GENERAL COMMENTS

Your insightful comments are appreciated and we feel have significantly improved the clarity of the paper. For your general comments, the lab slang has been reduced to the extent possible, mostly in section 3 (i.e. we've changed "parallel calibration" to "parallel calibration constant" and "parallel power" to "parallel measured signal", etc.). Your specific comments are addressed below.

SPECIFIC COMMENTS
p. 28357, l. 15: . . .several studies have been conducted. . . Please provide references.
Done.

p. 28357, l. 28: Please provide original citation for HSRL (Shipley et al. 1983).
Fixed.

p. 28358, l. 15: Give an outline of the paper.
Done.

p. 28360, last paragraph, discussion on calibration: What about the calibration of the 532 nm perpendicular component?
Fixed.
p. 28369, l. 29: The value of the scattering ratio should be 1.05 (not 0.05).
Fixed.

p. 28370, first paragraph of Sec. 4.2.: You speak about "ozone and molecular" or "ozone and molecules". Ozone is a molecule, of course. What you mean is ozone absorption and molecular scattering. Thus, again, use the physically correct terms!
Fixed.

p. 28370 – 28371, discussion in Section 4.2: The two-way transmittance between the uppermost HSRL measurement height and the CALIOP calibration region obviously is the most important source of uncertainty in the comparisons. Therefore, this discussion needs a bit more care.

(addressed below)

First of all, there are a couple of mistakes in the numbers which contribute to the confusion here:
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p. 28370, l. 27: 0.075 – you certainly mean 0.0075 (but this estimate is probably wrong, see below) –
p. 28371, l. 8: . . .we estimate the minimum detectable cloud optical thickness to be 1.2%. . . – you certainly mean 0.012 (absolute value, not percent) –
p. 28371, l. 8: 0.978 – you certainly mean 0.987 Then, you should check and discuss the following issues:

Fixed – see below for more.

- p. 28370, l. 20: What is the horizontal scale of cloud detection for the histogram in Fig. 3? What is the difference in detection sensitivity during day and night?

This analysis was done with the 5km Cloud Layer product, which has been made evident in the text. From the backscatter sensitivity in the ATBD, we estimate the nighttime sensitivity to be lower, approximately 70% of the minimum COT from the day. Figure 3 is, of course, biased heavily by the large number of daytime flights (66 day, 20 night), which has also been clarified in the text. Unfortunately we do not have enough statistics to confirm this with an estimate the COT over HSRL at night.

- p. 28370, l. 21: . . .maximum bias introduced by undetected clouds. . . What is the uncertainty related to the temporal and spatial inhomogeneity of cloud fields? Clouds can move in or out the flight track between the CALIOP and the HSRL observation.

There is no uncertainty due to clouds moving in or out of the track between the observations. For example, if there was a 20 km cloud above HSRL over a given location at time T but not during the time T + dt when CALIPSO observed that location, both data points will be considered as the HSRL measurement will be uninfluenced by the 20 km cloud. However, if HSRL measures a location at time T and there is no cloud above, but there IS a cloud above during the CALIPSO observation both the HSRL and CALIOP profiles will not be considered in the analysis so as not to bias the HSRL average with regions not included in the CALIOP observation.

- p. 28370, l. 27: When estimating the undetected COT you speak about “multiplescattering enhancement” and you multiply the CALIOP value with 0.6. Is this the correct way to deal with the multiple-scattering influence? I think we need some more discussion here. What does “multiple-scattering enhancement” mean? Generally, multiple scattering increases the attenuated backscatter signal within a thin cloud, because forward scattered light doesn’t get lost instantaneously. As a consequence, the observed optical thickness is reduced compared to the true optical thickness of the cloud. Thus the apparent COT should be divided by the approximate factor of 0.6, and not multiplied, to get the (ice) cloud’s single-scattering optical thickness (you should mention that you speak about thin ice clouds; the factor is only valid in this case). Then, you would end up with a COT of 0.0208 and you have a bias of 4% and not 1.5%. On the other hand, you retrieve the COT distribution from the CALIOP Level 2 cloud layer product. To my knowledge, there is already a multiple-scattering correction applied in the CALIOP retrieval scheme for ice clouds (see, e.g., Winker et al. 2009, Young and Vaughan 2009). Thus it is probably not necessary to apply this multiple-scattering factor in your estimate. Please clarify!

We apologize for the confusion and have reworded this section to be more clear. First, “multiple scattering enhancement” has been changed to multiple scattering factor. Our treatment of multiple scattering here is correct, though we have included more details to allow readers to follow. You are correct in that the version 3 CALIOP cloud layer products already has a multiple scattering correction. CALIOP reports the single scattered COT. However, when we are looking at the level 1 product comparison, it is single + multiple scattering transmission that will introduce the bias, $\exp(-2*\eta*\tau)$ so we need to reintroduce the multiple scattering into the transmission term, thus multiplying by 0.6.

- p. 28371, l. 3–4: . . .at 20 km (multiply by 0.25). . . Please explain this factor! You may refer to Fig. 4 in Winker et al. 2009; from this figure it also becomes clear that the factor holds for nighttime only.

This was not clear due to a typo – the assumption was the feature is detected at 80 km, not 20 km. We scale the detection sensitivity from 5km to 80km by assuming the detection statistics are Poisson, so the detection sensitivity scales lower by the square root of the number of shots, or sqrt(240/15), which gives the 0.25 factor. We've made this more clear and fixed the typo. This does apply to both day and night lighting, which is evident in Fig. 4 of Winker et al. (2009) both the day (dotted) and night (solid) lines decrease as the horizontal averaging is increased.

- p. 28371, l. 7: Again, how do you deal with the multiple-scattering factor?

Here, we are using the same principal as above in that we need to use the transmission term of, $\exp(-2*\eta*\tau)$ where we are approximating the optical depth with the minimum detectable backscatter, a layer thickness, and lidar ratio. We've included a note after equation 10 that explains the need for a multiple scattering factor, and also discusses that no correction needs to be applied to aerosol layers because any significant AOT for aerosols occurs over a long vertical range (greater than 0.5 km), as discussed by Winker (2003). We have considered carrying the multiple scattering term in all of the equations, but as it only applies to ice clouds in this analysis we feel the note after equation 10 is less confusing.

Volcanic activity in Alaska, Kamchatka, the Aleutian and Kuril Islands has been increased since the eruption of Mt. Okmok in July 2008. Since then, stratospheric optical depth in the northern hemisphere has been considerably higher than the typical background values you discuss. Mattis et al. (2010) found aerosol optical depths of up to 0.025 at 532 nm in the upper troposphere and the stratosphere over Europe. The values might have been even higher over North America which is much closer to the source region. The aerosol layers are often visible in the CALIPSO browse images of attenuated backscatter, but not always in the VFM. You should check whether you see an enhanced bias in your comparisons after July 2008.

We already looked at this and the nigthtime biases are actually the highest in Aug 2006 and trend lower with time. The biases for 2008, 2009, (not published due to paper submittal in 2010) are lower than that of 2006 and 2007. You can see this by looking at Fig. 6a and Fig 8b. The larger nighttime biases observed in the summer months of Fig 6a all occurred in 2006, while all of the data points in Fig 8b are in early 2009 (note that the data points in Fig 8b are also contained in Fig 6a, but this separation allows you to see the biases actually are lower in 2009 than in 2006. We believe the larger biases in 2006 are due to stratospheric aerosol from the Soufrière Hills as seen in Vernier et al (2010). However, in 2010 (not published here due to paper submittal), the biases were on the order of 4% so could be due to some of the activity you note.

The aerosol plumes seen in Mattis are, as you say, not usually not detected in the CALIPSO VFM, and could cause very large biases in the data. Still, the thick aerosol layers that Mattis observes with an optical depth greater than 0.025 should be seen in the manual screening we apply. Essentially, we look at the average CALIOP lineplot (e.g. figure 2d) compared to the molecular signal for significant deviations where some aerosol or cloud may have made it past the VFM screening into the average. Note that we have not yet seen any aerosol plumes in this screening. We've modified the text in this section to include the results from Mattis et al. (2010) and increased our estimate of undecteced AOD to 0.015, or 3%. This value comes from adding the tropospheric + stratospheric AOD from Figure 1 of Mattis et al, (2010), not quite at the peaks, but within a month or so of volcanic activity as the AOT starts to fall off. We note care must be taken when comparing data closer than 1 month to volcanic activity where the plumes may be thicker.

p. 28372 – 28373, discussion in Section 4.4 and Table 2: You should clearly distinguish between uncertainties and biases (throughout the paper, see also the general comments). Only uncertainties (± errors) can be treated in a root-sum-square sense. The biases which are caused by the undetected cloud and aerosol optical depths between the HSRL height and the CALIPSO calibration height have a well-defined sign and do not cancel each other out in the root-sum-square sense. From Table 2, I would calculate your overall error to +3.7% ± 2.6%. Thus you would expect to obtain a positive value of the difference (HSRL – CALIOP) within these error limits, and that is exactly what you find in your data.

Good point, thank you. We've incorporated this comment as well as the updated bias values discussed here.

p. 28374, l. 11–16: Wouldn't aerosol loading in the CALIPSO calibration range lead to the opposite effect (CALIOP too high compared to HSRL, i.e. a negative bias)? Such a negative bias would reduce the positive bias you get from the unknown cloud and aerosol transmission above the HSRL. If you can quantify the bias, you may include it in your overall error estimate (see previous comment). Check whether increased volcanic activity can explain the larger positive biases in the respective measurements instead!

I see your point that the biases might be larger due to volcanic aerosol and hope my addressing of your previous comments have alleviated those concerns. However, I do not think that aerosol loading in the CALIOP calibration region will result in a negative bias (i.e. CALIOP attenuated backscatter too high). Let's take a look at equation 2. Aerosol loading in the CALIOP calibration region will result in the measured power, P(rc), (in the numerator) being slightly larger than if a purely molecular atmosphere is assumed, correct? Not accounting for aerosol backscatter (in the denominator), as CALIOP does not, will therefore result in the calibration constant too large. Now, we plug this too large calibration constant into equation 1 (in the denominator), resulting in the CALIOP attenuated backscatters being too small (i.e. positive bias in our context).

p. 28374, l. 28: Give a hint to Fig. 7.
Done, thank you.

p. 28378 – 28380, Section 6: Please consider all aspects discussed above in your conclusions (especially wrt biases and uncertainties).

TECHNICAL CORRECTIONS
In general all equations should be embedded in complete sentences, including punctuation. Please check all occasions where you refer to the difference HSRL–CALIOP for consistency, especially the figures and tables. Sometimes you write HSRL–CALIPSO or LIDAR–CALIOP.

Thank you - we were initially a bit loose in the CALIOP/CALIPSO terminology but have corrected it. The Lidar-CALIOP reference only applies to Table 3 and is intentional as many different lidars are described in the table and opted for the generic term lidar instead of including each system (or network) acronym.

Check your reference list. There are several inconsistencies between the citations inthe text and the reference list (e.g., Hunt et al. 2009, McGill et al. 2006 or 2007).
Done.

p. 28356, l. 13: delete “an”
Done.
p. 28359, l. 13: long-term
Done.
p. 28359, l. 16: primary (not primarily)
Done.
p. 28359, l. 24-26: Check sentence wrt level 1 and level 2.
Done.
p. 28360, l. 2: coefficient (not coefficients)
Done.
p. 28361, l. 12-13: Subscripts of beta must be parallel (not perpendicular).
Done.
p. 28365, l. 15: . . .to produce the 532 nm total. . . (delete “of”)
Done.

p. 28366, l. 17, Eq. 10: Subscript of r in the middle part of the equation should be 30 km.
Done.
Done.
p. 28371, l. 4: Please correct the sentence: . . .a lidar ratio of a mean lidar ratio. . .
p. 28371, l. 15: Jäger (not Jager)
Done.
p. 28374, l. 1: You obviously mean Fig. 6, not Table 2.
Done.

p. 28376, l. 26: . . .this selection criterion (not criteria).
Done.
p. 28388, Fig.1: What campaign is “CALIPSO” in the map?
Done – it was Other.

p. 28394, Figure caption 7: . . .same as Fig. 2 (not Fig. 1)
Done.