Response to review of "The spectral signature of cloud spatial structure in shortwave irradiance" by anonymous Referee #2

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We thank the reviewer for the positive assessment of the manuscript and the helpful comments regarding the clarity and cohesion. We shortened the introduction, leaving out unnecessary references to the radiance aspect of the problem, which helped the cohesion of the manuscript. Owing to the reviewer's positive feedback, we kept the conclusion section largely unchanged. Regarding the applicability of our parameterization, see our response to general comment #2. [Note that page/line numbers refer to the original, not the revised manuscript.]

Assessment: Minor Revisions

General comments:

#1 There are several places in the manuscript related to radiances instead of irradiances (e.g., p17, l11ff) . For the flow of the paper discussions concerning the relation between H and radiance measurements by satellites should be shifted to the end of the paper.

This is an excellent point; the other reviewer had a similar comment. The discussion of radiances interrupted the flow of the paper; we removed multiple occurrences in the body of the paper and discussed it mainly in the conclusions. Rather than going into too much detail in this paper, we instead included a reference to a Ph.D. and the companion paper (Song et al. 2016, to be submitted soon). Changes are highlighted in the revised version of this paper.

References:

Song, 2016: The Spectral Signature of Cloud Spatial Structure in Shortwave Radiation, *Ph.D. thesis, University of Colorado at Boulder.*

Song, S., K. S. Schmidt, Pilewskie, P., King, M. D., Platnick, S., 2016: Quantifying the spectral signature of heterogeneous clouds in shortwave radiance and irradiance measurements, to be submitted to *JGR SEAC^{4}RS special issue*

#2 It is not completely clear how to use your findings for other users. How can we improve for example layer properties calculations from airborne irradiance measurements with respect to horizontal photon transport?

The other reviewer also brought up this point. Indeed, the term "parameterization" might suggest that it can be exploited for inferring, simplifying, or correcting 3D effects, and the authors are currently working on this topic. However, the parameterization is only the first step towards this goal, and it cannot (yet) be translated into such immediate practical applications, although this is certainly the goal for the future. The purpose of the parameterization is to capture the relationship between net horizontal photon transport and its spectral dependence using one main parameter (ε). The companion paper (Song et al., 2016) will look at the connections between 3D effects on irradiances and radiances. We will include this explanation in the revised version. For example, we conclude the abstract with the following statement: "Since three-dimensional effects depend on the spatial context of a given pixel in a non-trivial way, the spectral dimension of this problem may emerge as the starting point for future bias corrections." In section 6, we included this statement "Although our study was instigated by aircraft measurements, its findings are also relevant for satellite-based derivations of cloud radiative effects since the spectral perturbations *d*λ propagate into observed radiances (Song et al., 2016). This may be exploited in future applications for deriving correction terms for 3D radiative effects via their spectral signature." We hope this clarifies the purpose of the parameterization.

Specific comments:

#1 In the last sentence of the abstract the authors mention a companion paper. It is not necessary to refer to this publication in the abstract. Rather the authors should give an example how and where the parameterization can be applied for other users.

We made this change. We also added an outlook as final sentence in the abstract, which makes clear how the correlations and the parameterization may be used in the future ("Since three-dimensional effects depend on the spatial context of a given pixel in a non-trivial way, the spectral dimension of this problem may emerge as the starting point for future bias corrections.") At this point, the parameterization is useful to understand measurements of horizontal photon transport in inhomogeneous scenes, and can essentially be used as "fitting function" for the spectra with the free parameter ε. We will include a statement to this effect in the next paper (Song et al., 2016). In fact, this has already been done in the Ph.D. thesis (Song, 2016) which will become available for download on 8/18/2016. Once this happens, we will include a link and reference in this paper.

#2 (p3, l7) "can assume similar values as the absorbed irradiance"; Comparing the apparent absorption shown in Fig. 4a (500 nm) and 4b (1600 nm) in Schmidt et al. (2010) I identify the more the same magnitude than similar values. It's still a variable factor between the numbers. Use "same magnitude" instead "similar values". In addition, the authors should give reasons for smaller H-values in the NIR.

We changed the wording slightly to make this distinction. We actually did not say that H values are smaller in the NIR; we only compared H (VIS) to A (NIR). A more thorough discussion is given by Schmidt et al. (2010).

#3 (p3,l20ff) The wavelength dependence of horizontal photon transport is mentioned here. Could you give a more detailed literature review on this since it is crucial for the entire manuscript?

There have been many studies on the wavelength dependence of 3D effects in *radiance*, and the manuscript cites a small sub-set of these in Section 5 (Wen et al., 2007; Marshak et al., 2008; Varnai and Marshak, 2009), at which point the connection to the irradiances is made. It reads as follows: "Remote sensing studies (e.g., Marshak et al., 2008; Várnai and Marshak, 2009) had previously established that the above-mentioned *radiance enhancement* for clear-sky pixels near clouds was associated with "apparent bluing," and proposed molecular scattering as the underlying cause for this spectral dependence." Following the reviewer's suggestion, we did add two additional studies pertaining to radiances (Marshak et al., 2014; Kassianov and Ovtchinnikov, 2008) further up in the text, which now reads: "For the extreme case of zero cloud optical thickness, the effect of horizontal photon transport had previously been observed as clear-sky radiance enhancement in the vicinity of clouds (Wen et al., 2007; Kassianov and Ovtchinnikov, 2008; Várnai and Marshak, 2009; Marshak et al., 2014)."

Unfortunately, studies for *irradiances* are rare, and the only ones that the authors were aware of (Ackerman and Cox, 1981; Marshak et al. 1999; Kindel et al., 2011) had been cited. However, the most relevant study (the one by Kassianov) had only been included as a footnote, and we moved it into the body of the text at the location commented on by the reviewer.

#4 (p4, l2-15) The paragraph is a mixture of outline and outlook (l6-9). Please strengthen the content. A structure of the paper is already described in the last paragraph of the introduction. Therefore the idea of the paper should be presented before (performing 3D and 1D simulations with a measured cloud data set, identifying H and it's spectral behavior, . . .) without prejudging the results.

Thank you for catching this, we agree. We deleted the lines in question (L6-9, also 13-15). We also shortened the introduction in general.

#5 (p5, l18) Eq. (3) states the spectral absorptance. Add here directly, that these layer properties are valid for homogeneous conditions without horizontal photon transport. The reader might be confused otherwise because Eq. (3) contradicts Eq. (1) without this restriction (as noted only on p.6, l5-7).

Thank you for this helpful comment. We made the reader aware of the difference between (1) and (3) by pre-ambling the formula with this statement: "For **homogeneous conditions (*H*=0)**, this can be quantified in terms of the layer property absorptance".

#6 (p8, l8-12) This paragraph gives an outlook. Better put this at the end of the manuscript.

This statement (l8-12) was deleted.

#7 (p9, l4-6) As stated by the authors using height-constant effective radii has an effect on the vertical distribution of the phase functions which probably differ from reality. Why does the phase function don't affect the 3D radiative transfer? Changes of the phase function result in changes of the scattering direction. Maybe this is not as relevant as for radiance simulations. Please clarify.

We agree that this simplification undoubtedly has an effect, and we only made this simplification lacking better data. It is true that this would be a bigger problem for radiances than for irradiances because of the hemispherical integration. Luckily, this paper is basically a *modeling* study, albeit based on observations. We preferred actual imagery data to idealized cloud fields, which arguably could also have worked to carry out the study. Whether or not our calculations actually depicted the truth is therefore not as relevant for the message of the paper. This is different in the follow-on study (Song et al., 2016) where we used actual irradiance measurements to validate the model output.

#8 (p9, l8) Please define WC.

Done (it's water content, not a sanitary facility ☺).

#9 (p9, l17) Please justify the choice of spatial resolution (with respect to typical spatial scales of radiative smoothing).

The chosen resolution is certainly not fine enough to reproduce radiative smoothing in the radiance fields, but that was also not the point of the paper, which focuses on radiative energy budget quantities instead. The finest scale that is usually considered in such studies is 1km. We modified the sentence in question to "The resulting cloud field was gridded to a resolution of 0.5 km horizontally **(similar to the MODIS pixel size of some channels)** and 1.0 km vertically (chosen larger than the mismatch between CRS and MAS in cloud top height)," in order to convey our motivation for 0.5 km as spatial resolution. Undoubtedly, a finer resolution would be better, but it would have been computationally prohibitive to achieve appropriate signal-to-noise level for each of the pixels.

#10 (p11, l16) What will be generalized? The solar position?

We modified the sentence as follows: "The scene parameters (solar geometry, surface albedo, cloud properties) will be generalized in future work (Song, 2016)."

#11 (p12, l8-11) The enhancement of radiance in the vicinity of clouds is mentioned here. Can you cite also papers dealing with the enhancement of irradiances? Add also the fact that this effect is wavelength-dependent.

We added more references at this point (Wen et al., 2007; Kassianov and Ovtchinnikov, 2008; Várnai and Marshak, 2009; Marshak et al., 2014). See also our response for comment #3 regarding the wavelength dependence. We did not really mention the enhancement of *irradiances* in this context; this has been done in numerous other studies (including two of our own, Schmidt et al., 2007; 2009). We didn't cite these here because we wanted to keep this focused at the wavelength dependence. Note that the Kassianov paper is the only one (to our knowledge) besides the Ackerman and Cox paper which addresses this topic.

#12 (p13, l15) Could you insert the linear fit in Fig. 3a?

Done, and we added a statement later on (following the discussion of Equation (12)) that a linear fit is less accurate than the spectrally dependent parameterization that we developed later on: "This is more accurate than the derivation of the slope from a linear fit to the spectrum as used for Fig. 3, which, due to the non-linearity of the spectral dependence, differs from that of the tangent if finite wavelength intervals are used."

#13 (p13, l24) "pixel-to-pixel radiation exchange" → Please add "horizontal" here. There is of course a vertical exchange of photons.

Done.

#14 (p18, l16-19) "Eq. (1) suggests..." In my opinion these two sentences do not contribute significantly to the context of this section. Referring to transmittance here somehow interrupts the flow of the discussion on spatial aggregation.

We deleted these two sentences.

#15 (p20, l20) Please motivate the restriction of conservative scattering here, otherwise the missing absorption term might confuse the reader.

We did remind the reader in a few places that we are only talking about wavelengths where clouds do not absorb; the general equations including A are only used to motivate our study in the introduction. However, the other reviewer also commented on the potential confusion on p20/L20, and to make it clear that we are making the simplification of A=0, we modified the text as follows: "Juxtaposing energy conservation for a horizontally homogeneous atmosphere (TIPA + RIPA = 1) with Eq. (1) for conservative scattering (***A*=0, therefore** T3D + R3D = 1 – H) yields the plausible relationship..." We will turn our attention to wavelengths where clouds do absorb in a future study.

#16 (Sect. 8, first paragraph) To make sure that the equations are valid only for a specific wavelength range, the index "λ" would be helpful for H, R, T,...

We preceded the formulae with this statement "For any atmospheric column, H is connected to R and T through Eq. (1) and manifests itself in a transmittance and reflectance bias (λ index omitted):" to indicate that the following discussion addresses a range of wavelengths (with conservative scattering, as later explained).

#17 (p23, l4, l10) If you give numbers here then you have to mention that these numbers are case specific with respect to surface albedo and solar position.

We modified the text as follows to clarify the scene dependence of ε: "$\varepsilon = 0.7 \pm 0.1$ **for the scene we studied**" on p23,l10. As for the x parameter, it is not scene dependent. We did not make a strong statement about this in this manuscript because more scenes will need to be studied, but it is plausible that x~4 would not change much from scene to scene, whereas ε depends on scene parameters such as surface albedo. We pointed out the need for further studies on what drives ε in the following question (conclusions): "How does the discovered correlation and the constant of proportionality in its parameterization,

ε, depend on scene parameters such as solar zenith and azimuth angle, surface albedo (magnitude and spectral dependence), and cloud morphology and microphysics? What "drives" the parameter ε?" This question is addressed by Song (2016, chapter 4), and the content of this dissertation chapter will likely be published as a stand-alone paper at a later time (probably combined with the generalization to NIR wavelengths).

#18 (Sect. 9) Be more consistent with using indices for H. For example p.23, l.16: Is it H or H_0 or H_λ which has to be known?

We attempted to follow this suggestion and went through the indexing in the manuscript. In this particular place, we changed as follows: "Once ε is established for a given cloud scene, the spectral perturbations associated with horizontal photon transport can be derived for each pixel if the value of H_0 is known. Conversely, if the spectral shape of H_λ is known at one wavelength, its magnitude can easily be inferred for the whole spectrum."

#19 (Fig3b) Is there any reason for the increasing scatter[~~ing~~] of 3D-based S_0 –H_0 correlation for negative slopes?

Great question; there are two parts to this: (a) the asymmetry between the (negative) minimum of H_0 and the (positive) maximum [probably not what the reviewer referred to] and (b) the increasing variability of S_0 for a fixed (negative) H_0.

Regarding (a): In the domain average, $<H_0>=0$ despite the asymmetry. This is because fewer cloudy pixels with high values of H_0 balance a larger number of clear or low-optical thickness pixels with smaller (negative) values of H_0.

Regarding (b): We don't have a very good understanding of this yet, but the likely explanation is that for pixels that are clear or have low optical thickness, the spectral signature associated with horizontal photon transport may be affected by additional processes that are not captured by the simplified mechanism as presented in Figure 5. For example, for an optical thickness <4, the partial compensation to horizontal photon transport through molecular scattering as indicated by blue arrows may become more complicated. We did not comment on this extensively and leave this to the future. We did, however, add the following statement to section 6: "Note that below $\tau \approx 4$, directly transmitted radiation dominates the downwelling irradiance, and the cloud may not act as a "diffuser" as shown in Fig. 5. The direction of the green arrows is then along the direct beam." This effect is most likely the cause for the deviation from the correlation that the reviewer observed.

Technical comments:

1) Please remove footnotes

Done.

2) Check that symbols in figures have italic format.

Done, figures were replaced.

3) (p12, l25) Figs. → Fig. 4): (p13, l25) "H" → "H_0" 5): (p14, l3) "H_λ" → "H_λ" (italic)

All done, thanks.