

Dear Reviewer,

Thank you very much for your positive evaluation of our paper. We took your comments into account in the revised version of the manuscript. Please find below our detailed replies (black font) on your comments (blue font).

Reviewer#3 comments:

General comments

The manuscript is clearly and carefully written and I have very few minor comments at all, but do have some overall comments to make.

The current work is clearly, and is stated to be, a follow-up to previous LOTUS work, but is also a direct descendant of the preceding work in Harris et al. 2015 and Steinbrecht et al. 2017 (and these papers should be cited), and I would say goes back to Harris et al. 1998.

The authors therefore have apparently made a decision to reduce the scope to that of an update. This is quite understandable but in one or two places I think some things have been missed that needed to have been included (if only briefly). In my view the paper does need to be able to stand alone to some extent. It certainly doesn't need to repeat everything from previous work but the fundamental points should be covered.

In the revised version, we added these references and briefly summarized the results of these studies.

The first example is the lack of discussion of the decision made by the LOTUS team not to include one or more proxies to represent the strength of the Brewer-Dobson circulation in the regression. This is an idiosyncratic choice because such a term is usually considered very important if the overall goal is to assess how ozone has changed due to chemical changes. If the goal is just to see how ozone has changed then you don't need any proxies at all of course. The QBO, ENSO and Solar Cycle terms will approximately even out over a long time period (although I note they are not shown in the current work) in contrast to circulation changes which potentially have a major influence on trends. To be clear I am not requesting that the analysis be re-performed with dynamical proxies but I do think there needs to be some discussion of this fundamental point of why they were not included, as it is closely tied to the purpose of the trend analysis, that is, what exactly are you trying to show?

We added the following clarification of the goal of this study in Sect3.1 of the revised version:

“Various approaches to trend analyses are discussed in detail in P19. The analysis presented in this paper does not include attribution of ozone trends. This study is aimed at examining changes in ozone trends (a combined effect of chemistry and dynamics), derived from observations and chemistry-climate models, between two time periods, 2000–2020 and 2000–2024, and therefore we choose similar proxies and regression model settings to those used in WMO-2022.”

However, there are alternative approaches to ozone trend analysis where regression can be performed using EESC (Equivalent Effective Stratospheric Chlorine) and dynamical proxies. Inclusion of these proxies instead of a linear trend can help to attribute ozone changes to the evolution of ozone-depleting substances (ODS) and to changes in the Brewer–Dobson circulation (BDC). However, several studies have revealed some limitations of this approach. A single EESC curve might not be representative for describing ozone changes related to ODS at all latitudes (e.g., Kuttippurath et al., 2015; Weber et al.,

2018). Dynamical proxies are usually estimated using data from models/reanalyses, which have uncertainties of their own and might differ across models (e.g., Rao et al., 2019; Šácha et al., 2024), especially in their trends. To minimize uncertainties in trends of dynamical proxies, they can be de-trended and used for characterization of dynamical variability (e.g., Li et al., 2023) and thus improving the fit. Recently, Petropavlovskikh et al. (2025) studied the influence of dynamical proxies on stratospheric ozone trends and found that inclusion of them has very small influence on trends from zonally averaged records in the middle and upper stratosphere but generally reduces trends uncertainties.

The LOTUS report (Sect4.1 of P19) discusses various trend and non-trend proxies, as well as methods for trend analysis (multiple linear regression and dynamical linear modelling (Laine et al., 2014), therefore we simply refer to the P19 discussion in Sect. 3.1 of our paper. However, we mention in our paper the importance of considering dynamical proxies, especially for trend analyses at high latitudes, and propose dedicated analyses in the future (Sect. 4.3). Additionally, we added in Sect.4.1: “Inclusion of de-trended dynamical proxies (e.g., Weber et al., 2022; Li et al., 2023; Petropavlovskikh et al., 2025) might reduce the sensitivity of trend estimates to the selection of the endpoint year. This can be investigated in future works” .

Previously (e.g., the supplement for Godin-Beekmann et al. 2022) the pre-1997 trends were shown in the latitude-height plane which is an important indication of the usefulness of the whole approach.

Since pre-1997 data, the regression model and pre-1997 trends are the same as reported by GB22, they are not shown in this paper. We added this note to the revised version.

Secondly there doesn't seem to be any indication given of how well the regression model performs. This is particularly pertinent with the recent unusual ozone years in both the northern and southern mid-latitudes. I would expect the QBO and ENSO proxies would have very different effects at different latitudes and altitudes but this is not represented anywhere.

One thing very helpful about the Weber et al. 2022 style of presentation for total ozone is that the reader can see at a glance how well the regression model has performed over the whole period and can visually assess the significance of the post 2000 trends in the different latitude ranges. This is a lot simpler to do for total ozone of course, but you could do it for a small number of selected height and latitude ranges. This would be very interesting for the NH lower stratosphere, for instance.

The performance of the LOTUS regression model is discussed in detail in P19 and also in GB22, so we believe it is not needed to repeat the full assessment of contribution of proxies in our paper.

The quality of the regression fit is usually characterized by fit residuals. Since trends uncertainty estimates are derived using fit residuals, comparison of trend uncertainties when using different time periods indicates also changes in the fit quality. The Supplement Figure S7 (Figure S6 in the original version) shows different components of uncertainty estimates. Dashed lines show the propagation of errors from fit residuals. Overall, they are smaller for 2000-2024 than for 2000-2020, as expected (details are presented in the manuscript).

Thirdly, table 1 lists no less than ten different merged satellite datasets (only eight are shown in figures 1 and 2) but I can't find any discussion of what the scientific value really is in having different groups

create these merged datasets from different combinations of the same set of instruments. You should give the reader some brief indication of why this is worth doing.

In the revised version, we added:

“While some merged datasets include similar sets of satellite observations (for example, SBUV MOD and COH are created from the SBUV data, SWOOSH and GOZCARDS primarily use SAGE II and MLS data, SAGE II data are used in six merged datasets), they still differ in the methods used to merge the original measurements as well as the exact selection of original measurements (time period, version, etc.). Comparing trend results across different merged datasets enhances confidence in the derived trend estimates.”

In lines 233-235 it is stated that two of these datasets give noticeably larger trends in the northern hemisphere upper stratosphere than the others, and in lines 275-277 you also say there are some differences in the southern hemisphere middle stratosphere. It would be important to investigate these further but I can understand that you might consider it to be out of scope for the present work.

We fully agree, these features should be investigated further. However, these studies are out of scope of the present work.

The discussion of the longitudinally resolved trends (in particular over the Antarctic) and the comparisons of ground-based instruments are also treated fairly cursorily, and I hope will be further investigated in more detail in other work. There is a statement (line 363) that discrepancies between the instruments at Lauder is greater than at the other two ground-based locations, but there has been some recent work specifically done on this topic motivated by SG 2022 which you don't consider or cite (Björklund et al. 2024 , Zhang et al. 2024).

Thank you very much for your suggestions and references. We include them and their conclusions in the revised version of the manuscript (see also below).

I think the set-up of the trends with the flat "gap" period in the regression model is very appropriate, and should lead to realistic fittings of the proxy terms (that is, it will give better results than trying to fit ozone to a V-shape).

Figures 3b and 3c are excellent, for many readers these will really be the key findings, especially that the southern hemisphere stratospheric ozone column shows a positive trend all the way from 20° S to 60° S.

Including the previous results in Figure 5 is very helpful for those interested in how the new results differ from the old ones, such as for the Ozone Assessment, along with the discussion in lines 239 to 260 and the supplement.

I was also very pleased to see Figure 7 with the representation of latitude dependent changes.

Thank you very much.

Specific comments

Line 41 "good agreement". I am not sure this is a justifiable statement but perhaps it is. There are some important differences between the model trends and the observations and the uncertainty ranges for both observations and models are still frustratingly large.

We removed “good”.

Line 84 "new datasets have become available" – explain what this means (new instruments, newly reprocessed versions of the data, new combinations of data, etc)?

Here, in introduction, we added “(the details are provided in Sect.2)”.

Lines 1-89 Somewhere in here you should mention the previous work on vertically resolved trends (at a minimum: Harris et al. 1998, Harris et al. 2015 , Steinbrecht et al. 2017).

In the revised version, we added these references and briefly summarized the results of these studies

Table 1 – I am not sure saying "present" is the best way to express this. For one thing, this paper will be published in 2026 but the datasets are only analyzed up to 2024. Secondly, people reading the paper in future years would probably prefer to know the actual year that was included.

We changed “present ” to “2024”.

Table 2 Isn't there also an ozone Lidar at Lauder?

The Lauder lidar is mentioned in Table 2.

Lines 115-132 There is no discussion of ozonesondes here even though we know the homogenization is a major issue and a lot of work has been done. (I suspect this might have been an accident).

This was accidentally missing. We added: ” Ozonesonde data have been homogenized, so that their stability is expected to be improved (Ancellet et al., 2022; Björklund et al., 2024; Van Malderen et al., 2025)”

Line 135 Should "CCMI" be "CCMI-2022"? I am a little bit confused on this point because I wasn't clear if you were using the newer model runs.

Corrected.

Line 139 "newly developed" - is this old text copied from an old document?

We replaced “newly developed” with “recent”.

Line 148 "a reference model dataset for comparison with observations" – this is what you're doing in the current work, isn't it? You should say that. Has there been other work to assess the models against observations?

Yes, we added “presented here”, and slightly rephrased this sentence.

Lines 149-150 This is an important point, to list what has changed since GB22, however I think you need a few more words to flesh out each of the four items listed.

In the revised version, we added a more detailed description of refD2 simulations.

Line 164 – were the other datasets deseasonalized with respect to 1998-2008 as well?

The deseasonalization in the datasets, which are created based on deseasonalized anomalies, is usually more complicated. First, the anomalies are computed using a reference time period (corresponding to a reference instrument or majority of the instruments), and then anomalies from earlier (like SAGE II) or later (like SAGE III/ISS) operating instruments are offset to this reference anomaly. Therefore, we used deseasonalized anomalies provided in these datasets.

Equation 1 – I don't understand why the beta terms are said to be a function of time? Wouldn't they be a function of height and latitude but not time?

The LOTUS regression model allows seasonal dependence for all coefficients. However, in this paper the seasonal variability is fitted only for three first coefficients, therefore we remove “t” from other β terms

Lines 222-223 "the uncertainties calculated with error propagation" – please clarify this statement

In the revised version, we explained the evaluation of uncertainties and added references.

Line 341 "CCMI" – again this should be "CCMI 2022", shouldn't it be?

Corrected.

Lines 361-362 Unfortunately, for me this statement "opens a can of worms". If the sampling is different, and that difference affects the trends, then doesn't that mean this comparison shouldn't be done at all unless the sampling effects are investigated and taken into account in some way? (Eg by using satellite data only on the day of each ozonesonde flight).

The climate data records from ground-based instruments are affected by so-called representativeness issues. For detection of drifts, analyses should be on collocated measurements, for both ozonesonde and satellite. And even in this case local profiles from ozonesondes measured during ~1-2h will not be fully compatible with nearly instantaneous but spatially (~300-400 km) averaged profiles from limb instruments.

Here we compare climate data records and the derived trends using data from different measurement systems, with their inherent features. From our point of view, the information on how the derived trends agree or differ is important.

In the revised version, we added the references on the papers, which discuss the problem of representativeness of ground-based climate data records.

Line 363 As mentioned above, you should at least mention Björklund et al. 2024 and Zhang et al. 2024 here.

In the revised version, we added: “Zeng et al. (2024) reported that homogenization of ozonesonde data at Lauder has a small impact on post-2000 trends. The analyses of drifts between climate data records from different instruments at Lauder by Björklund et al. (2024) have explained roughly half of them by the different sampling, vertical sensitivity, or time periods and gaps.”

Lines 376-377 Isn't the agreement better at the European sites because multiple records have been combined (2 or 3 of everything) over dispersed locations and so smoothed out rather than one each at single points?

In the revised version, we added this as a possible reason.

Line 419 In the figure I can only see a negative trend in SCIAMACHY-OMPS, not "especially".

Small negative trends over Antarctica between ~50° W and ~50°E are seen also in MEGRIDOP and MLS. In the revised version, we changed “especially” to “primarily”.

Minor comments

Line 88 "... the climate models" replace with "coupled climate chemistry models"

Line 104 Improve the wording of "(But Aura MLS v4 ...)"

Line 110 "is done" – replace with "was found by" or similar words.

Line 157 Insert "and" before "stratospheric"

Line 188 Delete "the" before "zenith sky".

Line 232 Replace "they are" with "which are"

Line 236 Insert "the" before "majority"

Line 237 Insert "the" before "uncertainties"

Line 399 Insert "the" or "these" before "aforementioned"

Line 401 Insert "the" before "shorter"

Line 402 Insert "of" after "poleward"

Line 404 Insert "the" before "2003-2018".

All are corrected.

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