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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Resolving ice cloud optical thickness biases between CALIOP and MODIS using infrared retrievals" *by* R. E. Holz et al.

Anonymous Referee #1

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This paper applies inverted 11 um-based retrievals for ice optical thickness as a normalization standard for investigating the fidelity of corresponding MODIS C5, new MODIS C6 and CALIOP constrained/unconstrained retrievals, including accompanying retrievals of effective radius. As you'd expect from this group, this is a strong and innovative study, and thus the paper is very appropriate for ACP. The narrative is well-written, with some exceptions, and figures are clear and legible.

My summary recommendation is that the paper be accepted pending what I think are mostly minor revisions.

Technically, I'm providing my proofing/notes, which I hope are helpful. If there is one

important thought that I'd like to leave with the authors its that I struggled with the Intro and Sects. 2/3 trying to figure out where things were going. Having had this sit in ACP review now for a few months, I encourage the authors to go back and try to simplify the intro a bit more so that the motivations, goals and experimental design fall out a bit more easily. For example, and you'll see my notes, I got confused in the Intro as to how the work would ultimately be based on investigation of C5 first, and then a comparison of new C6 to demonstrate differences. In the end, this is a very simple, but elegant, experiment, and that doesn't really fall out of the intro. In fact, its all rather intimidating at first.

Within my technical notes are a series of mostly minor questions about the science. There is one thought, however, which sticks with me as I write this review. That is, the original result found with C5 seems, and I make note of this in the attachment, rather consistent. The diffusivity of ice-phase clouds near top should be expected to render a measured BT, even at 11, um, that is warm relative to an inversion retrieval based on absolute cloud height boundaries resolved with CALIOP...shouldn't it? Conversely, the impact of your sample selection, which is relatively coarse in terms of all single-layer CALIOP cirrus (which is based on potential separation between multi-layer elements of as much as 3 km), should be expected to include attenuation-limited CALIOP samples where CBH is overestimated. Thus, the inverted 11 um BTs should be colder than actual measurements, right?

If that is correct, shouldn't that be stated up front? I agree with you that its a sobering result. But, isn't it just a natural extension of the limitations of the observing methods that we have? I find that to be a significant conclusion in its own right. However, I think then that the steps taken to overcome this act more a a tuning fork than a physical justification for use of the severely-roughened crystal habits in C6, aren't they? I accept your conclusions of improved performance, and applaud you for being so innovating in clearly and simply demonstrating the improvement. But, isn't there something larger that can be said, in context, with respect to how you reach this point relative to the

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limits of MODIS to accurately resolve BT/IOT for diffusive clouds like ice/cirrus in the first place, and thus what decisions have to be made in order to render more accurate retrievals that account for these biases? There is some discussion leaning this way, with respect to decisions made for global datasets, but could it be more strong within the context of the limitations of passive radiometric sensors overall?

Another point that I think requires some important consideration by the team here is the use of the unconstrained CALIOP retrievals at all within the analysis. First, you'll note that the discussion of the 32 sr solution in Sec. 5.3 is out of order relative to its initial introduction at L534 (also...no Fig. 4c?). I recognize that Mark Vaughan is on the author team. So, I presume that there has been discussion with Mark about the ramifications of what you are saying relative to the community applying 32 sr going forward. But, frankly, I'm far more compelled by what Garnier et al (2015) have shown relative to multiple scattering correction and lidar ratio going forward in V4 of that archive. The result here has some fidelity for data users of V3 going forward, but how limited a user base is that ultimately going to be?

Still, my broader point relates to the impact of attenuation and overestimate of CBH in these data. In your sample, I would expect relative underestimate of IOT from this effect, which your data shows for increasing IOT a greater bias. 32 sr may be a strong effective value, but its gains its significance relative to your sample choice and consideration of broad sample. As I said above, its less physical and more data engineering.

But, its not clear to me that you absolutely need the unconstrained data sample here anyway, except for the possible need to expand the collocated MODIS/CALIOP sample overall. That is, if you expect attenuation within the sample, and accept the bias in Fig. 3 as binding and physically consistent, what are you getting going forward in evaluating the C6 sample relative to it anyway? Again, I can understand need to have a bigger data sample compared with just the constrained sample, but something should be said clearly about this decision (I realize that you discuss need to expand night v. day, but...). Then, the 32 sr should be introduced not in the context that it be used going forward

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by the community, but instead is applied here as a realistic means for normalizing the CALIOP sample investigated to render a better comparison point for MODIS C6? Maybe that's even stretching things a bit.

In sum, the Garnier study will carry more weight, presumably as the V4 retrievals are based on those parameterizations. It seems like the discussion should be more circumspect in that regard, at the very least. But, again, if you removed the unconstrained data from your analysis overall, would you lose anything? I don't think you do. You still have the inverted 11 um BT/IOT and constrained CALIOP samples. Then, you're not dealing with any of the caveats of the unconstrained sample at all, and the paper becomes that much more simple?

I hope these thoughts are useful. Again...a really fun and novel experiment, and I'd endorse any way you wish to proceed in finalizing the paper. I hope, though, that some of these thoughts lead to some changes that really streamline the analysis. Its so hard to bridge the active and passive observations, and I think a lot of folks would learn a great deal reading a paper like this to understand the important differences. Making it accessible for the broader community, then, and simplifying the experimental design and discussion would be very much worth your effort.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/15/C9814/2015/acpd-15-C9814-2015-supplement.pdf

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