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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 #2

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Ishidoya et al. present an analysis of atmospheric variations of the Ar:N2 ratio. Changes in oceanic heat content (OHC) drive variations in atmospheric Ar:N2 due to changes in solubility. Measurements of Ar:N2 (reported in delta-notation relative to a standard) thus bear promise to infer OHC changes without measuring oceanic temperatures. Given the paramount role of OHC in the context of global warming, complimentarty methods that provide independent estimates of OHC changes such as the one discussed in this paper are extremely important.

The study by Ishidoya et al. argues that the Ar:N2 ratio measured near the surface not only reflects OHC changes, but also changes in the strength of the stratospheric

Brewer-Dobson circulation. The latter is a consequence of non-negligible gravitational separation according to earlier work by the same group. In a nutshell: The atmosphere is depleted of Ar with increasing height. A strengthening of the BD circulation decreases the separation, leading to an Ar:N2 ratio increase aloft, and decrease near the surface.

The manuscript is challenging to review as it touches on a wide range of topics but information provided is often (too) sparse. As far as I can see, the main points of the manuscript are:
(i) Ar:N2 measurements are accurate enough to detect trends, which was not possible previously (Line 37).
(ii) Variations in the ratio over a decade are well correlated with OHC variations as reported by NOAA/NCEI.
(iii) There is a quantitative mis-match if only OHC is considered.
(iv) A model simulation with imposed change in the stratospheric BD circulation suggests that the near surface Ar:N2 ratio is as sensitivite to the BD circulation as to OHC variations (for the observed magnitude of changes in either of these).
(v) If one assumes that the OHC measurements and their impact on Ar:N2 are correct, one infers that over the period of measurements (2012-2019) the BD circulation slowed down.

The authors discuss the last point in some detail, and seem to be concerned that the sign of the change in the BD circulation is the opposite of a supposedly long term strengthening of the BD circulation. There is no reason, however, why a short period such as the one studied here should show the long term trend given the large interannual variability of the stratospheric circulation. I recommend to shorten this somewhat misleading discussion (the authors are most likely not observing "trends"), and instead strengthen the points (i)-(iv). Specifically:
(a) This reviewer is not sufficiently familiar with the measurment technique to be able to assess claim (i); the data as presented in Figures 2,3 and 4 does look, however, rea-

sonable; as does the calibration (Figure 1). Nonetheless, I hope that another reviewer may be in a position to comment.

(b) The time filtering and frequency separation method needs to be better explained - based on the information provided in this manuscript, it is not possible to reconstruct what precisely the authors have done. This is an extremely important part of the argument as the claim is that seasonal variations are due to local SST variations, whereas the longer-term changes are due to global OHC changes (i.e. the seasonal cycle at TKB and HAT differ (Fig 3), but their longer-term variations (Fig 4) are highly correlated). Also, the analysis then uses largely only TKB and HAT - but why not also show COI in Figure 4? Please use 2 different colors for AHT and TKB in Figure 4a - the separation with "+" and "o" does not allow to see the differences (Looking at Figure 2; it is surprising that these two timeseries align as well as shown in Figure 4).

(c) The SOCRATES model is not sufficiently described. Please provide more information on vertical and horizontal resolution, time steps, and parameterization of atmospheric transport and mixing. Please show the model's mean vertical profile of the Ar:N2 ratio, along with the measurements thereof in the stratosphere. Do you have other information (e.g. CO2 mixing ratio profile) that could serve as validation?

(d) Generally, a better description of the system studied, and implicit assumptions, would be helpful. (Also - equations 3 and 4 can be valid only for specific conditions - please provide more details).

Additional points:

- Figure 5: My understanding is that this is simply an idealized calculation for 10 years; if so, please change x-label (labels 2000 - 2010 suggest that this is specifically for this period; which is confusing also because OHC changes are taken from this period, see next item).
- OHC: Why do you discuss OHC change for 2000-2010 (Line 267)? Why are you not using the data for 2012-2019? The text here is confusing; Figure 4b suggests you use the data for the correct period.
- Please indicate the baseline period for OHC (i.e. the figures shows departures from

a baseline, not absolute OHC as one would think based on labels and caption).

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