

# Author’s Response to Reviewer #1

## AIRS Deconvolution and the Translation of AIRS to CrIS Radiances with Applications for the IR Climate Record

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### 1 General Remarks

We thank the reviewers for their thoughtful comments and have tried to incorporate or respond to all suggestions and questions. In the remainder of this section we summarize the main changes we’ve made. Section 2 consists of reviewer’s comments followed by our responses.

The discussion of reference truth for the deconvolution (starting on page 3 line 30 RHS of the original submission) was not clear. We have updated the labels in figures 3 and 4 from “gauss” to “decon ref” to better indicate we are showing the deconvolution reference truth. We updated the associated discussion to emphasize that the deconvolution reference truth is intended as validation of the deconvolution—we don’t need it to do the deconvolution or for subsequent reconvolution to CrIS or other targets.

We corrected the relationship of standard deviation and full-width half-max (FWHM),  $s = \text{FWHM}/(2\sqrt{2}\ln 2)$  in the original submission, to  $s = \text{FWHM}/(2\sqrt{2}(\ln 2)^{1/(2p)})$ . The latter is correct for the generalized Gaussian. The difference is small for the range of values  $p$  we used. As noted above this does not effect the accuracy of the translations, just (to a very small degree) reference truth for the deconvolution and the basis functions for our L1d “idealized grating model”.

The section on regression translation has been revised and shortened. We dropped the paragraph on adding noise since we don’t show those results. The direct and principal component regression matrices are now shown only for the MW, enough for an overview. The regression matrices are now for apodized radiance, matching the residuals. The interpolation section in the appendix has been revised to show apodized residuals and to emphasize the advantage of using both the source and target response functions.

### 2 Response to Comments

- p.1 Line.14 r.h.s.: It would be useful to introduce here, rather than on p.3 and p.6, that the CrIS instrument has two modes of operation, the nominal and full spectral resolution (NSR and FSR, respectively). It should also be mentioned that CrIS was in the NSR from launch in 10/28/2011 through Dec. 2014 or Dec. 2015 (after bit-trim mask upgrade, depending how much detail you want to cover here).

REPLY: We agree this info should be given, but in section 3 the (AIRS to CrIS translation) rather than the introduction. There is a limit to what belongs in the introduction—for example we do not get into other significant details, such as AIRS L1b vs L1c, there. The key idea of the paper is that we can get a modest resolution enhancement from the AIRS deconvolution and we do say that in the introduction. As it turns out this is good enough for NSR but probably not FSR, but again the CrIS section is the natural place for that discussion.

- p.1 Line.60 r.h.s.: “The SRFs are not necessarily symmetrical, especially at the high end of the band.” The SRFs are not necessarily symmetrical due to fringing in the AIRS entrance filters, especially at the high end of the band is repetitive and can be combined into one sentence.

REPLY: Right, we fixed this.

- p.1 Section 1: It might be best to simply include a table of significant instrument attributes for AIRS, IASI, and CrIS. Things like type (grating or interferometer), spectral sampling, resolution, launch dates, number of channels, and NEDN at selected frequencies.

REPLY: This would be interesting, but we can’t really justify discussing IASI here in any detail, and AIRS and CrIS are different enough that the differences are not easily summed up in a table. A short descriptive paragraph, expanding a bit on what we have now, is probably best. The suggested table would find a better home in a paper dealing with the many questions beyond response functions that come up in building a common data set.

- p.2 Fig.1: It might be worth plotting this on a vertical log scale. The real AIRS SRFs have a long tail that is not adequately demonstrated in a linear vertical scale. The significant overlap mentioned on p. 1 was by design as a Nyquist sampled spectrum was desired (as is IASI and CrIS) but there is additional significant overlap caused by the long-tail. These details should be briefly mentioned so the reader is not misled into thinking that AIRS can be represented by a simple Gaussian function in the equation that follows later on this page.

REPLY: It’s interesting to look at the SRFs this way, and as you note the Gaussian approximation does not have the long tails of the tabulated SRFs. We’ve added a comment to this effect. But we aren’t proposing to use the approximation as an alternative to the tabulated AIRS SRFs. Our applications are reference truth for the deconvolved radiances (where they are used at a different spacing and higher resolving power than AIRS L1c), and as basis functions for the idealized grating model of section 4.

- p.2 line 25 r.h.s.: on the first uses of the  $\|r\|_2$  it would be helpful to let the reader know this is a Euclidian norm or even more obvious, a root sum square.

REPLY: We’ve added a note to that effect. The notation used in the paper is taken from the Wikipedia articles for the Moore-Penrose inverse and mathematical norm, and we added both as citations.

- p.2 line 37-39 r.h.s.: The  $c$  in this equation is not the same as the  $c$  used above (in  $S_b r = c$ ). Suggest that a different symbol be used to avoid confusing the reader.

REPLY: Agreed, we replaced this with  $s$ .

- p.3 line 45-50 r.h.s.: The CrIS NSR mode will ultimately become a historical oddity for S-NPP. It is irrelevant for JPSS-1 to JPSS-4 (ultimately to be known as NOAA-21, 22, 23, 24 if they survive launch) as the NSR will no longer be processed. Thus, the authors need to justify why they would degrade the entire AIRS 2002 to 2016 and S-NPP/NOAA-20+ record from 2016 to 2030s instead of potentially finding an alternative higher resolution grid. For example, consider the loss of spectral information in the AIRS carbon monoxide spectral domain when going from AIRS SRFs to CrIS NSR the CO band information is completely lost. Similar loss occurs in the 2390  $\text{cm}^{-1}$  R-branch that affects lower tropospheric temperature information.

REPLY: We agree, medium to long term. But in the meantime we have the significant overlap of AIRS and CrIS NSR data to work with. The effective resolution of deconvolved AIRS does not take us quite to CrIS FSR for the MW and SW bands. You can still do a translation but the residuals are relatively large in comparison with those shown in the paper.

One solution might be to pick an intermediate resolution for CrIS, for example 0.6  $\text{cm}^{-1}$  in the MW, that roughly corresponds to the AIRS resolving power, and we've added a note to that effect. This is easy to do for both regular CrIS processing and our AIRS to CrIS translation. In both cases we have an intermediate representation—sensor grid for CrIS and our deconvolution grid for AIRS—that can be resampled to any nominal resolution we like. We've expanded the discussion of NSR and FSR at the end of the CrIS translation section and used this topic as a lead-in to the next section, translation to an idealized grating model.

- p.3 Fig.4: I had a tough time understanding exactly what was plotted here and I think the text and caption could be improved. I believe AIRS is the original AIRS radiance. Kcarta is the 0.0025  $\text{cm}^{-1}$  LBL representation, the Gauss and decon are a comparison of the 0.1  $\text{cm}^{-1}$  intermediate resolution where Gauss is derived from direct convolution of k-Carta and decon is derived by the pseudo-inverse process but I am not sure if I have Gauss/Decon flipped. It would be best if the plot labels and text were identified explicitly.

REPLY: Agreed. We've update the figure labels and associated discussion. In figures 3 and 4 the old label “gauss” is now “decon ref” (reference truth for the deconvolution) and the old “decon” is now “AIRS decon” (deconvolved AIRS radiances).

- p.4 line 34 l.h.s.: given that Hamming apodization is reversible there cannot be any loss in spectral resolution. Thus, the appearance of the radiances in unapodized (or deapodized since there is self- apodization effects with CrIS) versus Hamming apodized only has to do with non-local ILS effects (i.e., side-lobes of the SRF) and not resolution. The 15 micron band line spacing is a resonance that is aliased with the CrIS OPD, such that the sinc() function produces a distortion of that band (i.e., the peaks and troughs are exaggerated).

REPLY: We agree there is no loss in interferometric resolution with an invertible apodization. Hamming apodization does reduce resolving power  $R = v_i/\text{FWHM}_i$ , since it increases FWHM while decreasing the side-lobes. We changed “resolution” to

“resolving power” and set Barnett et al. 2000 as noted below as our first citation for apodization, for the sentence in question.

- p.4 line.34 l.h.s.: The AIRS SRF is a modified Gaussian that is mostly a local function. It seems odd that the authors would propose convert AIRS to a unapodized SRF since the sinc() function is a non-local function. That is, all AIRS channels would be necessary to produce a CrIS sinc() function. Alternatively, if one were to convert the AIRS channels to CrIS Hamming channels then each CrIS channel would be reconstructed from a small and local set of AIRS channels. The Hamming apodized radiances could then be converted to unapodized via a transformation as suggested in Barnett, C.D., J.M. Blaisdell and J. Susskind 2000. Practical methods for rapid and accurate computation of interferometric spectra for remote sensing applications. IEEE Trans. Geosci. Remote Sens. v.38 p.169-183.

REPLY: The deconvolution-based translation is a two-step process: (1) deconvolution from AIRS L1c channels to a regularly spaced  $0.1 \text{ cm}^{-1}$  grid spanning the AIRS channels, or an AIRS and CrIS band intersection, followed by (2) a conventional convolution (via resampling or a zero-filled double Fourier transform) to the CrIS user grid, followed by optional Hamming or other apodization. So all AIRS channels are in fact available (in the sense of contributing to the  $0.1 \text{ cm}^{-1}$  grid) to produce a CrIS sinc response function.

For the regression of section 5, if we regress against unapodized radiances and then apply apodization we get results very close to regression against apodized radiances, and if we regress against apodized radiances and apply a deapodization, we get results very close to regression against unapodized radiances. So for both regular and principal component regression it doesn't seem to matter if the target is apodized or unapodized CrIS radiances.

We have found an apodized target as suggested in your citation is a powerful tool for doing fast transmittance radiative transfer models—it was much easier to do the coefficient fitting with apodized transmittances, and deapodize radiances as needed.

- p.4 line 39-56 l.h.s.: this discussion adds confusion. It looks like the 49 set will be used for training and the 7377 is the independent set. Seems like this could be said most succinctly.

REPLY: Agreed, and we've rewritten this paragraph. The 49 profile set is always the test or independent set, as it makes for a more strict test.

- p.4 line 54-59 r.h.s. Could the authors give some explanation as to the physical basis for the statistical correction they are about to discuss. I was completely lost as to why this should be necessary. One concern came to mind and I will share it here. It seems like this “ringing” or “regularity” could be caused by trying to compute a sinc() function from a regularly spaced intermediate Gaussian (that also has side-lobes). The fact that it is diminished by Hamming says it might be an artifact caused by the sinc() side-lobes or band edges. Maybe this is a naive idea (or at least it might give you a clue how to improve this discussion), but why did you not consider  $0.1 \text{ cm}^{-1}$  boxcars for the intermediate spectrum that would have perfect localization?

REPLY: the motivation for the statistical correction was simply that the residuals showed some regular structure and the standard deviation was relatively small. The

deconvolution is an imperfect process, so it seemed plausible that a correction might help.

The Gaussian basis was used to convolve kcarta radiances to get reference truth for the deconvolution; it is not actually used for either the deconvolution or subsequent reconvolution to CrIS radiances. We did try a number of other functions for this reference truth, including the 0.1 cm<sup>-1</sup> boxcar you suggest. As I recall 0.2 cm<sup>-1</sup> gave slightly smaller residuals, but both were larger than for the generalized Gaussian.

Figure 5 shows two adjacent rows of the deconvolution transform,  $S_b^{-1}$ . This is applied to channel radiances  $c$  to get deconvolved radiances  $r = S_b^{-1}c$  at the 0.1 cm<sup>-1</sup> grid. Each deconvolution point is a linear combination of several AIRS channels. So it seems at least plausible that a generalized Gaussian would work as basis functions for reference truth.

- p.6 line 48-55 l.h.s. and p. 8 line 52-55: For most applications the noise spectral correlation is something that needs to be specified. Since you have used linear transformations you have potentially altered the amplitude of the random component of NEDN and potentially added significant correlation (see Barnet 2000 reference for a discussion of this in the application of apodization). Any data assimilation or retrieval application would be strongly impacted by this correlation. Given that one application of this methodology is for climate data records, it is worth discussing if the NEDN can be accurately translated. The AIRS, IASI, and CrIS have similar information content in the signal-to-noise; however, if only the signal (radiance spectra) are transformed and not the noise than this process could, in fact, alter the information content such that AIRS, IASI, and CrIS cannot be combined in a meaningful manner. This thought is tied to the comments given on p.3 line 45-50 r.h.s. above.

REPLY: This is a good point, but a problem for any sounder to sounder translation. I think the best we can do here is to mention this and cite Barnet et al. 2000 again for this matter. We could model the correlation and add correlated noise to the CrIS radiances to better match the AIRS to CrIS translation, if that seems desirable. But that's a matter for future work.

- p.6 line 47-48 r.h.s.: see comment for p.3 line 45-50 r.h.s. this seems worthy of a bit more discussion given that this approach has been advertised as a solution for a climate data record.

REPLY: Agreed, we've expanded this, as discussed earlier.

- p.10 line 38-58 l.h.s. (and p.4 line 47-49 r.h.s): this is a trivial point. The conventional interpolation would be the wrong thing to do. For interferometers an exact transformation could be done via cosine transforms (presumably what you are doing for the IASI-to-CrIS transformations). The issue here is what is the best manner to transform a grating instrument with non-ideal SRFs to an interferometer system. Maybe this discussion is leading towards an alternate solution converting all instruments to an idealized SRF which preserves the signal-to-noise as best as possible. This would be an ideal localized function that is Nyquist sampled and has minimum noise correlation (similar to the discussion on p.7 l.h.s., but with the noise component discussed also). The AIRS is nearly a localized, Nyquist sampled, spectrum

whereas the CrIS and IASI unapodized (or more properly deapodized) spectrum is not-ideal and introduces significant spectral distortion. The CrIS and IASI apodized spectra is nearly localized by the Hamming function has significant (1%) side-lobes that diminish somewhat slowly (see Barnett 2000 for discussion).

REPLY: Some related applications do start from conventional interpolation of AIRS data. For example the JPL algorithm for small frequency shifts uses a cubic spline augmented with a statistical fit for the derivatives near the initial channel frequencies. And in the past we have used spline interpolation to a regular intermediate grid before reconvolution via the cosine transform; that is, we used spline interpolation in exactly the same way we use deconvolution here.

The “L1d basis” was our take on the idealized SRF you suggest. It started out as an attempt at finding an approximate, relatively simple model for the AIRS SRFs. We agree this is an interesting alternative as the target for a common data set. A key point of the paper that the AIRS deconvolution could be useful for more than one final target.