Author response to D. J. Diner

D. J. Diner's comments are in black and author comments are in blue.

This is a generally well-written paper explaining the AirHARP calibration process. Several detailed comments/questions are below.

The authors thank D. J. Diner for their time and thoughtful review of the manuscript. We will respond to each comment individually and in the case of small edit recommendations, we may chunk several comments together and respond underneath.

Lines 65-72: For consistency with earlier text, suggest changing "MSPI" to "AirMSPI".

Line 67: Change "gimble" to "gimbal".

Line 68: Change "FPA" to "focal plane array (FPA)" (spell out acronyms when first used).

Line 68: The AirMSPI FPA is two-dimensional but not used in the sense of a framing area array. Perhaps change "two-dimensional" to "pushbroom".

We will address these minor errors in the manuscript in the revision (and any others mentioned below). Thanks for the recommendations.

Line 81: I believe PACE launch is now in 2024.

Yes, the PACE launch is currently set for January 2024. We will update this reference.

Lines 95 and 832-833: Check the Alternative Vision link. I couldn't get it to work.

One of the reviewers also mentioned the link was dead. We will refer to Borda et al. (2009) instead in the revision.

Line 150: The Semiconductor Components Industries 2015 reference is missing.

Thanks for catching this, we may have omitted this by accident. The reference is a technical spec sheet and is here: https://www.imperx.com/wp-content/uploads/2017/08/KAI-04070-D.pdf

We will add this to the references in the edit.

Line 172-173. If the dark counts are sensitive to operating temperature, then they are either related to dark current, or there is some offset in the video signal that is temperature sensitive. Counts attributed to dark current would presumably be sensitive to integration time. Do you have an explanation of why the dark counts are not integration-time dependent?

Copied from the response to Reviewer #1, who also commented on this –

This is a good catch and as phrased, could be a little misleading. Dark counts are sensitive to integration time in general for CCDs. However, at the range of integration times we use operationally in flight (10-20ms), we do not see a significant variation in dark counts at a given temperature. In-flight detector temps were often on the colder side (< 25 C, and in the lab we typically saw > 35 C), so the dark values were closer to the ADC bias level. During both LMOS and ACEPOL campaigns, we kept the instrument at a single 20ms integration time as well. We will change this sentence to discuss temperature only.

Equation (2) and surrounding text: It might be helpful to put a subscript on the delta parameter to show that it is pixel dependent. Does the $DN_{raw\,[0-200,1848-2048]}$ represent a mean (or perhaps median) of dark values over pixels 0-200 and 1848-2048? Are 0-200 and 1848-2048 the full range of masked pixels, because in line 209 you state that the active science area is pixels 200-1800.

We can see how this notation could be somewhat confusing. We propose this new format for Eq. (2):

$$DN_{dark} = DN_{\widehat{dark,lab}}\,\delta$$

This is a better notation because the dark lab template (with the carat to signify normalization) is the workhorse of the correction. The delta would be better served as a factor that comes from the vignetted area of the sensor in the respective live data capture (and further explained in text).

The 0-200 and 1848-2048 are the active science pixels cross-track. The 1800 on line 209 is a typo, we will change in the revision. Thanks for catching that.

Lines 198-199: Define the terms port fraction and sphere multiplier. The Perkin Elmer reference is missing.

Copied from the response to Reviewer #2, who also commented on this –

We will add to this paragraph that the port fraction of an integrating sphere is the ratio between the internal surface area and the area of open holes and apertures. The sphere multiplier characterizes how much the reflective surface and port fraction of the sphere contributes to increasing the light output relative to the source. We will also add the PerkinElmer citation, which we accidentally omitted from the reference list.

Line 204: The smoothing process could cause individual pixels with greater or less responsivity to have a residual calibration artifact. Has this been considered?

This has been considered in the flatfielding process, but as of this work, only the pixels with large offsets relative to the smoothed response (typically defects/contaminations, etc.) are addressed. These pixels are masked as the imagery moves through the L1B pipeline. A more sophisticated quality flagging will be in place for HARP2 calibration/L1B products next year.

That said, most of the pixel-to-pixel relative structure is described by the smoothing window in Figure 4b. In general, these statistics across the FOV are distributed within 0.5% (1 standard deviation of the Gaussian distribution of residuals after the flatfield is applied back to the generating dataset). Binning pixels in L2 retrievals, as was done in Puthukkudy et al. (2020), McBride et al. (2020), and Gao et al. (2021) will further smooth the variation in sensitivity between neighboring pixels.

Line 206: The andor.oxinist.com link does not work.

We will find a new reference. Thank you for catching this.

Line 209 and Figure 4b: If the active area is pixels 200-1800, why don't the plots cover this full range? Is this because of the sliding smoothing window?

Yes, the smoothing window algorithm used here may create artifacts at the edge of the active science area, since the vignetted portion of the sensor contains far lower counts. One way to solve the edge artifacts is to mask the vignetted area and linearly extrapolate the flatfield at the edges of the science area by using the trend of the last 10 or 20 pixels in each direction. We will revisit these plots in the revision.

Line 218 et seq.: Perhaps put subscripts on the flatfield correction factor *f* to indicate that it is pixel (and row) dependent.

All terms in Eq. (3) are pixel-dependent and this is explained in text.

Line 234: Are the optics perfectly telecentric, so that the AOI on the detector is exactly 0º for all pixels? In practice there is generally some slight non-telecentricity on the order of a fraction of a degree to a few degrees.

The optics are telecentric to less than 1° over the entire image plane, with a large majority of chief rays within 0.5°.

Section 3.3: The linearization curves are derived by changing integration time, but not illumination level. Have you done any tests to verify that reciprocity holds? Why is linearization done after flat-fielding correction; shouldn't linearization occur first?

Yes, we have analyzed sphere radiance data from AirHARP without the non-linear correction applied and we see a similar behavior when varying the sphere illumination level at a constant integration time. The reason why we vary integration time in this test is to verify that we can change integration time on orbit and expect a linear gain response (after this correction is applied).

This could be done the other way around, but it requires more degrees of freedom in the test environment. Light source illumination needs to be cycled for each integration time, and each new light level requires time to stabilize. Therefore, it is not as efficient as varying integration time for a stable illumination level.

And yes, since linearization is relative to the ADC, it should be done before any superficial corrections, like flatfield or polarization calibration. Here, we are studying linearization along the AirHARP optical axis where the flatfield is set to 1, so it is effectively the same thing.

Line 418: Define what a superpixel is, as this is the first time this term is introduced.

Line 424: Spell out SRF when first used (applies to all acronyms).

We will do a dedicated pass through the manuscript and define all hanging acronyms. The other reviewers also emphasized this revision.

Lines 444-446. Text indicates that narrowing of the leading edge of the 870 nm channel was discussed earlier, however I could not find such discussion.

Thanks for the catch. This may have been omitted from an earlier section before submission but the reference remained. We will revise this in the edit.

Lines 446-545. The rolloff at the short wavelengths of the 440 nm band changes the effective band center and bandwidths of this channel. I don't follow the logic "Rayleigh-like" SRF adjustment. Why not simply represent this channel by its effective spectral parameters that roughly represent the same radiometric output as the actual SRF?

A "Rayleigh-like" SRF adjustment would use the fact that most of the TOA signal at 440nm will come from Rayleigh scattering and the Rayleigh scattering efficiency with wavelength is sloped in the opposite direction of the SRF slopes in each detector (higher at shorter wavelength, lower at longer wavelength). Using a unique "Rayleigh-like" correction as a function of wavelength for each sensor could adjust their responses to reflect a more square SRF. However, as of this work, this has not been applied at the L1B stage.

Your recommendation may work well, too. Understanding the impact of this differential SRF on the L1B is still an active area of research in our group. This effect is not seen in HARP CubeSat or HARP2 SRFs.

Line 461: Leading and trailing edges suggest something to do with flight direction. Blue edge or short-wavelength edge, and red edge or long-wavelength edge would be better terminology in my opinion.

We apologize for the confusion. Short-wavelength and long-wavelength edge are reasonable substitutions. We will address this in the revision.

Line 469. I think you mean Eq. (15).

Line 475: Do you mean solar zenith angle rather than view zenith angle?

Yes, both of these will be addressed in the revision.

Line 560: Since this refers to testing in late 2022, were the referenced measurements acquired?

This was also brought up by reviewer #2. We have decided to remove this reference and date from the paper. While yes, the data was acquired, we prefer to focus on AirHARP here, and leave HARP2 results on POLBOX to a future publication.

Line 611: Change “RSB” to “RSP”.

Lines 686-688: I would have expected the sunglint pattern to have a steeper variation in signal as a function of view angle than the desert scene, so I don’t follow the argument that the ocean signal is more robust against angular misregistration.

This comment was about the actual surface features that are present at the Rosamond Dry Lake, versus potentially smoother ocean surface (especially off-glint). The Dry Lake has some spatial variation that could impact the intercomparison in this way. Off-glint ocean, for example, maybe less so. Of course, this depends on ocean scene – heavy phytoplankton loading or high winds that chop up the ocean surface may create similar variations, especially near glint. Thank you for your insight on this - we may remove this statement in the revision. It may require more explanation than what it is worth.

Figure 4 caption: Perhaps change “only the SNR remains” to “only pixel-to-pixel variations due to noise remains”.

General comment: Directional measurements of radiance, multiplied by pi and normalized by illumination irradiance, are referred to as “reflectance” throughout the paper. Per Schaepman-Strub et al. (2006), Rem. Sens. Environ. 103, 27-42 and Nicodemus et al. (1977), NBS publication, reflectance is a ratio of fluxes (exitance/irradiance), hence a measure of albedo. In the nomenclature defined in these publications, the quantity referred to as reflectance in McBride et al. is a “reflectance factor” for illumination at normal incidence.

Thank you for this observation. We will make the necessary changes and add these references.