, you need to use Level 2 data (i.e. CERES SSF data product) to do the study correctly. I do not think that coarse 5 degrees by 5 degrees scenes provide a rigorous test.
We agree that the instantaneous irradiance depends on the altitude of the nonscanner and on the location of e.g. bright spots within the footprint. The cited comment was intended as a comment on the long-term (in our case annual) global mean, essentially considering imaginary surfaces at different altitudes as with the Poynting-Mishchenko approach.
We argue that our method actually follows the Poynting-Mishchenko approach, which avoids scene-type complications (see later comment below).
To clarify the scene classification and the grid size: Within the field of view, the scenes are specified on the same 1°x1° grid as the reference input fluxes. Each individual wide-field-of-view measurement is computed as the sum of contributions from each visible grid cell on this 1°x1° grid. The coarser 5°x5° grid is only used later in the processing, for binning the individual measurements (see Sect. 2.5 in the manuscript).

In the manuscript, we have clarified that the scenes are determined on the same 1°x1° grid as the CERES input data.

In section 2.3, you jump to test samplings by four different orbits. Given the question mentioned above, I do not understand why you need to test sampling by orbits here. You need to test the scene type issue first with SSF data product. Then you can show that the accuracy does not depend on orbit. If you like, I am happy to discuss and help you on investigating those.
(We presume that this comment refers to Sect. 3.2 Angular sampling.)
We first present our computed annual errors in Sect. 3.1, in which we mostly find only small differences between the Lambertian and anisotropic cases. Given that the focus of the manuscript is on the impact of reflected shortwave anisotropy, we find it natural to continue in Sect. 3.2 by investigating why the differences are generally small. By construction, the Lambertian and anisotropic cases differ only in the angular distribution of the reflected shortwave radiation, and we therefore choose to investigate the differences by quantifying the angular sampling of the four orbits. Regarding the scene issue, we argue that because our method follows the Poynting-Mishchenko approach, this is not a problem (see next comment).

There might be a way to avoid complications associated with scene type within the field-of-view. The approach is based on the Poynting theorem and discussed in Mishchenko et al. (2016). The nonscanner measurements provide electromagnetic energy crossing an imaginary sphere at the satellite orbit. Given the incident radiation is known, the power absorbed within the imaginary sphere can be inferred from nonscanner measurements of scattered power. The absorbed irradiance can be derived by dividing the absorbed power by the area of the sphere with a reference radius. You still need to assure complete and uniform spatial and temporal samplings on the imaginary sphere probably by gridding the measurements. Besides assuring uniform samplings, I think the challenge of this approach (or both approaches) is to separate directly transmitted solar radiation through the atmosphere from radiation

scattered by the atmosphere in the forward direction. Continuously increasing the radiance near the edge of the earth can be seen in Figure 3 of Kato and Loeb (2003).
This approach is indeed what we are trying to do. After computing the individual wide-field-of-view measurements, we bin them on the coarse 5°x5° grid that corresponds to our imaginary sphere. From there, we compute the global mean i.e. our absorbed irradiance.
In terms of the spatial and temporal sampling, we previously found that with a single satellite that makes measurements every minute, each of the 5°x5° bins is populated almost daily on average, with only minor differences in space and time (Hocking et al., 2024).
As for the separation of directly transmitted versus forward-scattered radiation, we agree that this is an interesting challenge. As stated in Sect. 2.1, we do not consider transmitted radiation in the current study, but we intend to investigate this as part of future work, and appreciate the mention of the Kato and Loeb (2003) reference in this context.

Satellite orbit. If sun-synchronous orbits are considered, we need two satellites to capture the diurnal cycle of clouds for accurate daily or monthly mean irradiances. We do not know how other orbits combinations perform but Terra and Aqua who are separated 3 hours around the local solar noon were able to capture the diurnal cycle (Loeb et al. 2018).
In the current study, we focus on the global annual mean. For our calculation of the global annual mean, we use a different method than what is used for the post-processing of data from Terra and Aqua.

Minor comments
The line numbers in these comments appear to refer to the public preprint version of the manuscript, rather than the revised version that was prepared in response to the first round of referee comments. Some of the comments were addressed in that previous revision. Our comments below reflect the contents of the latest version, detailing changes that were made as part of either the first or second revisions.

Section 2.2.2 is still harder to understand. Here are my suggestions.

Equation (1): I do not understand what “the radiant exitance of the surface element” means. But your reply said it is flux. If M is flux and IL is isotropic radiance, you do not need cos(theta) in here. Il is only a function of solar zenith angle.
Radiant exitance is indeed a flux. Specifically, it is the power per unit area emitted by the surface element. This has been clarified in the manuscript.
The equation now reads:

$$L_{Lam} = \frac{M}{\pi}, \tag{1}$$

where $L_{Lam}$ is the Lambertian radiance and M is the radiant exitance.

Line 125. Change Lambertian emission to isotropic emission.
This now reads: ”Lambertian reflectance”.

Line 127. I do not understand what anisotropic emission means if you are discussing emission here. Perhaps anisotropic radiance field?
This now reads: ”anisotropic radiance”.

Line 129. Change Lambertian radiance to isotropic radiance.
This still reads ”Lambertian radiance”, because we want to make it clear that this is the radiance in our Lambertian reference case. The current phrasing also explicitly connects this to the term $L_{Lam}$ that is used in Equations (1) and (2).

Equation (2): Again, Il is only a function of solar zenith angle.
The equation now reads:

$$R_{scene}(\theta_0, \theta, \phi) = \frac{L_{scene}(\theta_0, \theta, \phi)}{L_{Lam}} \tag{2}$$

Page 7: reflected shortwave irradiance from a twilight zone (solar zenith angle greater than 90 degrees) is between 5 to 10 Wm-2 (see Figure 2 of Kato and Loeb 2003).
A comment on this twilight irradiance has been included, with a reference to Kato and Loeb (2003).

References

Kato, S. and N. G. Loeb, 2003: Twilight irradiance reflected by the earth estimated from clouds and the earth's radiant energy system (CERES) measurements, J. Climate, 16, 2646-2650.

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Mishchenko, M. I., J. A. Lock, A. A. Lacis, L. D. Travis, and B. Cairns, 2016: First-principles definition and measurement of planetary electromagnetic-energy budget, J. Optical Soc. America A, 33, 1126-1132.