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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Acetylene C₂H₂ retrievals from MIPAS data and regions of enhanced upper tropospheric concentrations in August 2003" *by* R. J. Parker et al.

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Response to Anonymous Referee 2

(Responses in bold)

4. Are the scientific methods and assumptions valid and clearly outlined? No. The authors should use a global or a trajectory model to support their speculative conclusions.

We disagree that the conclusions are speculative since we did use trajectory

calculations to verify our findings, as noted in the submitted paper, but we accept that perhaps we should have included additional figures:

Page 29753 Line 23 of submitted paper.

"This was verified further through the use of trajectory modelling (not shown) which provided further confidence in both the C2H2 retrieval and in the use of the C_2H_2/CO ratio to act as an indicator for the photochemical age of air in future work."

However we did not include any plots from these calculations in the manuscript. We will now include the trajectory calculations in the revised manuscript to help strengthen the conclusions. We will also use additional literature references to show consistency with our findings (see comments to referee 1).

9. Does the abstract provide a concise and complete summary? No. It is wordy and could be significantly shortened.

We accept that we need to address the lack of conciseness.

11. Is the language fluent and precise? The text is verbose.

We accept that we need to address the lack of conciseness.

14. Are the number and quality of references appropriate? No, as indicated below.

Additional references published will be added as also noted in response to referee 1.

Main Comments

The purpose of the manuscript is poor: "In this paper, a description is provided of global retrievals of C_2H_2 performed from MIPAS infrared limb emission spectra for the upper troposphere." The manuscript does not include any evaluation of the new product with observations, such as from aircraft. While the new data may be of scientific interest, it

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is not shown how this new product is better/worse than other existing products, such as ACE. The only comment is “These data provide greater temporal and spatial resolution than the ACE instrument albeit with a much lower signal to noise (SNR) compared to the solar occultation method employed by ACE.” I recommend adding a section on the evaluation of the data and another section on a comparison of ACE and MIPAS acetylene so that the reader can judge the quality of the MIPAS data.

We disagree that the purpose of the manuscript is poor. Ideally comparisons to aircraft and ACE data would be performed, but we have retrieved MIPAS C_2H_2 data for a period when MIPAS is operating at its highest spectral resolution and the data are particularly useful. This period was August 2003 which is prior to the launch of ACE and hence no direct comparisons are possible. Unfortunately there is also a lack of relevant aircraft data for this period with which to perform any comparisons. If data became available we would be happy to perform these comparisons. We will comment on this aspect in the revised manuscript, explaining our evaluation of the data.

Regarding ACE comparisons, although direct comparisons were not possible, we have qualitatively compared our values to ACE but a one-to-one comparison is not possible and the ACE data is extremely sparse in comparison in the tropics. We will refer to these qualitative comparisons in the revised paper. Note that a comparison of ACE and MIPAS data would provide comment on the quality/consistency of both MIPAS and ACE data since validation of ACE C_2H_2 against aircraft data had also not been performed at the time of the submission of this paper. We note that Gonzalez-Abad has just published a paper on ACPD (2011) regarding ACE C_2H_2 data comparing to aircraft data from 1992, and with a more comprehensive error budget. We are not convinced of the efficacy of this comparison given the large time gap and the need to average ACE data spatially and temporally. It therefore remains the case, that ACE data have been used in the refereed literature, including publication in Science, without as comprehensive

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an evaluation as in our paper. We therefore feel that our paper has considerable merit, whilst agreeing that there will be a need to perform validation studies and report them in future papers alongside the data evaluations that we can meaningfully report in our revisions of this paper.

In this spirit, we will also note in the paper that retrieval of data from the optimised MIPAS data, post-2004, would be ideal to compare to ACE but that this data will necessarily be of lower precision and most likely higher systematic error than the current data set for August 2003.

Having said all of this, we have performed a full error analysis of our C_2H_2 at the full MIPAS resolution and believe this is satisfactory with regards to the quality of the data due to the extremely clear spectral signature. We note that many other MIPAS papers in the literature do not have as detailed an error analysis as we have presented, and we have done this deliberately in order to really give good guidance as to the quality of the data. We also believe that the comparisons to CO are very convincing and show an excellent consistency within the expected errors of the data.

The purpose of the manuscript does not include a scientific objective in the introduction. I believe that this objective is listed in the first line of Section 3: "The objective of this work was to examine the C_2H_2 spectral signatures in MIPAS L1B spectra with a full optimal estimation retrieval algorithm in order to identify regions of high C_2H_2 volume mixing rations (VMRs)." Again, this purpose is poor. Can't one use aircraft data and ACE data to identify regions of high C_2H_2? Can one only use MIPAS data for this purpose? What is unique about the MIPAS acetylene data set?

We feel that we may have not emphasised enough the far superior spatial and temporal coverage of MIPAS to ACE, particularly in the tropics, nor indeed have we noted the significant gaps in aircraft coverage. We clearly need to expand on these points in the revised paper, although actually these points are very well

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known and well covered in the literature so it is surprising that the referee is unaware of this.

As an example, the Park et al, 2008, study using ACE C_2H_2 data over the Asian monsoon included ALL data for the THREE summer months for a FOUR year period which resulted in approximately 20 measurements inside the Asian monsoon anticyclone. In contrast, with just ONE month of MIPAS data we have many times this number of measurements and are able to spatially sample not only the core of the anticyclone but also to sample transects across the edge and core of the cyclone. We feel that due to the potential of the data for globally sampling dynamic events at relevant timescales, the MIPAS data presented offers valuable information. We will amend the text to emphasise this accordingly although it was not the major point of our paper to note the poor spatial coverage of other observation systems.

The scientific significance is poor as the authors' results are primarily speculative. The authors should use a global model or a trajectory model to diagnose transport pathways and to show how the ratio of C_2H_2 to CO may be used to diagnose such transport pathways.

Please see response above. We did use a trajectory model but have not emphasised this enough in the submitted paper. We will include appropriate trajectory calculations, along with literature references, to emphasize the robustness and quality of the results.

The discussion in Section 4.3 would benefit from a literature search on the cross-tropopause transport of pollution, such as recently shown with Aura Microwave Limb Sounder (MLS) CO and HCN data. Again, the conclusions are only speculative. A model should be used.

We believe that we already have provided a sufficient reference similar to the one requested. In Section 4.4 we refer to Randel et al, 2010 for reference to

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"transport of air masses from the surface deep into the stratosphere through the use of HCN retrieved from ACE". However we will update the manuscript to reflect more articles that have been published on this subject.

With regards to using a model, as stated above we performed trajectory calculations to help verify our results and we will report these also.

The discussion in Section 4.4 would benefit from a literature search on the Asian monsoon.

Again, we believe we have included the most definitive references on the Asian monsoon with Park(2007), Park(2008) and Randel(2010). Again, we will update with other recent publications.

The summary of conclusions in the last paragraph is not supported without an evaluation of the data.

The data currently has been evaluated through a thorough error analysis as well as limited qualitative comparisons to ACE. Hence we do not agree with the referee comment that our summary is not supported by our assessment of the data and note that the comparisons with CO give very good grounds for geophysical consistency in addition to our detailed data assessment. We also note that ACE C_2H_2 data, with a poorer error characterisation and no significant aircraft comparisons, has been extensively used in the literature.

We agree that further validation of the data product is important but is most likely to be performed for MIPAS data sets produced from lower spectral resolution data (post-2004) with different error characteristics to the superior data described here. This will be completed once the ongoing work at the MIPAS "optimised resolution" is complete.

In summary, we do not feel that this significantly weakens our conclusions since we think we have a good evaluation of the errors of our data set given that only

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MIPAS data are available for this period, and since the data show very good correspondence with simpler detection methods and with the CO fields. It is clear that sometimes one instrument provides the only observations available for some atmospheric trace species. We will comment on these facts in our revised manuscript.

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Interactive Comment