

Interactive comment on “Variability and trends in stratospheric NO₂ in Antarctic summer, and implications for the Brewer-Dobson circulation” by P. A. Cook and H. K. Roscoe

Anonymous Referee #1

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The main purpose of this paper is to study the variability and trends in stratospheric reactive nitrogen with the aim to diagnose changes in the Brewer-Dobson circulation. The authors use mid-summer NO₂ vertical columns obtained from zenith-sky measurements made at two Antarctic stations (Faraday 1990-95, Rothera 1996-2007) to study the trends in NO₂ and NO_y. They conclude that their technique, i.e. the atmospheric photochemical box model used together with their RT model and analysis routine are a useful method to analyse NO₂ slant columns. They find that the NO₂ and NO_y columns have a large inter-annual variability with a broad maximum around 2000. The authors show that these changes are robust to a variety of alternative settings and conclude that this indicates a possible similar change in speed of the Brewer-Dobson

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circulation with opposite sign (broad minimum around 2000).

General comments:

I am a bit torn about this paper for the following reasons: The authors provide a very thorough and detailed description of the method used to interpret their NO₂ slant column measurements. This is certainly very informative but at the same time highly technical. The actual scientific interpretation and discussion - as promised in the title - only happens at the very end. However, the authors also write that there will be a follow-up paper presenting a "quantitative interpretation of their NO₂ and NO_y trends in terms of changes to the Brewer-Dobson circulation". My suggestion would be to either tighten the technical part and extend the discussion a) to include a proper trend analysis and not just a straight line fit, b) to then also include whatever else is planned for the "more quantitative version"; and c) reflect this appropriately in the title OR to stick with the paper pretty much as is but submit it to a more technically orientated journal such as AMT (Atmospheric Measurement Techniques) and then submit the follow-up paper to ACP.

IF the paper should stay with ACP, then I would strongly suggest that the authors provide a trend analysis (multi-linear regression) which apart from the linear trend includes e.g. QBO, ENSO, solar cycle and possibly a volcanic term. This would be much more convincing given the aim of the paper (implications for the Brewer-Dobson circulation) and the high inter-annual variability clearly displayed by the data set. Also, if the paper stays in ACP, I really would expect to see more emphasis on the discussion of the trends and implications for the Brewer-Dobson circulation.

Specific comments and suggestions:

1) Could you please provide a couple of lines (basic background) about the instrument that was used for the measurements and also the RT model.

2) Some of the figures are quite hard to read and I had to look at them on the screen

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quite strongly enlarged; would be helpful if they could be edited for easier reading (e.g. at least enlarge axis title).

3) Figures 1a+c should have a second y-axis (e.g. on the right side of the figure) with the actual SZA values rather than using one axis with SZA/15.

4) Page 839, lines 6-9: "... demonstrating that further processes are involved". These "further processes" have been discussed in McLinden et al., and that should be mentioned here; please add the ref: McLinden, C.A., S.C. Olsen, M.J. Prather, and J.B. Liley, Understanding trends in stratospheric NOy and NO2. J. Geophys. Res. 106(D21): 27787-27793, 2001.

5) Page 840, line 11: should read something like that: "..., but the AMF also depends on the wavelength";

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 837, 2009.

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