

Interactive comment on “On ozone trend detection: using coupled chemistry-climate simulations to investigate early signs of total column ozone recovery” by James Keeble et al.

Anonymous Referee #1

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1 Recommendation

Based on simulations by the UM-UKCA chemistry climate model, the authors investigate past and current trends in total column ozone, the years when the expected increases in total ozone might become significant, and the years when total ozone might return to 1980s levels. These questions are relevant for the expected recovery of the ozone layer, and for checking the success of the international Montreal Protocol protecting the ozone layer.

The used data and methods appear solid. The paper is generally clear, concise and

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well written. What I am missing, however, is a more thorough comparison with observed trends, and with existing literature. I am also missing a more detailed explanation, how uncertainties were estimated. I think all this can be fixed in a revised version. After addressing my comments below, the paper should be acceptable for publication in ACP.

2 Major comments

1.) I think it is absolutely necessary to compare the simulated trends and their uncertainties with trends from observations. In my Fig. 1 below, I have overlaid observed trends from Fig. 7 of Weber et al. (2017) onto the simulated trends from Fig. 2 of the current manuscript. Note that the trends from Weber et al. are in % per decade, so at higher latitudes they tend to appear smaller compared to the DU per year trends of the current manuscript. Nevertheless, the observed trends appear to be much smaller at mid-latitudes and in both hemispheres. The uncertainties, on the other hand seem to be quite comparable. A comparison like that would be extremely valuable. I urge the authors to add such a comparison to their paper. Preferably this would be in an additional Figure, comparing simulated and observed trends (e.g. from Weber et al. 2017), as well as their uncertainties, and using the same units (DU per time or % per time). If this comparison confirms the impression from my Fig. 1 below, and the observed trends at mid-latitudes are indeed much smaller, this would be an important finding. Such an apparent lack of significant ozone increases at mid-latitudes would question our expectations for ozone recovery (see also Ball et al. 2017).

2.) A similar comment applies to the recovery detection years, where the current results need to be put more into the context of existing literature. My Figs. 2 and 3 below, for example, compare recovery trend magnitudes and detection years from this study (Figs. 2 and 3) with those from Figure 3 of Weatherhead et al. (2000). Weatherhead

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et al., from their 2 D model, find trends that are only about half the size of trends in the current study, and also find detection times that are about twice as long as in the current study. In my Figs. 2 and 3 below, I have scaled the Weatherhead et al. results to account for that (see also Eq. 2 of Weatherhead et al. 2000). The comparison in my Fig. 3 indicates that the expected detection years in the current study are generally earlier than in the Weatherhead et al. study, particularly in the tropics, but also at Northern mid-latitudes. Clearly the magnitude of the expected trend plays a large role, especially when trends go to zero (tropics). I think this needs to be brought out much clearer in the current manuscript.

3.) What also needs to be discussed more is one of the main messages of Fig. 3 of the current manuscript. Essentially, this figure says that by 2017 significant ozone recovery should have been detected between 15° and 50° latitude and in both hemispheres. In reality, however, I don't think that is the case, e.g. Weber et al. 2017. So what is going wrong? Is the model too optimistic? Are the uncertainty bars too small? Are the observations too bad? Is the atmosphere not doing what it is supposed to? I think these questions need to be discussed more, and could really be key points of the paper. Just pointing out the large sample size of the simulations (e.g. page 8 lines 16, 17) is not enough. Certainly, to be meaningful, these results need to be translated into something that is observable in the real world.

4.) In this general context, I am surprised about the small uncertainty of the detection years in the tropics in the authors' Fig. 3. Since the uncertainty of the trends in the authors' Fig. 2 includes zero, no trend is a possibility, and detection of a significant trend would take forever. Why is that not reflected in the small tropical error bars in Fig. 3? Compare also the (much more realistic) large spread between the blue and red data points in the tropics in Fig. 4, or the late tropical detection years in Weatherhead et al. (2000).

5.) Generally, I am missing clear explanations, how the error bars were obtained in Figs. 2 to 4. See my detailed comments below for specifics.

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6.) Since the authors have not really presented much information about point (i) the slowing of past ozone decline and the date of minimum column ozone, I suggest to delete this specific point, especially in abstract and conclusions. I agree with the authors statements in Section 4, especially page 5, lines 19, 20: the date of minimum ozone is a poor metric and therefore point (i) should not really be given much attention and should not be mentioned in abstract and conclusions.

3 Detailed comments

Page 1, lines 10, 11: See my major comment 6, above.

Page 1, lines 12 to 14: I find this sentence weird and confusing. Of course, all kinds of mistakes can be made. Maybe just drop this sentence, move the "(e.g. solar cycle, QBO, ENSO)" after "natural cycles" in the following sentence, and start that sentence with something like "Our investigations point to the need . . ."

Page 1, line 17: See my major comment 3, above.

Page 1, line 18: What do you mean by "sizeable"? I think what you really mean is something like the ratio of trend to natural cycle variability, or trend to unexplained variability. Please reword, clarify.

Page 1, line 22: This is a good statement, but it is in conflict with the small tropical uncertainty bars in Figure 3. See also my major comments 4 and 5.

Page 1, line 25, "were shown to"; page 5, line 16, "seen to be"; page 7, line 9, "are found to": I suggest to drop such unnecessary wordings, possibly also in other places.

Page 2, line 5: Drop "the difference"?

Page 2, line 9: Drop "Solomon et al. 2016". I don't think that paper says much about changing BDC / ozone transports.

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Page 2, line 20: I agree that the linear assumption is “somewhat simplistic”. However, so many studies, including complex CCM studies, have shown that, in the end, the whole system behaves remarkably linear, and that the linear assumption does work very well. So maybe replace “somewhat simplistic” by “surprisingly robust”?

Page 2, line 33: Maybe drop that line, and reword the previous sentence? See my major comment 6.

Page 2, line 34: I would add “after accounting for natural variability” after “values”. In fact you say and show later that accounting for these cycles is important. Obviously, unaccounted for ups and downs are prone to misinterpretation. So here, and in other places, little text and attention should be given to those “raw” results.

Page 2, lines 38, 39: I would drop “as proxies for atmospheric observations”.

Page 3, line 24: I wonder about the sea-surface and ice conditions. For two reasons: In Fig. 1, the red line appears to be much smoother after 2000, and much more variable from 1960 to 2000. Are your runs using observed sea surface conditions before 2000, and some climatology after 2000? Do missing real surface conditions have something to do with the mismatch between your simulated trends and observed trends e.g. from Weber et al. (2017), see also my supplemented Fig. 1. I think you should clarify this, and also make some statements about the importance of sea surface conditions for these ozone trends. I think there is past work by Braesicke and others on the influence of sea surface conditions on the stratosphere, and probably a lot more to be cited here – ask John Pyle.

Page 3, lines 35, 36: This equation needs a lot more explanation. Are you using monthly means, or what? Are the data deseasonalized?

If the $TO3_{e,l,i}$ is to be meaningful ozone, all the predictors have to be normalized to mean 0, or to 0 under “normal” conditions. This should be stated.

What does the subscript i mean in $TO3_{e,l,i}$? Calendar month?

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Certainly the a^x need to depend on latitude and be a_l^x .

Is the regression applied to all ensemble runs at once (providing a_l^x), or individually to each run (providing $a_{e,l}^x$)?

How do you deal with autocorrelation in the $N_{e,l,t}$? Autocorrelation can be substantial, e.g. 0.6 in the tropics, (see Plate 3 of Weatherhead et al. 2000). This reduces effective sample size and increases error bars, and needs to be accounted for. In the same direction: How independent are the ensemble runs? In the model world they may be independent, but compared to the real world, they are not really independent samples drawn from a large population.

Page 4, lines 12 to 14: I would move that sentence much closer to the Equation. I think it is important to understand what is fitted.

Page 4, line 17: I am missing an explanation how the trends in Fig. 2 are obtained. Presumably for the MLR trends, you fit straight lines to the $RO3_{e,l,t}$ and obtain the trend uncertainty from the fit residuals / remaining noise? Again: Are all ensembles fitted at the same time, or do you fit each ensemble separately?

Don't the a^x need subscripts e, l ? How is autocorrelation in the fit residuals dealt with?

How do you obtain the raw model trend in Fig. 2? By simply fitting straight lines to the $TO3_{e,l,t}$ on the left side of the first equation? Or piecewise linear trends? Please add text here or later, and answer these questions.

Page 5, lines 18, 19: See major comment 6. Same paragraph: I think you should also add some arguments based on your Fig. 1, e.g. the large uncertainty range for minimum ozone from 1992 to almost 2010, with little difference between blue and red curves.

Page 5, line 35: 95% confidence intervals – obtained how and from what? Please explain.

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Page 5 lines 36, 37: “heterogeneous . . . vortex”. Not only that. The Brewer Dobson Circulation also “transports” the large ozone trends from the upper stratosphere polewards and downwards into the lower stratosphere. There, near the ozone maximum, they make a big difference for column ozone (whereas otherwise upper stratospheric ozone does not contribute a lot to the total column). Please reword, or add some text.

Page 5, lines 38, 39: Please add “declining” or “1980 to 1997” before “trends”. Otherwise this is misleading and might be mistaken with the increasing 2000 to 2017 trends.

At some point, you might also want to point out that by picking 1980 to 1997 and 2000 to 2017, you have picked two one-and-a-half solar cycle long periods. This would maximize solar cycle effects on the trends (e.g. solar max at one end, solar min at the other end). So some of your results might include large solar cycle effects – but still the comparison of raw and MLR trends in Fig. 2 does not look too bad. You do have a corresponding discussion on page 6, lines 9 to 18. However that discussion reads a bit awkward, and, to me, puts too much focus on the “raw trends”, which obviously are influenced by the solar cycle and obviously should not be used. Maybe reword that discussion.

Page 6, line 2: maybe add “and Pinatubo aerosol effects”

Page 6, line 10: add “and autocorrelation” after “variance” and “(Weatherhead et al. 2000)” after “data”.

Page 6, line 19: Replace “month” by “year”? Also in other places in this paragraph?

Page 6, lines 19 to 30: How did you obtain the error bars in Fig. 3? From comparing results of the different runs? Is that realistic? See my major comments 4, 5.

Page 7, line 24: add “calendar” after “to”, and replace “certain months” by “e.g., in September”

Page 7, line 35: same as major comment 6.

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Page 8, lines 2 to 5: same as page 1, lines 12 to 14.

Page 8, lines 12 to 19: What about transport variations? You are not talking / accounting for them at all. See also my comment above about sea surface conditions. I think you should add something here, and also discuss the differences to the observations more, e.g. citing Weber et al. 2017 and Ball et al. 2017. See also my comment above about sample size, and my major comments 2 and 3.

Page 8, line 20: Same as Page 1, line 18.

Page 8, line 23: To me, it is worrying that precisely there Weber et al. 2017 find small and non-significant increases (see also Ball et al. 2017). I think you need to comment more on that, and I think this difference could be a key message from this study. See also my major comment 1.

Page 15, Figure 2: In the legend in the Figure. Please replace “Model trend” by “simple trend” or “raw trend”. That would be clearer, and the “MLR” trends are “model” trends as well. In the caption, please explain how the error bars were obtained.

Page 16, Figure 3: In the caption, please explain how the error bars were obtained. See also my major comment 4.

Page 17, Figure 4: Why was that not done for the MLR / residual total ozone as well? Should that not be shown? In the caption, please explain how the error bars were obtained.

4 References

Weatherhead, E. C., Reinsel, G. C., Tiao, G. C., Jackman, C. H., Bishop, L., Frith, S. M. H., DeLuisi, J., Keller, T., Oltmans, S. J., Fleming, E. L., Wuebbles, D. J., Kerr, J. B., Miller, A. J., Herman, J., McPeters, R., Nagatani, R. M., and Frederick, J. E., 2000.

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Detecting the recovery of total column ozone, *J. Geophys. Res.*, 105, 22201-22210, <https://doi.org/10.1029/2000JD900063>.

Weber, M., Coldewey-Egbers, M., Fioletov, V. E., Frith, S. M., Wild, J. D., Burrows, J. P., Long, C. S., and Loyola, D., 2017. Total ozone trends from 1979 to 2016 derived from five merged observational datasets – the emergence into ozone recovery, *Atmos. Chem. Phys. Discuss.*, <https://doi.org/10.5194/acp-2017-853>, in review.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, <https://doi.org/10.5194/acp-2017-1206>, 2017.

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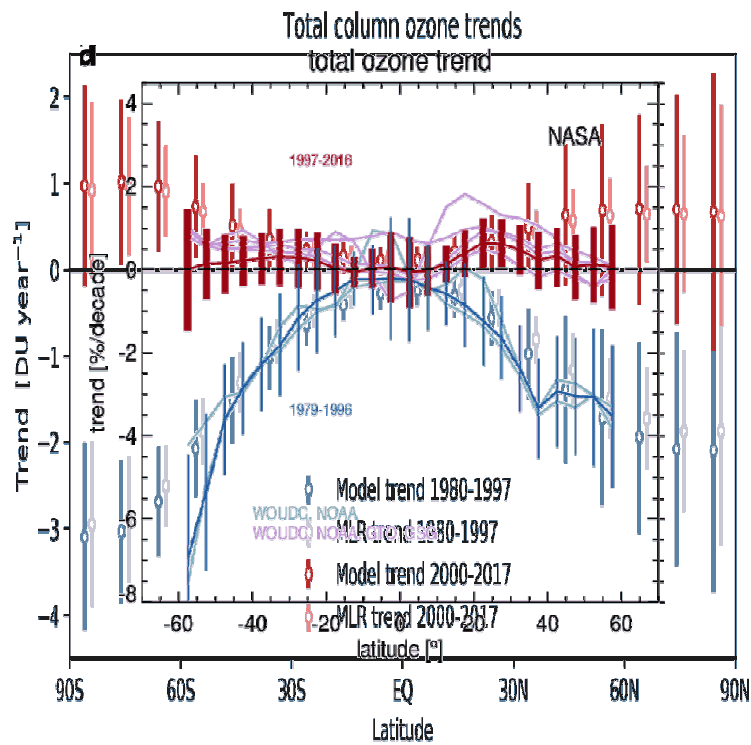


Fig. 1. Comparison of Fig. 2 of reviewed manuscript (trends in DU / year) with observed trends (% / decade) from Weber et al. 2017.

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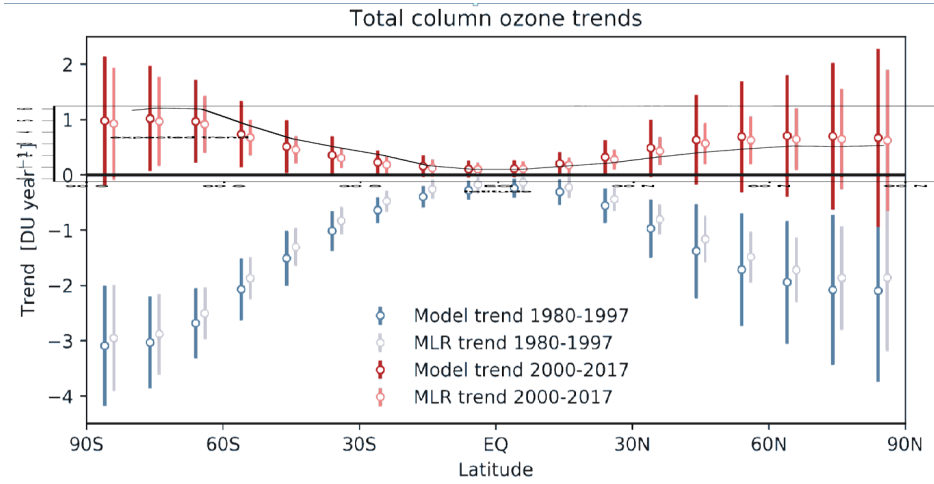


Fig. 2. Comparison of total ozone trends from the reviewed manuscript with Weatherhead et al. 2000 (black curve), scaled up by factor 2.

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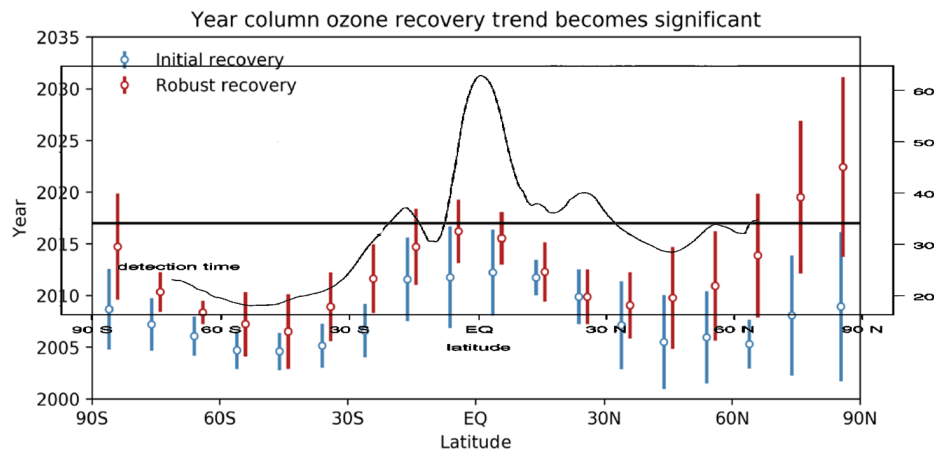


Fig. 3. Comparison of year for detection of significant increase from the reviewed manuscript with Weatherhead et al. 2000 (black curve), scaled down by factor of 2.

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