Atmospheric
Chemistry
and Physics
Discussions

Open Access

ACPD

Interactive comment

Interactive comment on "Stratospheric ozone loss in the Arctic winters between 2005 and 2013 derived with ACE-FTS measurements" *by* Debora Griffin et al.

Anonymous Referee #3

Received and published: 31 January 2018

Review of: Stratospheric ozone loss in the Arctic winters between 2005 and 2013 derived with ACE-FTS measurements

By Griffin and colleagues.

General comments

This is a nice paper that does a commendably thorough job of using ozone (and other observations) from the ACE-FTS instrument to quantify, via application of a range of techniques, chemical ozone loss in the Arctic winter/spring. The work is a welcome addition to the field and, in my view, close to being ready for publication in ACP, a journal to which it is well suited. The discussion and results are presented in a very

logical fashion. The standard of English and the quality of the figures etc. are excellent.

I really only have a very few minor comments/suggestions/fixes that should take very little time to implement/explore.

Given that the Match approach has been applied to similar measurements (e.g., from POAM), and one of the authors is highly versed in that technique, it is perhaps a little surprising that that method was not included, or even discussed very much. That said, I can well believe that the ACE-FTS sampling presents a challenge to the implementation of Match-based calculations. Whatever the reason, it would make sense to comment on why it is omitted here. If it's left for "future work", then it's fine to just say that. On the other hand, if there is some reason why it's not practical in this case, it would be good to note it here, as this may prevent others from potentially spending time fruitlessly investigating it in future.

Specific comments

Page 2 line 10: Add a comma after "March 2005" possibly.

Page 7 line 16 and line 20: "blue dots" should be "green dots" in both places. Also, it's a little jarring to be talking about dot color before the figure has been formally introduced (line 16/17).

Page 7 line 29/30: I think "One method that provides a correction for both mixing and for descent..." would be clearer. That is unless I've misunderstood the currently ambiguous wording (it currently could be read as saying that "descent" is another "method" that fixes the mixing issue, rather than another problem to be addressed).

Page 8 line 21: "blue dots" should be "green dots" again.

Page 11 line 5: Just to clarify this is a "horizontal" interpolation only, correct? From the text I get the sense that the vertical "interpolation" is simply "nearest neighbor", correct? Would be good to clarify.

C2

Page 11 line 29: "reset" to what (presumably "ozone that responds to chemistry", but would be good to be clear).

Page 12 line 3: I suggest you change "up to" to "within" and add "great circle" after 0.5<degrees> (unless it's actually latiude or longitude specifically you mean here).

Page 14 lines 14-19: It feels odd to have the "artificial tracer" discussion after the discussion of descent here, given that earlier, in section 3, you introduced those techniques in the other order.

Page 14 line 32: I'd suggest changing "error" to "estimated uncertainties" here, to avoid anyone thinking your taking some kind of inter-method difference as a measure of a (potentially "correctable" error).

Page 15(ish): It does feel a little disjoint to have section 4.1 talking about the various tracer methods, and yet not have any discussion of the ATLAS/SLIMCAT results until you get to the overall intercomparison discussion in 4.2. Might some of the AT-LAS/SLIMCAT discussion not merit a subsection of its own.

Page 16 line 7: Here I think you're using "passive subtraction" to only mean the ATLAS/SLMICAT methods, correct? However, in the opening discussion of the manuscript, you have used "passive subtraction" to describe all of your methods (rightly so, as all involve some kind of estimate of passive ozone). Might be better to use a different term here.

Figure 1. I'm curious as to where the cluster of black points ("fliers" actually) with O3 around 4.5-5 ppmv in panels a,b,c and d have "gone" in e and f? Are these cases where there were no OCS or CCl3F measurements? Or are they all hiding under the "e)" and "f)" legends (I hope not). Also, in the former cases (a-d) I would expect that they may be contributing significantly to the "uncertainty" in the fit. Might there be something geophysically unusual about them (their ozone abundance clearly implies as much) that would give you a good basis for discounting them? Also, you might want

to think about moving the a-f legends to a different corner of the plot to avoid clutter.

Figure 2: I'd move the legend (January, March) somewhere else so it doesn't get in the way. Also you don't need it on all four panels (you only had it on one panel in figure 1). That should make it easier for you to find an out of the way place.

Figure 5, caption, line 2: "...2011, with the combined regression fit for January and March...", assuming that's a correct interpretation.

Printer-friendly version

Discussion paper