Authors’ response to comments from anonymous reviewer 1

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We thank the reviewer for their thorough reading of our paper and for providing thoughtful comments. We have addressed each one below.

Reviewer’s General Comments

1. This paper is a study on the potential of the upcoming CLARREO Pathfinder (CPF) mission to provide more detailed retrievals of cloud properties than heritage (MODIS-like) imaging sensors thanks to a combination of decreased radiometric uncertainty and increased spectral sampling. The specific geophysical situation studied is joint retrieval of cloud optical depth (COD) and effective radius at the top and bottom of the cloud, for marine stratocumulus scenes. In contrast, one of the major current large-scale approaches (MODIS-like bispectral) retrieves COD and near-top effective radius using a pair of bands, and makes multiple bispectral retrievals with their differences being semi-informative on cloud structure.

2. There are two main parts to the analysis. First is use of VOCALS-REX field campaign data to set up some case studies for the proposed retrieval method using MODIS. This has the advantage of being something which can be tested now. The second is a sensitivity study, comparing the capabilities of a MODIS-like sensor with the EMIT instrument as a surrogate for CPF. Together these provide a starting point for moving towards this next level of detail in passive imager cloud retrieval algorithms.

3. The manuscript is in scope for AMT. There is a lot to like about this paper: it tackles an important problem, is clearly written, and has some nuance to the discussion. I particularly appreciated the discussion of sampling scales in the VOCALS-REX part of the discussion. The quality of writing and presentation are good. It is (mostly) well-referenced. I also appreciate the authors quickly noticing and fixing the incorrect panel of Figure 3. I think it is worth consideration for an AMT science highlight.

4. That said, there are points where I think clarification and deeper discussion with respect to realistic performance are needed. As the paper does not claim to be a fully operational approach it does not need to be the final word on the matter, but as a case study and example of what can be done, I think there are sections where more caveats should be discussed, and there are a few things I was not certain about. I recommend minor revisions before publication. I would be willing to review the revision, if the Editor would like.

Reviewer's Specific Comments

1. Line 31: I'm not sure I'd seen COD described as mean photon free paths through the cloud before, although I can see this framing makes sense as it is the integral of extinction coefficient which is units e.g. km^{-1} (extinction events per unit distance). Normally it is just referred to as vertical integral of extinction coefficient. I'm curious if there's a reason the authors picked this particular framing for COD.

    a. *Authors' Response:* We find it useful to describe optical depth as the number of photon mean free paths because it is an intuitive way to think about a quantity that depends on how the number density of particles and their scattering and absorption cross sections vary along a particular path. Bohren and Clothiaux (2006) show that the probability of a photon traveling a geometric distance x before being scattered or absorbed (assuming no multiple scattering), is $p(x) = (\kappa + \beta)\exp(-(\kappa + \beta)x)$, where κ is the bulk absorption coefficient and β is the bulk scattering coefficient. The mean free path is then $< x > = \int_0^\infty x\, p(x) dx = \frac{1}{\kappa+\beta} = \ell$. Since $\tau_\lambda = \int (\kappa + \beta) dz$, we can also define optical depth as $\tau_\lambda = \int \frac{dz}{\ell}$, the number of mean free paths.

    b. *Proposed changes to the manuscript:* None.

2. Line 31: not sure I'd describe effective radius retrievals as "extinction-weighted" but maybe "photon-penetration-weighted"? For a really deep convective cloud, for example, the photons seen from space are still mostly coming from near the cloud top even if the water/extinction would be somewhat further down. And this is in line with e.g. the Platnick (2000) reference cited and weighting functions shown in the paper. To me "extinction-weighted" implies an optical center of mass.

a. *Authors' Response:* We used the term 'extinction-weighted' to convey that the retrieved effective radius is a vertically-weighted average that depends on the extinction properties of liquid water at the set of wavelengths used in the retrieval. However, it is clear that our term may lead to some confusion, while the reviewer's proposed term is strictly correct.

b. *Proposed changes to the manuscript:* We will use the term "photon-penetration-weighted" instead of "extinction-weighted".

Commented [AB1]: It felt odd to say we would use 'optical path weighted' instead of their suggested 'photon-penetration-weighted' without a reason. So I just said we would use their suggestion. Do you have objections?

Commented [PP2R1]: okay

3. Introduction, general: I like the historical discussion, but there are a few omissions that I think are quite relevant. One is the ORAC retrieval which came out of the same lab as Clive Rodgers who put down the Optimal Estimation (OE) formalism used here, applied mostly to European sensors (ATSRs and successors). See Sayer et al (2011) and Poulsen et al (2012). This isn't an explicitly bispectral approach (uses all bands together) but only retrieved a single effective radius (sensitivity from 1.6 and 3.7 micron bands) as opposed to attempting a profile. Another is the VISST algorithm applied to cloud properties from MODIS observations within CERES pixels (as part of the CERES data processing chain), which is also not bispectral but again retrieving a single effective radius from visible and multiple SWIR bands (0.65, 1.6, 2.1, 3.7 micron). The reference I use for this is Minnis et al (2011) – that paper cites some earlier AVHRR work using that algorithm from the late 1990s, but it's in conference proceedings that don't seem to be broadly available, so I can't say for sure what was done. There is also earlier OE work by e.g. Heidinger (2003) applied to the AVHRRs (a lot of later work from that NOAA team focuses on the infrared, but the above algorithm also used solar radiances and is more conceptually similar to bispectral). All of these approaches (ORAC, CERES, AVHRR) have been applied to multi-decadal multi-sensor records and approach the question of effective radius parameterization a bit differently from either the bispectral method or the profiling method, so I think merit some discussion in the manuscript. Also, I think all of these methods were applied somewhat earlier than the publications describing them were written (otherwise mostly documented in proceedings and technical reports) so they are not such newcomers as the paper dates might imply.

a. *Authors' Response:* We appreciate the reviewer sharing these relevant papers. Poulsen et al. (2012) was particularly illuminating with its thorough outline of the ORAC retrieval methodology and the description of forward model uncertainty. The results of Sayer et al. (2011) suggest that the multispectral retrieval of effective radius estimates effective radii deeper in the cloud where droplet sizes tend to be smaller. The paper concludes with an endorsement for the retrieval of vertical droplet profiles. Heidinger (2003) applied the Rodgers optimal estimation technique to retrieve effective radius and optical depth using one channel in the visible, one in the near-infrared, and two in the infrared. Minnis et al. (2011) describes an iterative technique to retrieve cloud phase, optical depth, and effective radius from MODIS and VIIRS observations to support the CERES data products. We will highlight all of these papers in the introduction.

b. *Proposed changes to the manuscript:* We found the suggested papers relevant to our historical overview and will add them to our introduction. In addition, we will rely on the work of Poulsen et al. (2012) in section 2.3, where we will include a description of forward model uncertainties.

4. Line 106: I see there is a paper reference there but for ease it would be good to detail the expected pixel size, orbital geometry, swath width, and spectral sampling/bandwidth of the CPF mission as well. This should be recapped in the conclusion as well, where relevant (e.g. in the discussion of scales of variability in marine stratocumulus clouds).

    a. *Authors' Response:* We agree.

    b. *Proposed changes to the manuscript:* Spectral sampling and resolution, orbital geometry, spatial resolution, and swath width will be added to line 108. We will also include the CPF spatial sampling and swath width information in Section 4.1 to provide context to our discussion on comparing sampling volumes between in situ and remote measurements. Lastly, we updated Figure 6 to include an additional histogram with a length scale of $0.5 km$, the spatial sampling of the CPF instrument at nadir. While the result is similar to the $1\ km$ spatial sampling of MODIS when looking nadir, we found it useful to show that, as pixel size decreases, the average variability of effective radius with respect to the horizontal plane decreases.

5. Section 2.1: I would suggest renaming this "the bispectral method" instead of "the standard method". What does "standard" mean? From a polar-orbiting viewpoint, yes, this method has been applied routinely to MODIS and VIIRS. But that in my view implies it's the only way things are done, despite e.g. the ATSR, AVHRR, CERES references I provided which have similar (or longer) time series of data.

    a. *Authors' Response:* We agree and will adopt "bispectral method".

b. *Proposed changes to the manuscript:* We will change section 2.1 heading to 'The bispectral method'. We will also update our phrasing throughout the paper to use 'bispectral' instead of 'standard'.

6. Line 233: In practical terms S_ε tends to be used not just for measurement uncertainty but the combination of measurement plus forward model uncertainty covariance. This may be worth noting. Mathematically, it doesn't make a difference whether one puts only measurement error in S_ε (in which forward model parameterization uncertainty is normally put in another matrix often called S_b in Rodgers notation), or combines both measurement and forward model uncertainty. This is omitted from the equations and discussions here. See also my comment 11, which is my main issue with the paper as written.

    a. *Authors' Response:* We agree with the reviewer's comment that there should be some discussion on forward model errors. Indeed, our forward model deviates from the true nature of clouds and the atmosphere due to the many simplifications, which deserve scrutiny. The recommended paper by Poulsen et al. (2012) was particularly illuminating in this regard, thanks to its thorough discussion of forward model uncertainties.

    b. *Proposed changes to the manuscript:* We will update line 233 and section 2.3 to explicitly state that the covariance matrix, S_ϵ, includes measurement and forward model uncertainty. We will define the various sources of forward model uncertainty that are relevant to our problem in section 3, citing Poulsen et al. (2012).

7. Line 260 and elsewhere: the paper often refers to the "constrained" OE approach, kind of making it seem like the constraints are unusual or an innovation. In reality though every algorithm (including OE ones) are putting in constraints similar to this (state bounds). I'm not sure that the word "constrained" needs to be emphasized in the paper very much as it makes the reader focus more on that while in my view the novel aspect is getting at radius profiles in adiabatic clouds.

    a. *Authors' Response:* We do not claim that constrained optimal estimation is an innovation of our own. We chose to repeat that phrase to emphasize the importance of the constraint. Without it, using MOIDS measurements with $\sim 2\%$ measurement uncertainty can lead to retrieved profiles where the droplet size at cloud top is smaller than cloud bottom, violating our forward model assumption.

That being said, we appreciate the reviewer's comment because we do not wish to distract readers from the more important result of retrieving droplet profiles.

b. *Proposed changes to the manuscript:* We will remove the qualifier *constrained* from each mention of the optimal estimation method. Where the constraints are first introduced, we will adjust our wording to emphasize their importance, particularly for spectral measurements with MODIS-like uncertainty.

8. Line 276 and elsewhere: the residual/left side of L^2 norm is most commonly referred to as the "cost function" and often denoted capital italic *J* in the Rodgers formalism. For ease of readers comparing different references, I think it would be good to note these notation/terminology differences somewhere around here.

   a. *Authors' Response:* We will adopt the terminology and notation that are commonly used in the retrieval community. However, we would like to point out that the left side of equation 11 in our manuscript is not the cost function in the sense outlined by Rodgers (the first two terms on the right-hand side of equation 5.3 in Rodgers, 2000) or Poulsen (equation 1 of Poulsen et al. (2012)). This is why originally defined the L^2-norm of the difference between the forward-modeled reflectances and the measurements as the 'residual'.

   b. *Proposed changes to the manuscript:* We will define the cost function as the L^2-norm of the difference between the forward-modeled reflectances and the measurements. Instead of continually referring to the L^2-norm throughout the paper, we will change the phrasing to either 'cost function', or simply J.

9. Line 286: for completeness, I'd add the equation for uncertainty estimate on the retrieved state here. Unless I missed it, it seems to not be included, and as part of the paper is talking about expected improvements from CPF I think it is worth including explicitly how this is calculated.

   a. *Authors' Response:* We agree.

   b. *Proposed changes to the manuscript:* We will add the equation for computing the posterior covariance matrix after line 286.

10. Line 335: the MODIS retrieval uncertainties used as the a priori uncertainty should be stated here, and a citation to where they came from added.

    a. *Authors' Response:* The a priori uncertainty for optical depth and effective radius at cloud top was defined as the MODIS retrieval uncertainty for optical depth and effective radius, respectively. The MODIS retrievals and their respective uncertainties vary between pixels. Therefore, there is no single number to report. For the three MODIS scenes used in our paper, the mean retrieval uncertainty for cloud effective radius over ocean with an optical depth of at least three was 10.6% ($\sim 0.89\ \mu m$). For optical depth, the mean retrieval uncertainty was 5.9% (~ 0.56). These values align with the expected retrieval uncertainty of the MODIS Collection 6 cloud products (Platnick et al., 2017).

    b. *Proposed changes to the manuscript:* We will add a citation to line 335 for the retrieval uncertainty of MODIS Collection 6 cloud products (Platnick et al., 2017). We will also include the statistics mentioned above for retrieval uncertainty of cloudy pixels over ocean with an optical depth of at least three to provide readers with an idea of the values used in our analysis.

11. Sections 3 onwards: my main technical issue with the MODIS retrievals and simulated CPF uncertainties is that they are a realistic "best case" performance and this is kind of skirted over. The discussion more or less takes the only relevant uncertainty source as radiometric (sensor absolute calibration uncertainty and shot noise). Even if that were true, from my reading the calibration uncertainty is taken as spectrally independent. In reality it may be spectrally correlated (based on experiences with various space-based sensors) which affects downstream uncertainty characterization. But really, the main issue is the implicit assumption that the forward model (including its numerical implementation) is perfect which is inherently false (and semi-acknowledged by the fact the section 3 title includes "forward model assumptions"). These assumptions, as well as e.g. factors like lookup table interpolation precision, uncertainties in ancillary data (surface reflectance/albedo, gas columns), and non-calibration image artefacts (e.g. 3D radiative transfer effects, image ghosts, delayed impulse response after bright pixels), are often similar to or larger than absolute calibration uncertainty. And these can all have e.g. angular dependence and spectral covariation as well. So this is a big reason why retrievals are never as good as idealized sensitivity studies (as they rarely can take into account these factors). I understand this paper is a proof of concept and not a full operational algorithm. But I think it is necessary to acknowledge these issues seriously (I really doubt we can make our forward models good enough to take advantage of CPF's radiometric calibration quality). Otherwise it feels like it is misleadingly over-hyping the

CPF mission as folks who don't work in algorithm development may well not be aware that radiometric quality is only one of the determining factors in retrieval quality. I wonder if somehow this discussion could be tied into the existing sensitivity studies (or new sensitivity studies). Maybe this could involve comparing MODIS retrieval uncertainties with the width of contours in figures 7 and 8 – I will leave this to the authors to decide how best to respond.

a. *Authors' Response:* We acknowledge the lack of discussion on sources of forward model uncertainty. Forward model uncertainty is difficult to quantify but should not be ignored. We agree with the reviewer that our discussion in sections 4.2 and 5 should focus on total uncertainty. Minimizing forward model uncertainty leads to a measurement-limited solution that, the reviewer points out, may be unachievable with CPF measurements. Assuming a droplet profile is just one assumption that reduces forward model uncertainty because the assumption of a vertically homogeneous cloud is known to be a simplification for certain types of clouds (Platnick, 2000). In the future, an optimal estimation algorithm may be able to leverage the full spectrum of CPF to simultaneously estimate cloud phase (Pilewskie and Twomey, 1987), cloud top height (Rozanov and Kokhanovsky, 2004), above-cloud column water vapor (Albert et al., 2001), CO_2 column amount (Buchwitz and Burrows, 2004), and aerosol optical depth (Mauceri et al., 2019) , reducing forward model uncertainty by limiting the number of assumptions.

   We assumed the radiometric uncertainty of the instrument was uncorrelated, and the reviewer is correct in noting that this is not the best representation of real space-based spectrometers. Kopp et al. (2017) computed the relative total radiometric uncertainty for the CPF instrument, HySICS, as a function of spectral channel for bright (cloud-filled) Earth viewing scenes. Flat field uncertainty dominates at short wavelengths, while shot noise and brightness offset dominate at longer wavelengths. For each channel, the total relative uncertainty appears to strongly covary with neighboring channels (Kopp et al., 2017). Future iterations of this work will leverage these findings to define the off-diagonal elements of the measurement covariance matrix. That said, we will emphasize that the assumption of uncorrelated measurement uncertainty between spectral channels is a simplification of the true instrument.

   Lastly, we do not want the framing of our results to overstate our findings. We will adjust the wording in sections 4.2 and 5 to provide the necessary context for our results. We appreciate the reviewer's suggestion to include a discussion on how our multi-spectral retrieval uncertainty compares with the well-documented MODIS Collection 6 effective radius retrieval uncertainty (Platnick et al., 2017). We will incorporate this into section 4.2.

b. *Proposed changes to the manuscript:* We will update section 3 to include a description on sources of forward model uncertainty, following previous work by Poulsen et al. (2012). In section 4.2, we will adjust the uncertainty added to the simulated TOA reflectance spectra to include both measurement and forward model uncertainty. Instead of explicitly estimating the uncertainty of each source within the forward model, we leverage previous work by Watts et al. (1998) and Platnick et al. (2017) to describe the fraction of the total uncertainty due to forward model uncertainty. We also make it clear that forward model uncertainty can never be reduced entirely. Additionally, we will expand section 4.2 with a comparison of our multi-spectral retrieval uncertainty estimate using simulated CPF TOA reflectances with the MODIS collection 6 cloud products retrieval uncertainty.

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