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 all suggestions and respond to all questions. Section 2 below includes 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 simply a check of the deconvolution step—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), $s = \textsc{fwhm}/(2\sqrt{2\ln 2})$ in the original submission, to $s = \textsc{fwhm}/(2\sqrt{2}\,(\ln 2)^{1/(2p)})$. 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) the consistency of reference truth for the deconvolution and the basis functions for our L1d "idealized grating model".

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 added, but in section 3 the (AIRS to CrIS translation) rather than the introduction. There is a limit to what fits in a sensible 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, 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 fit better in 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 SRFS. The applications are (1) reference truth for the deconvolved radiances at a different spacing and resolving power, used for understanding the deconvolution, and (2) 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 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 cm^{-1} in the MW, that roughly corresponds to the AIRS effective resolution, 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 cm-1 LBL representation, the Gauss and decon are a comparison of the 0.1 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 Barnet 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 lon-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 Barnet, 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 short answer is that the deconvolution-based translation is a two-step process, and the deconvolution is independent of the final translation target. The Gaussian basis is used to convolve kcarta radiances in an attempt to get a rough reference truth for the deconvolution; it is not actually used for either the deconvolution or subsequent reconvolution to CrIS radiances.

For what we called "direct regression" discussed in section 5, we did try a one-step translation from AIRS to apodized CrIS, taking advantage of the relatively limited span of the apodized response functions. If that had worked better we could invert the Hamming apodization and get unapodized radiances that way. We use that technique in other contexts, for example to get unapodized CrIS radiances from an apodized fast model.

- 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 nave idea (or at least it might give you a clue how to improve this discussion), but why did you not consider 0.1 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 relativly small. The deconvolution is an imperfect process, so it seemed plausible that a correction might help.

  We emphasize again that the Gaussian basis was used to convolve kcarta radiances in an attempt to get at least a rough reference truth for the deconvolution; it is not actually used for either the deconvolution or subsequent reconvolution to CrIS radiances. We tried a number of functions for this reference truth, including the 0.1 cm-1 boxcar you suggest. 0.2 cm-1 gave slightly smaller residuals than 0.1 cm-1, but these were still larger than with the generalized Gaussian. We don't go into this in the paper because we believe there is nothing surprising about getting significant imperfections from the deconvolution. The interesting thing is that most of this dissapears with reconvolution, leaving us with a translation that works well in comparison with other approaches.

- 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 Barnet 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.