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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Secular change in atmospheric Ar/N_2 and its implications for ocean heat uptake and Brewer-Dobson circulation" *by* Shigeyuki Ishidoya et al.

Anonymous Referee #1

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This paper presents observations of surface Ar/N2 ratios from four sites (3 in Japan and 1 in Antarctica). A long-term trend is diagnosed from the two sites with the longest records (2012-2020) and this is converted to a trend in global ocean heat content. A 2-D atmospheric model is used to estimate the trend in surface Ar/N2 that might be caused by an assumed trend in the stratospheric Brewer-Dobson circulation, which amounts to 20% of the overall Ar/N2 trend. The corrected surface trend (and the uncorrected one) are consistent (within the large error bars) with the OHC trend calculated from temperature data.

I think that the paper presents interesting observations and is attempting a novel anal-

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Discussion paper

ysis. Investigating how surface Ar/N2 can be used to quatify ocean heat uptake is an important topic and investigating how stratospheric variations can influence this is timely. However, I find that some aspects of the methods are crude and/or not fully explained. I also think that caveats in the analysis need to be stated. Therefore, I think that the paper requires major changes before becoming possibly suitable for publication in ACP. My comments are below.

Main Points

1) Surface observations. TKB has the longest and most dense data record. The observed temporal variations (Figure 2) give a compact annual cycle. In contrast, the sparser observations at the other 3 sites show much more variability (large amplitude variations). What are the reasons for this? Is it a measurement issue or possible real atmospheric variations?

2) Observed long-term trend. Even the two longest data records (8 years) are short for deriving accurate long-term trends. The trend fit is not explained clearly. The model leaves variations which are > 36 months but the trend is quoted as if it is a linear term? The fits to Ar/N2 and OHC in figure 2 are far from linear (but do vary with the long-term variability in the temperature data). I am concerned that the paper is reporting values in the abstract which imply a long-term linear trend, which is (quantitatively) not obvious from the plots.

3) Atmospheric modelling. An updated 2-D model has been used to model surface Ar/N2. There is no evaluation of the model in the stratosphere using available profiles of Ar/N2 to show that the higher altitude gravitational separation is modelled realistically.

4) Imposed trend in the BDC. The stratospheric BDC is complicated with deep and shallow branches. A trend in the circulation is imposed in the model and the resulting trend at 35 km is shown. First, more information on the impact at other altitudes should be shown (e.g. latitude-height cross section of the impact on age-of-air). Second and more importantly, the use of the Engel et al paper to support a trend in AoA, which is

converted to a correction of surface Ar/N2 is unjustified. Engel et al use a series of sparse balloon observations of CO2 and SF6 to derive an AoA trend (from 24-35km) up to 2005 – so there is no overlap with the observation period in this paper. Moreover, the error bar on their trend is very large and the title of their paper gives the headline message of 'AoA unchanged within uncertainties'. Therefore, I cannot see how the imposed trend of 0.02 yrs/yr can be justified as the best estimate which gives the correction used in the abstract.

5) Ocean model. This is a crude approach (as acknowledged by the authors) and it leads to statements in lines 187-193 that the model maybe too simplistic (i.e. not suitable) and that other factors may need to be considered. Overall, this part of the analysis seems incomplete therefore.

Other Specific Points

Line 21. The uncorrected trend of 0.75 +/- 0.30 per meg yr-1 is also consistent with trend derived from ocean temperature, at the limits of the error bars. The correction is not needed, which is what is implied by the text.

Line 132. 'Fig 2 in 3-1'. What does this mean?

Line 163. 'Increase rates' (also in caption of Figure 4 and label axis). This should be referred to simply as 'rate of change'. The positive values will imply an increase.

Line 180 'boldly'. This is the wrong word. You must mean something like 'crudely'.

Line 183. 'modern' – better to say 'present-day'.

Line 184. Insert 'total atmospheric mass. . ..'.

Line 187 'drives'. Delete s

Line 216. 'molecular mass number' -> 'relative molecular mass'

Line 221-223. Add a reference or make it clear that you are referring to this work.

Line 238. ‘decrease’ (no s)

Line 240 (and later). I think ‘idealised’ is better than ‘virtual’.

Line 250. Don’t need to say ‘increase’.

Line 251. Give the dates that the Engel et al study covered (but see main comment above).

Line 255. The word ‘obtained’ is wrong. The perturbation to the model circulation was forced arbitrarily. Change to e.g. ‘forced’?

Line 258. Nb ‘simplistic’ is a negative term which means that the model is too simple to be suitable.

Lines 265. Change ‘increases’ to ‘is estimated to increase’.

Line 267. Change ‘expected’ to ‘is estimated’.

Line 269. Insert ‘are non-negligible trends compared. . .’

Line 274 Insert ‘. . . estimated effects..’

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