Reply to the review from referee 2

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

This study used AIRS high spectral infrared radiance data to quantify the impact of increasing atmospheric CO2 concentration on the absorption of 15-um CO2 band after eliminating interference from water vapor and temperature changes. They found that the isolated signal from increased absorption by CO2 from AIRS is consistent with RT model simulations. Overall, this letter was well written. The message the authors trying to deliver is clear. The results from this study based on data driven analysis are important to confirm our understanding of CO2 greenhouse effect. However, given its short length, some important details are not there in the paper and therefore lead to confusions. I will list them below.

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.

Some parameters in Experiments A and B are not the same. For example, you analyzed 2005-2015 for A but 2003 to 2012 for B. Is there a reason that the two experiments need to start from different years? Also, you explain the adoption of 1.2k and 1.2g/kg in the appendix for A, but in B, you used 1.4k and 1.4 g/kg. You need to justify these numbers.

Following the comments from referee 1, in the revised version of the paper we will not divide our study in experiments A and B. Rather we will focus on data obtained for a large number of reference profiles such as what was analysed and discussed in experiment B in the first version of the manuscript. Because of this, the current figure 1 (and its analysis) will be deleted from the revised version. So, the mismatch referred to above by the referee does not apply any longer in the analysis that will be shown in the revised version.

While there is some justification in the first version of the paper regarding the temperature and water vapor thresholds, in the revised version we will present a more detailed analysis of the data and of these thresholds. We will add a figure that compares the theoretically expected radiance differences due to CO2 changes versus radiance differences due to temperature and water vapor differences (constrained by the thresholds mentioned above) for the spectral region that we are focusing on. This will provide a sense of how large the radiance differences associated with these temperature and water vapor thresholds are expected to be, as compared to the CO2 differences.

In the experiments, you selected one set of profiles for A and 100 sets for B. It is not clear how the set of profiles were selected. How do you make sure they are representative of the temperature and h2o vertical distributions on Earth? It would be better if you can show some of the profiles in the appendix as well.

The reference profiles are selected in a random manner but subject to the following constraints: These profiles occur over the tropical/subtropical oceans (30 S to 30 N), in (almost) clear sky (cloud cover less than 10 %), during July of 2003 and with SSTs between 298 and 302 K. These profiles are expected to be representative of the thermodynamic vertical structure of the (almost) clear-sky subtropical atmosphere. This regime is characterized by low values of subsidence and by conditionally unstable boundary layers populated by small amounts of shallow cumulus clouds or even completely clear boundary layers. This is a regime that occupies a large fraction of the Earth's surface and that plays a key climate role in the surface evaporation over the ocean and the outgoing longwave radiation. Following the referee's suggestion, in the revised version of the paper we will add to the appendix a figure illustrating the vertical structure of the reference profiles and a discussion on how representative and relevant they are.

"In experiment B, a key assumption is that the annual mean spectral radiance differences corresponding to each reference state are (to first order) not sensitive to the reference state itself for these selected reference profiles." You now have 100 sets of profiles and the corresponding spectral change. Can you use these results to justify your assumptions here? For example, are the temperature (or h2o) variabilities correlated with the spectral radiance differences?

This is already partly discussed in the first version of the paper when writing that, referring to figure 2, 'The theoretical annual mean differences are calculated based on the reference states. This allows to estimate not only the theoretical annual mean difference but also the associated standard deviation, which is shown as red shading. Note that the standard deviation is so small that it is almost imperceptible in the figure. This apparent lack of theoretical sensitivity to the reference states supports the key assumption, mentioned above'. But we will make this point clearer in the revised version of the paper.

In addition, and following some of the discussion above, in the revised version of the paper we will discuss in more detail the impact of the temperature and water vapor variability on the spectral radiance differences. This will be illustrated with a new figure as mentioned above.

In Figure 2, how large is the uncertainty for the observations? You have that for Figure 1 but not Figure 2. Also, the mismatch between observations and theoretical calculations are large over those CO2 absorption line centers. The difference can be 0.04K for the lines on the left of 700 cm-1, which is larger than the expected spectra noise. You attributed this difference to CO2 uncertainty. Can you reconcile the two if you increase the CO2 in your RT model? It seems your current calculations have less absorption over those lines.

In the revised version of the paper, we will present a much more detailed discussion of the uncertainties associated with the results presented in figure 2, directly addressing the points raised by the referee. In addition, we will discuss in more detail the sensitivity of the final results to the scan angle and the filtering of potential outliers.

Minor comments:
Line 181-182, the absolute radiometric calibration accuracy is usually temperature dependence. For this 0.2K accuracy, is it relative to what temperature blackbody?

In the revised version of the paper, this issue will be clarified and more details regarding accuracy will be provided.

Line 206-207, do you have figure or reference to justify that the temperature profiles in AIRS/AMSU and AMSU MW-only are similar?

In the revised version of the paper a comparison between the AIRS/AMSU, the AMSU MW-only and the neural network (which is the first guess for the AIRS/AMSU retrieval) thermodynamic profiles will be added to the appendix.

In Longueville et al. (2021), the authors showed Figure 2 to illustrate the increased CO2 absorption in the IASI spectra from 2008 to 2017, though they did not isolate it from the joint effects of temperature and h2o. This is a related reference for this study.
De Longueville, H., Clarisse, L., Whitburn, S., Franco, B., Bauduin, S., Clerbaux, C., et al. (2021). Identification of short and long-lived atmospheric trace gases from IASI space observations. Geophysical Research Letters, 48, e2020GL091742. https://doi. org/10.1029/2020GL091742

This is an important reference in the context of this study that will be mentioned and cited in the revised version of the paper.