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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "MLS measurements of stratospheric hydrogen cyanide during the 2015–16 El Niño event" *by* Hugh C. Pumphrey et al.

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1 Reply to general comments

The reviewer notes that the manuscript is very short and lacks details. This is a matter of opinion and taste. The paper, if it were published as it is, would run to 13 journal pages; this does not seem short to us. Furthermore, we have attempted to show the MLS data with as much other information as the reader needs to interpret it, but no more. We have deliberately kept the material on ENSO to the minimum necessary; there is a huge amount of literature on the subject already.

The reviewer states that the zonal mean plots in figure 3 provide poor information be-

cause they do not show at which longitude the HCN is entering the stratosphere. It is for exactly this reason that we included figure 6, which makes it clear at which longitude the excess HCN first appears in the MLS data. Plots along the lines of figure 3 in the paper look much the same if you make them for a restricted range of longitudes; we show an example in figure 1 of this reply. Note that the two panels of this figure look almost identical except at 100 hPa and 68 hPa; here, the 95°E to 155°E panel shows clearly the sudden arrival of HCN in late autumn of 2015 (and to a lesser extent in 2006 and 2014). In the paper we make this point using figure 6; we did not feel that it was necessary to also include time-height plots for specific longitude ranges. It is unsurprising that tracer mixing ratios in the tropical stratosphere are zonally symmetric except for short periods immediately after large amounts of a tracer cross the tropopause at a specific longitude. The zonal winds in this region are far faster than the meridional winds or the vertical transport, ensuring that any difference between one longitude and another is smoothed out on a timescale of a month or two.

The reviewer makes a valid point in that it might seem surprising that air is entering the stratosphere over the maritime continent, at a time when the Walker circulation has moved so that air is, in general, descending in that region. Nevertheless, the maps in figure 6 of the paper make it clear that the excess HCN appears in the equatorial lower stratosphere at a very restricted range of longitudes around 90°E or 100°E, and does so in a very short period of time.

There is essentially no doubt that the source of the HCN is fires in Indonesia. A trawl through newspaper articles from the time makes it clear that the fires were a serious event, causing widespread regional pollution in the lower troposphere (See https://www.theguardian.com/world/2015/oct/26/indonesias-fires-crime-against-humanity-hundreds-of-thousands-suffer for a typical article.) A second article (https://www.theguardian.com/environment/ng-interactive/2015/dec/01/indonesia-forest-fires-how-the-years-worst-environmental-disaster-unfolded-interactive) contains some useful animations of the fire season, demonstrating that it ran from early July through to late November, but that the largest emission of CO to the free troposphere occurred in mid to late October. For more detail on the CO, in a refereed publication, see Field et al. (2016), which makes it clear that biomass-burning products were present throughout the troposphere over Indonesia in the latter half of 2015, and that they reached the tropopause in late October. As we explain in the paper, because the burnt material in Indonesia tends to contain large amounts of peat, the ratio of HCN to CO is likely to be larger than in biomass-burning events elsewhere in the world.

In an attempt to assess whether it is reasonable to expect transport from the ground to the stratosphere at the time we observe the excess HCN, we plot in figure 2 of this reply the vertical velocities averaged over the region in which we observed the HCN entering the stratosphere. The data are taken from the NCEP Operational Model Global Tropospheric Analyses (DOI: 10.5065/D6M043C6) and are smoothed with a 15-day binomial smoother. The vertical dotted lines mark 2015-10-24 and 2015-10-31, corresponding to the upper right-hand panel of figure 6 in the discussion paper. It is clear that during June-October 2015 the vertical velocity was mostly downwards, whereas the average over the 2010-2016 period was upwards. However, in all years, the air begins to ascend strongly towards the end of October, at the end of the dry season and the start of the wet season. In 2015 this ascent begins later than average and is not as strong as on average, but it is ascent nevertheless. Furthermore, the change to an overall average ascent (which may have been preceded by more localised ascent episodes) begins at the time that we see the HCN appearing at the tropopause. The period corresponding to the top-right panel of figure 6 in the discussion paper is marked on figure 2 of this reply with two dotted vertical lines. We therefore do not accept the referee's argument that upwards transport is not possible in this region at this time. The actual transport probably occurs in individual convection events, possibly initiated by the fires themselves, but figure 2 of this reply suggests that the average vertical velocity in the region is not such that it would suppress the convection.

2 Reply to specific comments

- *Authors missed adding information of the Walker circulation in the introduction section.* We will add a sentence or two, but we do not consider it appropriate to add a detailed description.

- *[A] few details on vertical transport into the upper troposphere and lower stratosphere by the convection would be useful.* It is far from straightforward to provide "a few details" on this in a useful way. In 1981, Newell and Gould-Stewart (1981) suggested that air must enter the stratosphere over the maritime continent in November-March and over the Bay of Bengal during the summer monsoon period. That paper has been cited over 250 times in the intervening years, not always by people who think it is correct (Dessler, 1998). The exact details of how air enters the stratosphere from the troposphere and the extent to which convection plays a role in the process is still an area of ongoing research. A good recent paper on rapid transport paths from the surface in the tropics to the upper troposphere and stratosphere is Hosking et al. (2012) — we will cite this, and add a sentence or two on the subject to the final version.

- *The abstract does not provide complete information about the manuscript.* Indeed, that is the point of an abstract. It is clear that the reviewer feels that there is a part of the paper which is not adequately represented in the abstract, but he does not state which part that is. We therefore intend to leave the abstract unchanged.

- *Section 2.2: Surface measurements at the Jungfraujoch in Switzerland (46.5°N), Mauna Loa in Hawaii (19.3°N), and Kitt Peak, Arizona (32°N), show higher than normal values during El-Nino. This can be justified by transport from Indonesia. However, vertical transport to the stratosphere from Indonesia may not occur during El Nino due to subsidence over this region.* In stating that the Jungfraujoch data are less easy to explain, we meant that Rinsland et al. (2000) were of that opinion; this is why they present several different explanations. We will re-word that sentence to make it clearer that this is the case. We have already shown why we do not agree with the reviewer's opinion that HCN is unlikely to be transported into the stratosphere over Indonesia during an El-Niño year.

- *Page 9, line no 8-13. During the normal years (Not El Nino years) MLS show transport from Asian monsoon region if you take an average over the region 20-40N, 60-120E). This is related to vertical transport by monsoon convection as stated by Randel et al. 2010, Ploeger et al., 2017). While during an El Nino year, HCN may not get transported to the stratosphere from this region due to subsidence over this region (see [this paper in climate dynamics: http://dx.doi.org/10.1007/s00382-016-3451-6]).* It may indeed be possible to see the monsoon-related transport as described by Randel et al. (2010) in the MLS HCN data, but we feel that to extend the current paper to examine that subject would make it too long and too unfocused. As noted above, there is, in fact, ascent and not subsidence at the time we observe HCN transported rapidly into the stratosphere.

- *Section 3.4 is very poor. I suggest not drawing conclusions from zonal or meridional average plots. Authors should select one specific region and provide details (e.g., Maritime Continent).* We are puzzled by this comment. Section 3.4 is the one place in the paper where we show maps, so that the geographical location of a phenomenon can be seen clearly, and where we are not drawing a conclusion from a zonal mean. For most of the time, HCN in the tropical stratosphere is close to zonally symmetric (as in the top left panel of figure 6) and a zonal mean tells you almost everything there is to know about it. The remaining panels of figure 6 show a period which is very unusual in that the HCN distribution is not zonally symmetrical, with much higher values appearing rather suddenly over the maritime continent. Once the excess HCN has arrived in the stratosphere, the

rapid zonal winds disperse it to all longitudes on a timescale of a month or two.

- *I suggest providing (maybe from Reanalysis data) a figure of anomalies (year El Niño-Climatology) of vertical winds and circulations over the selected region. It will be helpful in depicting vertical transport into the lower stratosphere.*

  We show vertical velocities in figure 2 of this reply. We will amend the text of the paper to mention that, although there is descent over the source region from June to October it changes rapidly to ascent at the end of October. We do not feel that it is necessary to include figure 2 of this reply in the final version of the paper.

3 Reply to technical corrections

- *Page 1 line no 2: greater than other El-Niño events or Climatology?* We meant that, in the major El-Niño years, HCN is both higher than the climatological mean and higher than it is in a more moderate El-Niño year such as 2006-7 or 2009-10. Based on the data that we have for major El-Niño years, HCN in such years is higher than at any other time. We will attempt to re-word that sentence so that any ambiguity is reduced.

- *It is difficult to read figure 4 due odd y-axis scale.* The y axis scale has a tick every 20 degrees of latitude, including zero; I am not sure what I should do to improve that. I have used a tick every 50 degrees in figure 5 as the panels are smaller, but I think 20 degrees is better for figure 4. I suspect that the reviewer's problem is the way that R has chosen to put labels on the ticks at -80, -40, 0, +20 and +60, where -80, -40, 0, 40, 80 would seem more symmetrical. We have re-built the figure, setting the labels by hand to be at -80, -40, 0, 40, 80.

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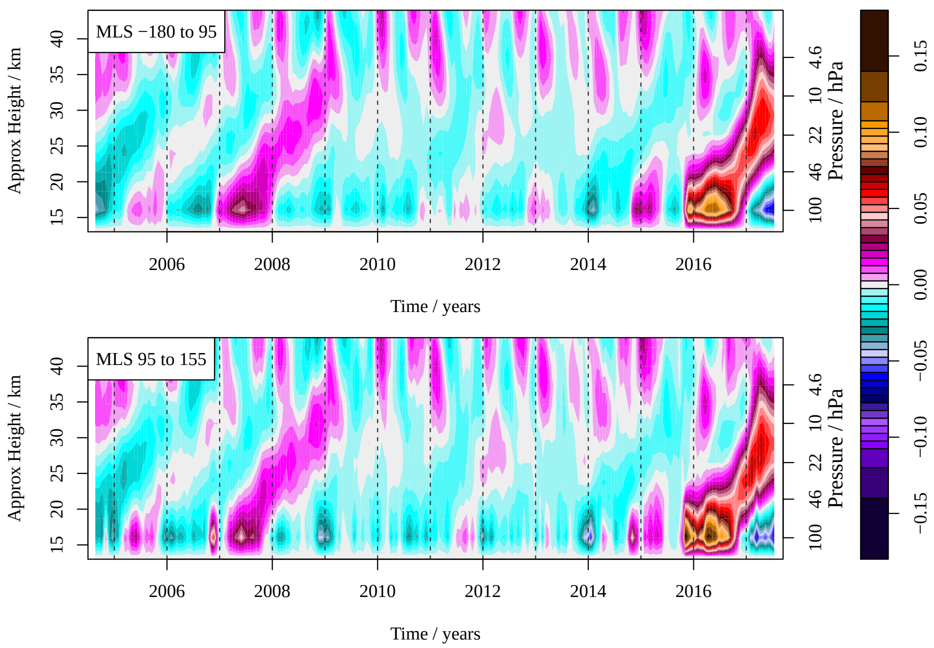

Fig. 1. MLS HCN anomaly (with respect to the 2005-2012 mean) as a function of time and altitude for two different longitude ranges: 95E to 155E and 180W to 95E.

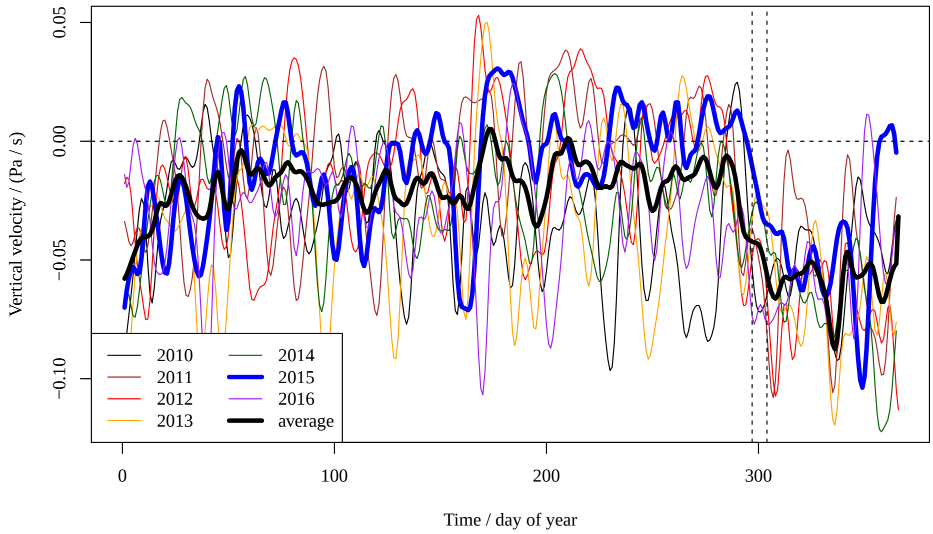

Fig. 2. Vertical velocity at 500 hPa averaged over a region between 90E and 120E, and between 6S and 6N. Note that, as the data are in units of Pa/s, negative values mean ascent, positive values descent.