Reply to the review from referee 1

Note: the original text from the review is in black and our replies are in blue.

While past studies have evaluated and interpreted the effects of CO2 on IR spectral changes from observations, none have done so from a purely observational standpoint. Instead, the past studies have relied on modeling or theoretical interpretation to separate the direct effects of CO2 change from the effects of temperature and water vapor changes that also occur at the relevant CO2 abortion bands. This manuscript represents the first successful attempt to perform that isolation solely using observations. To do so, the authors search for profiles over different years with significantly different CO2 concentrations, but with very similar T and WV profiles. They then quantify the difference between the corresponding spectral radiances (between a reference year and more recent year) to demonstrate that the expected isolated effect of CO2 to reduce OLR is evident in the AIRS observations over the tropospheric CO2 absorption band evaluated in this study. The authors also perform radiative transfer calculations solely with changes in CO2 concentration, as further support that they are truly isolating the effects of CO2 in their observational estimates. Their work will certainly be of interest to ACP Letters readers and marks an important milestone in observing the effects of CO2 on the climate. I provide some minor comments below that will hopefully help improve the manuscript.

We would like to thank the referee for a detailed and constructive review. All the comments and suggestions by the referee have been addressed and have been extremely helpful in improving the manuscript. The revised version will be an improved manuscript because of the referee's comments and suggestions.

To identify analogous profiles, the authors use RMS difference thresholds of 1.2 K for temperature and 1.2 g/kg for water vapor to identify analogues to the reference profiles and thresholds of 1.4 K and g/kg for Experiment B. Some evidence should be provided that those thresholds do indeed, represent sufficiently small radiative effects from T and WV changes. One could suspect a 1.2 K temperature change, even if just locally, could have a significant radiative response relative to the influence of CO2 (for instance, thinking in the context of a climate radiative feedback). One option is to run both the reference and analogue profiles through kCarta, with the same CO2 concentrations, and show the radiative effects from any T and WV are small compared to the direct CO2 effects

We follow the referee's suggestion and in the revised version of the paper we will have a new figure that compares the theoretically expected spectral differences due to changes in CO2 with spectral differences due to temperature and water vapor changes, for the spectral region that we are focused on. This will help quantify the spectral radiance uncertainty due to the uncertainty in temperature and water vapor from the analogues.

Line 70-73: The authors should explain why it is important to stay as close to nadir as possible. Although they explain in the appendix that doing so leads to smaller biases relative to the theoretical calculations, it would be helpful to mention why that is the case.

Text will be added to the revised version to clarify this issue.

It's not clear why the authors chose to publish both experiment A and B. Experiment A seems like a light test of the methodology before performing the more robust Experiment B. I can understand performing A while putting this study together, but its not clear why the authors have chosen to feature the results of A so prominently in the manuscript (and have given it a figure). I suspect the authors have good reason for doing so, but it does not come across clearly in the text. I worry someone who skims this Letter won't realize Figure 2 is the more robust, important figure.

We agree with the referee and in the revised version of the paper we will delete the current figure 1, and while we will still briefly mention some of the key points of the figure, we will not organize the paper in the same way. Basically, we will not divide the paper in experiment A and experiment B sections. We will focus on what we previously referred to as experiment B.

For Figure 1, experiment A, the observed radiance difference has a clear negative bias relative to theoretical for both experiments. The authors should explore the source of this bias further. They correctly mention that the bias increases towards the higher wavenumbers where H_2O is a stronger absorber. Does this suggest the 2006-2015 analogue profiles have systematically more WV than the 2005 references (albeit still within the threshold)? And that this could be leading to the systematic bias in the difference calculation? One can imagine that due to the small sample size, this could be possible.

Following the referee's previous comment, we will delete figure 1 and its specific analysis from the revised version of the paper. In the revised version, we will discuss in more detail the uncertainties related to results shown in figure 2. The new figure mentioned above will play an important role in this discussion.

Line 223-225: It is not clear why the authors are using just three CO_2 concentrations for three different years and then using a curve fit to identify the corresponding spectral radiances for years in between. Doesn't the Maona Loa data have CO_2 concentrations for all months and years within the studied timeframe? Some clarification would be helpful.

This analysis was performed for the data presented in figure 1 in the original version of the paper. Following the referee's previous comments, we will delete figure 1 and the discussion associated with it (including this part) in the revised version of the manuscript. We will mention the potential theoretical uncertainty due to CO_2 uncertainty while analysing figure 2, but we will not undertake the procedure mentioned above by the referee.

I view this work as an important proof of concept that AIRS is able to detect the influence of CO_2 on radiances in isolation. That alone, is worthy of publication. I wonder if this methodology can be applied longer-term to isolate and track trends in how CO_2 is influencing the climate (e.g. in the context of radiative forcing). Using analogues with similar T and WV would seem to be the only way to isolate CO_2 effects purely from observations, but radiative forcing itself is sensitive to the underlying climate state (e.g. Y. Huang et al. 2016). So on one hand, by trying to keep T and WV fixed, the method is not capturing the true direct effects of CO_2 on the climate. Additionally, one could imagine that, if this method is applied over a wider range of years, thus covering more climate change, it would become more difficult over time to find analogues within a reasonably small threshold and T and WV undergoes more changes. For the sake of appealing to a broader audience, I encourage to authors to add some discussion along these lines, about the broader implications of their work. Maybe in their conclusion section.

These are critical aspects, and we agree with the comments, interpretation and suggestions of the referee. In the first version of the manuscript there is already some text in the conclusions along these lines. Specifically, we wrote 'In the future, variants of this methodology could be used to isolate the observational radiative impact of different physical and chemical properties of the climate system and as such provide a better observational depiction of the Earth's radiative forcing and of climate feedbacks.' In the revised version we will expand on the topic following the referee's suggestions, in particular on how to potentially generalize this methodology. The reference provided is an important one that we will add to the revised version of the paper.

Huang, Y., Tan, X., and Xia, Y. (2016), Inhomogeneous radiative forcing of homogeneous greenhouse gases, J. Geophys. Res. Atmos., 121, 2780–2789, doi:10.1002/2015JD024569.