

Interactive comment on “Evolution of the eastward shift in the quasi-stationary minimum of the Antarctic total ozone column” by Asen Grytsai et al.

Anonymous Referee #2

Received and published: 9 September 2016

Review of the manuscript "Evolution of the eastward shift in the quasi-stationary minimum of the Antarctic total ozone column" by

Grytai, Milinevsky, Kiekociuk and Evtushevsky

In the publication, the authors describe the observation of an eastward shift of the minimum of the Antarctic total ozone column. After stating the effect, the authors proceed to search for correlations and relationships with several other changes in meteorological fields.

The authors start with a literature review and describe briefly the methods they used. It is followed by detailed description of a lot of phenomena and a Conclusion and sum-

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mary.

General

I have been often left wondering, what the new work in the publication is, where existing work is just continued and, most important, how relevant the work is. Mostly the authors describe phenomena and correlations. The authors frequently jump from finding a correlation of two features to the conclusion, that the connection is shown. Examples will be named below. I also found contradictory statements, see also below.

Although the authors used models (ERA INTERIM and NCEP) they did not investigate, if the phenomena they describe are reproduced by the models. At least ERA INTERIM provides ozone also. Although they are assimilation models, it would be crucial to investigate if they reproduce the correlation described here also. The authors also failed to explain, why two models have been used. I would expect this part of the Section 2.

I understand that this is not a modelling paper and am not asking the authors to evaluate models. However, easily accessible data should have been used to help the reader to classify the paper. The fact, if models produce the effects described in the publication would change the message considerably.

The structure of the publication is very confusing. The authors mix data analysis, description of the results and conclusion in almost every section, except section 1. Some details are also provided below.

The conclusion is rather lengthy (3 pages as compared to 6 pages in the results section) and set off after stating the main results of the work followed by further discussion of effects. This is not standard and should be changed.

I understand and accept from the analysis, that common tendencies exist. However, I wonder, if this insight is sufficient to populate a paper. Hardly ever an explanation is put forward where this correlation may come from.

Taken everything into account, I cannot recommend the manuscript for publication.

Detailed Review

Section 2: Data and methods

A discussion of the results should be left to the respective section (around line 95).

How is the ozone whole defined? By eye sight?

I have to admit: I find the equations more confusing than clarifying, especially the second one: It says: "spectral Fourier harmonics" were evaluated. But of which quantity. I suppose TOC? And how is $f(\lambda)$ defined?

I would recommend to provide data and the software in the supplement. I doubt, that it would be possible to repeat this study using the information given.

In the whole publication I did not find, how an 'anomaly composite' is defined. I can sort of guess it, but it should be explicitly stated.

I also did not find, why sometimes NCEP and sometimes the ERA_INTERIM assimilation model is used. I would expect an argument for this also in the end Section 2, not merely stating this.

Section 3: Results Section 3.1.

Why did the authors not show the comparison for polynomials $k=2$ to $k=6$? The reader is left wondering, what exactly 'similar' means. I would suggest to include such things to the supplement, if they are not changing the message of the publication.

The authors speculate about the influence of several quantities on the TC of Ozone. Sometimes they have been observed by others, sometimes the coincidence seems obvious. However, the publication is rather descriptive. This is surely valuable in its own right. However, similar things have been described several times, as the authors long reference list shows, but often it is not clear which phenomena has been described

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in which publication. An example:

The authors frequently hint vaguely at some possible connection followed by a list of publications in lines 189ff. There are several statements in one sentence:

1. Significant decadal changes in the SH polar ozone are coupled with the stratospheric thermal regime
2. they may impact planetary wave propagation
3. and regional climate change in both the troposphere and the stratosphere

followed by four citations. But which publication dealt with which statement? I would expect some more guidance from a publication, than just getting a list of suggestions of what to read. So, please sort, who stated what.

Section 3.2.

The whole subsection is a listing of observed correlations, but no ordering of how important the authors think the correlation is and often without a statement of what the authors think the stated effect means.

On page 9 line 301: The authors state that: 'Close connection ... is confirmed by Fig. 8. But figure 8 shows regression coefficients. Again: correlation does not constitute causation and not even, that there is any connection.

In the end of section 3.2 (line 373 ff) I read the sentence:

'Note also that, because Figs. 6–11 present the relationships on a seasonal time scale, the statistically significant results seem to reveal new features of the troposphere–stratosphere interaction in Antarctic spring (September–November).'

Here the authors state some finding, but I don't understand what exactly they mean in the figure 6-11. Surely not all of them, because effects are sometimes cited (here in this chapter or later in the Section 4): for example: line 398-403 in the Section 4.

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Discussion and Conclusion:

I think that the sentence "Our work provides further evidence that asymmetries in the distribution of Antarctic spring ozone exhibit trends and variability that relate to both tropospheric processes and the action of ozone." is not justified, because it implies that the authors describe some mechanisms of how they are related, which they did clearly not.

The authors should untangle the section 4: Discussion and Conclusion. It is very confusing to follow the authors jumps from conclusions (e.g. lines 380 till 400) to discussion i.e. line 410, which even contradicts some of the conclusions. I.e. I would understand the sentence in line 408:

'A large part of the QSW min longitude variance can be explained by the SAM-index variance (35%, Fig. 8b).'

that the SAM index variance causes the QSW min longitude variance. But in line 410 is said: 'Our results do not give information on the direction of the 'QSW min –SAM' coupling:...'

Result 3): No relationship has been demonstrated, but it has been shown, that the development is parallel even on a decadal time scale.

In line 438 ff a possible interpretation of the results is put forward. But this should clearly be discussed before stating the main results in lines 384 ff.

In the conclusions line 474 a fully new aspect comes into play: the ozone recovery. It has not been discussed before only briefly mentioned. While the statement that the recovery takes longer is backed by citation, the next sentence remains very unclear and speculative:

'... the possible influence ... the eastward shift could be renewed.'

But what does this mean in the context of the paper?

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