Response to review of "The spectral signature of cloud spatial structure in shortwave irradiance" by anonymous Referee #1

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We very much appreciate the thorough and positive review of this manuscript and the helpful comments for improving content, clarity, and context within the literature. We are open for further input, should we have mis-interpreted the reviewer's points (point-by-point response below).

Assessment by reviewer: Minor revisions

General points:

#1 Even though this paper contains plentiful new findings and scientific discussions, I feel that the manuscript lacks coherence. I believe that the manuscript can be significantly improved if the authors rearrange paragraphs and shorten unnecessary explanations in Introduction and Discussions.

We agree with the reviewer and heeded the advice by removing unnecessary explanations (not just in the introduction), especially the ones pertaining to radiances, which interrupted the flow of the paper. It was tempting to allude to this topic in this paper, but we realize that it is better addressed in a companion paper. Rather than going into too much detail here, 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. Most of the changes in response to this comment are in the introduction and in the body of the paper; the Summary & Conclusions section was shortened only slightly because we felt the need to discuss the significance of our findings given the unusually large amount of material covered, and this was appreciated by reviewer #2.

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 This manuscript clearly showed a reliable relationship between horizontal net transport and spectral dependency, built a parameterization function, and solved coefficients of the function, such as ε. This is an excellent work indeed. However, it is also important to give a specific direction how the users can apply the parameterization method for inferring 3D effects. I think this is briefly discussed in Section 9 (page 23, line 4-23), so the authors can simply add more detailed explanation/justification of the parameterization in Sections 6 or 9.

This is a very good point, which was brought up by both reviewers. 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 very 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 $d\lambda$ 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.

#3 As also commented in the manuscript, the relationship between H and S was inferred in Schmidt et al. (2010). In my understanding, the paper definitely shows new findings, such as a strong linear relationship on a pixel-basis, confirmation of molecular effects from the sensitivity study, and parameterization for the future applications. If this paper highlights new findings in Abstract and Introduction clearly, the readers would catch them more easily.

We agree – it was somewhat unclear in the abstract what was done in earlier studies vs. this paper. The revised abstract was re-structured significantly, and clearly points out the new aspects of this paper at the very beginning, i.e., identifying the physical mechanism that causes the correlation between spatial structure and spectral signature, as well as the parameterization developed on its basic. The new abstract reads as follows:

"In this paper, we used cloud imagery from a NASA field experiment in conjunction with three-dimensional radiative transfer calculations to show that cloud spatial structure manifests itself as spectral signature in shortwave irradiance fields – specifically in transmittance and net horizontal photon transport in the visible and near-ultraviolet wavelength range. We found a robust correlation between the magnitude of net horizontal photon transport (H) and its spectral dependence (slope), which is scale-invariant and holds for the entire pixel population of a domain. This was at first surprising given the large degree of spatial inhomogeneity, but seems to be valid for any cloud field. We prove that the underlying physical mechanism for this phenomenon is molecular scattering in conjunction with cloud inhomogeneity. On this basis, we developed a simple parameterization through a single parameter ε, which quantifies the characteristic spectral signature of spatial heterogeneities. In the case we studied, neglecting net horizontal photon transport leads to a transmittance bias of ±12-19% even at the relatively coarse spatial resolution of 20 kilometers. 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."

Specific points:

#1 In Abstract, it might be necessary to comment significance of 3D effects, but the authors can simply mention it here and discuss in more detail in later sections. It seems this long discussion hinders main points of this paper (the strong linear relationship that authors found and devise a parameterization method).

Agreed; see the point above along with the modified abstract. The discussion of 3D effects for the particular case studied in our paper was moved to the end, to emphasize the main points (presented at the beginning).

#2 Line 1, Page 2: It is not clear what spectral radiance perturbation means. Please explain spectral radiance perturbation, or remove the last sentence of Abstract.

The last sentence of the abstract was deleted, and a more general statement was added ("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.").

#3 Line 5-10, Page 3: "The spectral dependence" and the following sentence, I am not sure why the fact - |H| at visible band is similar to |A| at near-infrared - is related to significance of H in broadband A. These two sentences do not seem cause and effect. Please revise them.

We revised this section on page 3 to address this problem, it now reads as follows:

"Schmidt et al. (2010) derived *apparent absorption*, the sum of *A* and *H*, from irradiance measurements aboard the NASA ER-2 and DC-8 aircraft that flew along a collocated path above and below a heterogeneous anvil cloud during the Tropical Composition, Cloud and Climate Coupling Experiment (TC^4) (Toon et al., 2010). The results of this study showed that, in absolute terms, *H* at visible wavelengths (where cloud and gas absorption are negligible) can attain a similar magnitude as the absorbed irradiance *A* at near-infrared wavelengths. Horizontal photon transport thus has the potential to mimic substantially enhanced absorption. Three-dimensional (3D) calculations confirmed the measurements, and radiative closure was achieved within measurement and model uncertainties without invoking proposed enhanced gas absorption (Arking, 1999) or big cloud droplets (Wiscombe et al., 1984)."

Note that we kept the statement "Horizontal photon transport thus has the potential to mimic substantially enhanced absorption," but removed the term "broadband". What we meant was that a broadband observation of "absorption" by way of collocated legs above and below a cloud layer is really the wavelength integral of A_lamda + H_lamda, not just A_lamda. If the magnitude of H in the visible is on the same order of magnitude as A in the near-infrared, the contribution of H to the broadband integral of A+H may be comparable to that of A. In fact, it may even outweigh it (not stated in the paper). For this reason, it is important to make spectrally resolved measurements; otherwise it is impossible to separate H and A (in the spirit of the Ackerman & Cox papers).

#4 The authors often used footnotes. However, ACP does not recommended foot- notes because they disrupt the flow of text. Please consider removing footnotes and includes them in the main text. Please refer to http://www.atmospheric-chemistry-and-physics.net/for_authors/manuscript_preparation.html .

Thank you, all the footnotes were either removed (where not of central importance to the manuscript) or incorporated into the manuscript.

#5 Line 6-9, Page 4:”In an accompanying paper. . .” In my understanding, we will need results of Song et al. (2015) to infer dH/dλ from satellite radiance measurements. Once we get dH/dλ from the satellite measurements (or slope), we can estimate H from the parameterization equation in this manuscript. I think this discussion is more relevant when authors explain possible application, e.g. Section 9. It does not carry practical knowledge to readers in Introduction stage.

Thank you for catching this; we deleted the radiance-related statement here. The shortened paragraph now reads as follows:

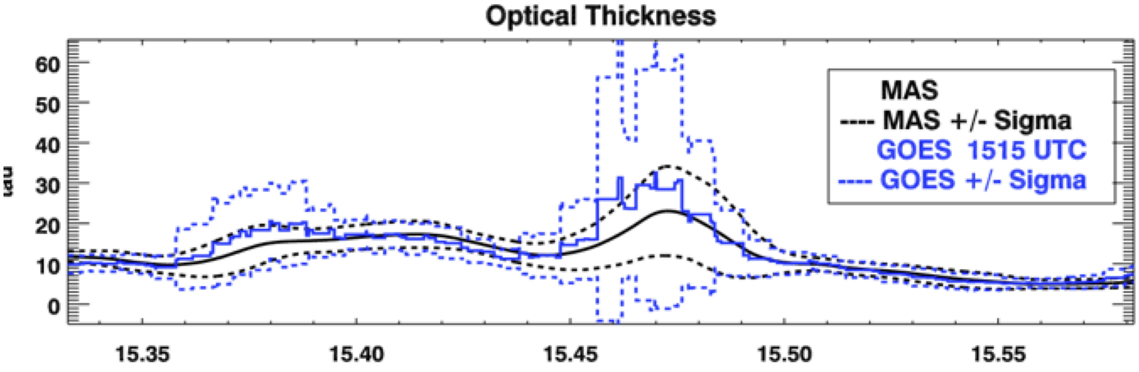
“Further analysis of the relationship between cloud structure and its spectral signature, presented here, revealed a surprisingly robust correlation between the magnitude of H and its spectral slope, dH/dλ. In the course of this paper, we provide evidence for molecular scattering as the physical mechanism behind this correlation and develop a simple parameterization based on this knowledge. We also examine at which spatial aggregation scale H can be ignored and whether the discovered correlation between H and dH/dλ is scale invariant. Finally, we consider the ramifications of our findings on the shortwave surface energy budget and find that while cloud transmittance biases may be significant even after spatial averaging, they are also accompanied by spectral perturbations similar to the ones that we encountered for H. These biases may thus be detectable and correctable using adequate ground-based radiometers.”

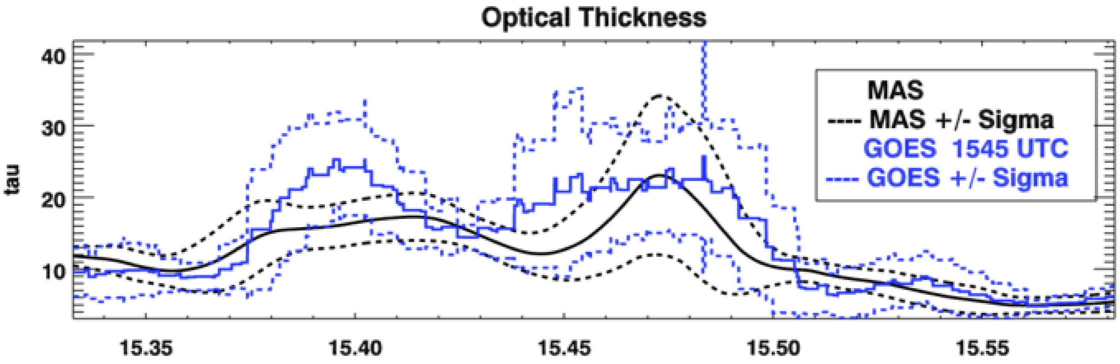
#6 Line 8, Page 8: “The spectral dependence of ..the full shortwave range” I think the authors cited Song et al. (2016) since this manuscript considered part of shortwave (< 1000 nm). Please state the wavelength range that this study covers.

The originally cited work (Song 2016, a dissertation, has now been published) actually changed scope and actually no longer covers any wavelengths beyond the near-UV, visible, and very near infrared. We have therefore removed this reference. We would like to point out here that there is work that has been done by Marshak and others for *radiances*, (Marshak, Evans, et al., 2014) and we added a statement to this effect. We also added a reference to Kassianov and Ovtchinnikov (2008).

#7 Line 1, Page 10: “we chose the earlier one because it was more consistent with the MAS retrieval” The 1515 UTC is more consistent with MAS in terms of cloud optical depth? Or perhaps 1515 UTC is closer to MAS observation time? Please clarify this.

This question allows us to show plots that we chose not to include in the manuscript. As the reviewer suggested, we used cloud optical depth to chose from two possible GOES scenes. The first plot shows the collocated MAS/GOES15:15 optical depth within 0.1° around the ER-2 latitude and longitude along the flight track. The second plot shows the same, but with the later GOES retrieval (15:45).





In terms of the timing, both GOES retrievals would be possible because the ER-2 flight leg (15:21-15:33) is right in between 15:15 and 15:45. However, the comparison of the MAS- and GOES retrieved optical thickness is more consistent when using the 15:15 scene. We changed the text as follows to make this clear: “In the sampling region, cloud property retrievals were produced at 15:15 and 15:45 UTC (Walther and Heidinger, 2012), of which we chose the earlier time because it was more consistent with the MAS retrieval in terms of the optical thickness along the ER-2 track.”

#8 Figure 2: From Figure 2, it seems that MAS domain is located boundary of cloud system, according to GOES retrieval. Figure 1 still shows large optical depth up to 80. How consistent MAS and GOES optical depths?

This observation is correct. The MAS swath does capture the edge of a cloud system (as shown in Figure 2). The color scale of Figures 1 and 2 is different; even GOES shows a fairly large optical thickness on the NE edge of the MAS swath. Because of the different pixel size, GOES and MAS retrievals are not expected to match exactly. For this reason, the retrievals were aggregated to 0.1° "super-pixels" in the optical thickness plots above. The edge of the cloud system that the reviewer mentions is sampled at UTC=15.47 by the ER-2, and MAS and GOES show optical thickness values of ~20-30 at this aggregation scale. The higher optical thickness values as observed by MAS (~60) are small-scale maxima. In general, GOES and MAS retrievals are consistent within the range of the standard deviation in the 0.1° circle.

Note that the agreement in other retrieval parameters (cloud top height, effective radius) was not as good, in part because of different channel combinations that were used by the MAS / GOES algorithms. We chose not to go into detail about the MAS/GOES consistency in this paper because this is not its main purpose; such studies may be done in a separate paper.

#9 Line 16, Page 11: It would be helpful if the authors provide # of photons per pixel and corresponding accuracy (e.g. 1/sqrt(N)).

We included some more information on the photon number in the revised manuscript.
Small domain: 1e11 or 7.4e6 per pixel
Large domain: 1e12 or 4.3e6 per pixel
These photon numbers led to sufficiently low noise level. For example, the maximum standard deviation for the upwelling irradiance at the pixel level is 0.008 W/m2/nm at 500 nm.

#10 Line 3, Page 12: Is it true that H0 cannot exceed 100%? H0 is divergence of horizontal photon transport (e.g. Eq. A7 in Marshak et al. (1998). Therefore, it should be rare, but isn't it theoretically possible that H0 > 100%?

Marshak et al. (1998) Biases in Shortwave Column Absorption in the Presence of Fractal Clouds, J CLI, 11, 431-446.

Thank you for this excellent catch! The reviewer is of course correct; this erroneous statement survived our internal review process. In fact, we found cases (in our own analysis for the next paper) where H_0 does exceed 100%. We simply deleted this statement, the revised version reads: "When H_0 falls below –100%, the radiation received through the sides of a column or voxel exceeds that from the top of the domain." We don't state that the opposite is also true (for H_0>100, but that goes without saying).

#11 Line 3-4 page 13: molecular scattering as the underlying cause for this spectral dependence. This is a bit different from conclusion in Schmidt et al. (2010) (paragraph [33]). Could the authors explain the difference?

A very good point! We need to provide a little bit of background to explain this. The statement from Schmidt et al. (2010) in question is the following: "Preliminary tests showed that switching off molecular scattering in the RT model did not change the slope significantly, thus ruling out molecular scattering as the cause for the spectral slope of the apparent absorptance." In light of new evidence, the second part of this statement is, in fact, incorrect, but we must emphasize the word "preliminary". It is true that when switching off molecular scattering, the slope did not change in this earlier study (incidentally done with a different model than used here). We therefore had to assume that the reason for the slope must lie elsewhere. At least two colleagues in the field thought that molecular scattering could not have such a large effect on irradiance (in contrast to radiance where it had been found at this point). While we always suspected molecular scattering, we could not present evidence at this point. In light of this, we should have worded this statement more cautiously. As it only turned out later, the explanation was that the switch in the model was actually inactive (keeping molecular scattering on regardless of the switch settings). We did not suspect this until after the paper was published, at which point we had a conversation with one of the code developers who brought up this possibility. In retrospect, this was a user error because we should have been able to diagnose this problem with further runs. We have since done these tests and found the cause of the problem. The analysis in the current paper (Figure 3) correctly shows that molecular scattering does explain the phenomenon. We added the following statement about the earlier study: "Note that the earlier study by Schmidt et al. (2010) remained inconclusive as to the mechanism of the spectral dependence they observed." This is justified as the earlier study states (in the conclusions): "The physical basis of the spectral shape of near - UV and visible apparent absorption remains to be explored, as well as the scales over which horizontal photon transport occurs in high - cloud systems (for example, by embedding the MAS cloud scene in the larger context of GOES retrievals)."

#12 Line 14-18, Page 17: "In this context, it is. . ..above a cloud field." It is hard to understand this paragraph. Could the authors consider revise this paragraph? Also radiance in this paragraph means spectral radiance and irradiance is angle-integrated spectral radiance?

We simply deleted this paragraph because it distracted from the main content.

#13 Line 1, page 18: CERES algorithm converts broadband radiance into irradiance without taking into account 3D effects, even though the ADM is based on observation. For example, if the CERES observes radiance in illumination side, radiance for that angle is higher than other angles, but ADM does not consider this. Therefore, I guess the derived irradiance is not completely free from 3D errors. Of course these errors are negligible if we get enough samples and take average spatially and temporally.

We agree, and the Ham et al. (2014) publication (cited in our paper) talks about the effect of horizontal photon transport (not so much about illumination though). However, the 3D errors in transmitted irradiance should be much larger than in albedos because in principle, the ADMs do include spatially inhomogeneous conditions, however sparsely the parameter space may be sampled for those. Also, our statement was meant in the statistical sense, i.e., averaging over multiple "realizations" of such scene types. We modified our statement as follows: "In principle, the mean albedo of an inhomogeneous cloud field derived from CERES observations should be fairly insensitive to 3D effects because they are statistically folded into anisotropy models of such scene types (if these empirical models adequately accomplish the radiance-to-irradiance conversion for a range of sun-sensor geometries)."

#14 Line 19-20, Page 20 I wonder why two equations in line 19-20 do not [have] absorption terms.

$$TIPA+RIPA+AIPA=1$$

$$T3D+R3D+A3D+H=1$$

Then Eq. (14) is $H=\Delta T+\Delta R + \Delta A$

This set of equations was written for conservative scattering (no absorption), but since the other reviewer also noted the lack of absorption, we made this more clear by slightly rewording as follows: "Juxtaposing energy conservation for a horizontally homogeneous atmosphere (*T*IPA + *R*IPA = 1) with Eq. (1) for conservative scattering (***A*=0, therefore** *T*3D + *R*3D = 1 – *H*) yields the plausible relationship…"

#15 From Eq. (14), horizontal transport term H is partitioned into 3D effects on reflection, absorptance, and transmittance (ΔT, ΔR, and ΔA). I think ΔT is strongly correlated with H since absolute magnitude of ΔT is the largest among ΔT, ΔR, and ΔA. Note that cloud albedo is 30%, atmosphere transmittance is 50%, and atmosphere absorption is 20%.

This is an interesting thought, and we believe that this partitioning may need to be investigated in the future. It is indeed plausible that the bias is correlated with the magnitude of T and R itself. However, we did not attempt to do the partitioning in the study and focused mainly on the transmittance in the remainder of the paper. We do note that H at the pixel level is correlated with ΔT, but not with ΔR. However, we do not draw conclusions about the magnitude of the two biases. Comparing Figure 9b with Figure 10 does show that the range of ΔT is larger than that of ΔR, however the point there is not the magnitude but the correlation with H. As to Figure 10, it was surprising to us that R and H do become correlated at scales greater than 5 kilometers.

#16 The authors noted that 3D effects are significant even for large scale. However, previous studies already showed that instantaneous 3D effects might be large, but domain-averaged 3D effects are small. I think the authors need to use 'instantaneous' term if necessary, to differentiate from domain-averaged 3D effects.

The emphasis of the paper as a whole was on the spectral aspect of this problem, not on the magnitude of the 3D effect, for which the single case presented in the paper would not have sufficient statistics anyway. We confirm that we mean a *local* 3D effect, rather than the domain-average effect. We prefer "local" to "instantaneous" as suggested by the reviewer because it is tied to space rather than time. Where appropriate, we added "local" in the few occurrences where we do talk about magnitudes. For example, the section in the abstract reads as follows: "In the case we studied, neglecting net horizontal photon transport leads to a **local** transmittance bias of ±12-19% even at the relatively coarse spatial resolution of 20 kilometers." More changes have been made to section 7. In other cases, we made clear that we are talking about pixel-level effects and biases. We agree that in the domain average (as shown in previous papers), 3D biases become small. At the same time though, our study showed that even aggregating the data to large scales, significant biases survive. Figure 8d is meant to illustrate this. One can essentially read off the biases for various aggregation scales. For example, at 0.5 km pixel size, we get >50% biases. Averaging to 20 km decreases the bias to just over 10%, and it eventually disappears at even larger aggregation scales. We do believe that Figure 8d and the text accompanying makes this clear. We fully agree with the comment by the reviewer and do not contradict earlier studies.

More recent research (Song, 2016) shows that by considering 3D effect on irradiance (as done in this paper) *and* on cloud remote sensing may lead to biases in transmitted irradiance estimates that do not disappear with increasing scale but survive averaging. This research will also be presented in a separate paper (Song et al., 2016).