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Discussions

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Interactive comment on "Radiometric calibration of a non-imaging airborne spectrometer to measure the Greenland Ice Sheet surface" *by* Christopher J. Crawford et al.

Anonymous Referee #2

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This paper reports on the use of airborne measurements of shortwave spectral reflected radiance and incident spectral irradiance to aid in ICESat-2 validation. Much of the effort reported in this paper is on calibration and characterization of the instruments and on comparison of measurements with radiative transfer simulations over a full dynamic range from darkest to brightest surfaces. In the spirit of reporting the details behind the calibration, testing and analysis, and in the context of this journal, I think this is a publishable manuscript. However, I think the authors must take the time to revise the manuscript based on the numerous comments that follow. I prefer not to segregate between major and minor revisions; the number of comments suggest that in total, the requested revision is major.

Some general comments: the number of acronyms used in the paper make reading very difficult. At times one would benefit from a key to keep track of the multitude of acronyms, many of which are nearly identical. There is also widespread application of jargon. For example, "at-sensor" can be stricken from the entire paper since only at-sensor radiance is investigated. Benchmark and benchmarking is used repeatedly – never once is it defined. Moreover, I would argue that the comparisons between measured and modeled spectra is misinterpreted. In the paper, it is defined as accuracy – it most certainly is not. If detailed error analysis of the simulations is conducted I think the authors will find significant overlap in their respective uncertainties. The conclusion will probably be that they agree to within their uncertainties, likely to be on the order of about 10%, but variable across the spectrum. It is on that basis that conclusions must be drawn. Then they can address that if 2% uncertainties are required, what can be learned? The authors must make revisions along these lines; a model uncertainty budget must be conducted.

Here are the detailed comments, with page and line numbers listed for most:

1. p. 2, l. 30: "benchmark"; Instead of using a term like this it is probably better to be specific about how you intend to use your simulations of spectral radiance and irradiance. As it currently reads, the measurements are to be tested against the modeled "truth". This begs the question, why are the measurements even needed?

2. p. 3, l. 3: what are "profile measurements"?

3. p. 3, l 29: "...within infrared wavelengths..." I think you mean "...in the near-infrared..." Also, you should be more specific and identify water as the absorber.

4. p. 4, l. 7: You should state the fields-of-view. I note that it is done later but it does not hurt to list them here.

5. p. 4, l. 14: "at-sensor" is jargon best used when there is some ambiguity about the vertical location of the radiance: water- or surface-leaving, etc. In this context, where

else can a sensor measure radiance other than at-sensor?!

6. p. 4, l. 18: spectral response functions of what?

7. p. 4 l. 15-18: the list of model input in confusing. Why are standard aerosol profiles and CIMEL measurements (of what? Presumable aerosol optical depths?) both used? What are the assumed state parameters? US standard atmosphere?

8. p. 4 l. 28: "gold standard" is jargon. What characteristics make it the best comparison reference? Does it have the appropriate near-infrared channels for this study?

9. p. 4 l..32: "sunlight" should be "sunlit" and change to "... regions with greater than 5..."

10. p. 4, l. 33: "... challenges that are a result of ..." Aren't the items that follow actually the challenges? If not, then state what the challenges are.

11. p. 4 l. 34: "longer path": longer than what? "greater atmospheric refraction": greater than what?

12. p. 5, l. 23: what is the remote cosine receptor? A transmissive or reflective diffuser? Integrating sphere?

13. p. 6, l. 13: The SWIR1 and SWIR2 detectors had not yet been identified. After going back, I think I know what they are – you need to state explicitly. This makes already confusing notation even more confusing: sometimes the acronyms use in this paper are longer that what they are meant to represent!

14. p. 6, l. 13: I have a hard time understanding what "the entire airborne mission that included a dark current subtraction during each flight" is supposed to mean.

15. p. 6, l. 28-29: What was optimized? Gains?

16. p. 6, l. 28: What is the NIST source? Lamp? Lamp plus integrating sphere? And is it really a NIST source or is it NIST-traceable?

17. p. 7. l. 2: point out the water vapor absorption bands evident in the stability curve of figure 2.

18. pp. 6-7: The figures in figure 2, especially the linearity curve in the upper left needs to be better explained, either in the text or the caption. The "optimization" in the abscissa is not explained.

19. p. 7, l. 4: Is 1 nm resolution the full-width half-maximum of the slit function, that is, spectral resolution? Or is it sampling resolution? And the wavelength precision of 2%: is that 2% of the wavelength scale (for example, 20 nm at 1000 nm, terrible) or 2% of the sampling resolution (2% of 1 nm, very good). Why not remove such ambiguity and list the precision in absolute units, nm?! And finally: the instrument spectral and sampling resolutions must be stated earlier in the text.

20. p. 7, l. 6. Now using Fieldspec 3 or Pro is extremely confusing and requires a scorecard or flipping back to see which instrument is which. Unless the reader works for ASD or used their products, they won't care. Please use the same identifying notation (how about simpoly zenith and nadir spectrometers?!) throughout?

21. p. 7, l. 7: What is the significance of "a PANalytical company"?

22. p. 7. L. 8-9: "less than 2% for 1 nm resolution". Same comment as above.

23. p. 7, l. 22: "per manufacture specifications." Do you mean "in agreement with manufacture specifications"?

24. p. 7, l. 24-26: I don't understand this sentence. Are you saying that window transmission should be appreciably larger than instrument stability? But this leads to a more important question, relevant to the previous paragraph: why wasn't a calibration made with the window/dome in place?

25. p. 7, last paragraph: The listed accuracies are really uncertainties rather than accuracies. How were they derived? Was a correction for window transmittance made? How was the solar zenith angle factored in to the uncertainty? For the zenith measure-

ments (presumably using the cosine receptor) that will likely be the largest source of error, especially if the platform was not actively leveled and in the Arctic where solar zenith angle is quite high.

26. p. 8, l. 6: Is it really an "in-flight radiance calibration strategy" or a strategy to optimize gain and integration time settings? If it is really in-flight-calibration, please explain what standard sources or detectors you are using during flight.

27. p. 8, l. 9: same as above.

28. p. 8, second paragraph: How about the effect of aircraft attitude on solar zenith error and its impact on downwelling irradiance error? (never mind; this is a topic for the next section, but it is not cover there either.)

29. p. 8, last paragraph: what is the threshold for setting the flag?

30. p. 9, l 8: You should probably say more about "cloud contamination". Why do clouds limit the retrieval of surface properties from spectral reflectance measurements? After all, since you are measuring incident irradiance (at flight altitude) it might seem like clouds can be accommodated.

31. p. 9, l. 9: "calibration strategy" again. See previous comment.

32. p. 9, second paragraph: I don't understand this – it seems like it defeats the entire purpose of measuring incident irradiance!

33. p. 9, l. 24: By "direct path" do you mean directly transmitted irradiance? A horizontal translation of the aircraft will be insignificant compared to pitch and roll offsets! I have yet to see this considered, or the angular response of the cosine receptor presented.

34. p. 9, l. 9: The mysterious NIST-traceable source has yet to be identified.

35. Section 3.3.2: I cannot tell if the angular calibrations are for azimuthal or zenith response or both. This is very important. On the other hand, if directly transmitted

solar irradiance cannot be sensed with the orange can in place it does not matter!

36. p. 11, l. 15-16: What is "direct cloud-sky"? Does that imply a broken cloud field? Cloud optical depth low enough such that direct transmittance is appreciable?

37. p. 11, l. 17: Listing times suggests the window can be nothing other than temporal.

38. P. 11, l. 20: Parabolic corrections for what?

39. p. 11, l. 25: Discriminate diffuse sky conditions from what? Or do you mean "identify diffuse sky conditions"?

40. p. 11, l. 31: "...discriminator for sky conditions more broadly..." More broadly than what? I cannot understand, either from the text or figure 7, how the zenith measurements can discriminate between different types of diffuse sky sources. Considerably more explanation is required.

41. Figure 7: I assume the radiation model was plane-parallel; how did you account for the complex cloud geometries? Nothing was said about the modeling in either the text or the captions.

42. p. 12, l. 5, first sentence in paragraph: I think you need to be more specific about what the science is. I assume you are trying to retrieve surface reflectance. You must state this specific application of these measurements. Many other applications do not require surface reflectance validation.

43. p. 12, l. 26 (and many other places): Again, the use of the term "benchmark" comes with no qualification. What do you intend to do with the MODTRAN simulations! Of course, compare them to measurements, but toward what end?

44. p. 12, l. 30: Not sure how you can check for changing solar illumination if you cannot see the sun with the orange can!

45. p. 13, eq. 3: This equation does not contain the atmospheric transmittance from TOA to flight altitude or from flight altitude to ground. Is that what is meant by "apparent"

reflectance? After finishing the paper, I don't see that this was ever considered. It is either flawed or you need to explain how it was applied.

46. p. 13, l. 21: "...to parameterize MODTRAN..." makes no sense. Do you mean "...to initialize MODTRAN..."?

47. p. 14, l. 6: Those are usually called slit functions, not spectral response functions. And finally, a mystery solved (from fig. 11): spectral resolution is 3 nm and 10 nm; 1 nm is sampling resolution. Please state this in the text.

48. p. 14, l. 15: Again, poor or confusing usage: "...successfully constrain MODTRAN ..." And as before, do you mean initialize? Or are you really constraining MODTRAN output over a range of input? The caption in Fig. 12 provides no clue. And once again, I think "parameterize" is misused again in the following line.

49. P. 16, l. 4-6: Is accuracy defined to be the relative difference of measured reflectance from MODTRAN reflectance? This is not accuracy! It is just that, a difference between simulation and measurement. Do the simulations have no error? And if not, why are you even trying to make these challenging measurements!

50. p. 16, l. 11: I was hoping this paragraph would quantify uncertainty in the model; alas, it does not. Without it is impossible to assess the significance of comparisons in fig. 14 and in the OLI comparisons presented on the next paragraph.

51. p. 17, l 13: Spectra is the plural of spectrum, not spectrums.

52. P. 17, l. 20-21: Again, what is listed in uncertainty, not accuracy.