

Interactive comment on “Global-scale distribution of ozone in the remote troposphere from ATom and HIPPO airborne field missions” by Ilann Bourgeois et al.

Anonymous Referee #2

Received and published: 11 May 2020

This paper reports tropospheric ozone distributions measured in ATom and HIPPO, compares them to ozonesondes and IAGOS to determine the consistency between the air sampled by ATom/HIPPO and long-term climatology, and comments on different features of the ozone distributions including vertical and seasonal distributions in the tropics and extratropics, and differences between the Pacific and Atlantic.

Ozone curtain data from ATom and HIPPO are worth reporting in the literature, and it is also of interest to place ATom and HIPPO in a broader climatological context, as can be done with ozone data but has broader implications for the ensemble of ATom/HIPPO measurements for which such long-term records don't exist. This is a useful though

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limited contribution. However, a big problem with the current paper is that it claims to discover what are very well known features of the global ozone distribution, and it fails to credit previous work (notably from Jennifer Logan, Ed Browell's group, and many model papers) that made exactly the same points 1-3 decades ago. GTE campaign papers from Ed Browell's group using DIAL (PEM series, INTEX-B, SONEX, TRACE-A, TRACE-P...) report very similar ozone curtains as ATom and HIPPO. All the findings reported here in the Abstract and Conclusions can be found in the previous literature. Once this is corrected and previous literature is properly credited, then it is not clear what is actually new in this paper in terms of scientific findings. It represents a limited contribution, and whether this is worth publishing in ACP is an Editor's decision.

Specific comments:

1. Abstract, lines 20-22: it is well known that there is more ozone in NH than SH.
2. Abstract, lines 22-23: uniformity of free tropospheric ozone at northern mid-latitudes has been known since Logan JGR 1985 from ozonesonde data.
3. Abstract, line 24: the higher ozone over tropical Atlantic than Pacific is well known – there is extensive literature on this from SHADOZ, TRACE-A, satellite retrievals (Jerry Ziemke...)
4. Abstract, lines 26-28: Continental influences over the NH oceans have been known for decades (PEM-Tropics B, INTEX-B over Pacific come to mind), vertical structure is a well known feature from ozonesonde and GTE data and again this has been extensively discussed in literature including countless model papers.
5. Abstract: that last sentence is gratuitous.
6. Introduction: there has to be some serious review of previous knowledge on the tropospheric ozone distribution from ozonesondes, GTE aircraft campaigns, satellites, models. The authors are addressing a very old problem, on which there is a lot of literature. Ignoring that literature is not right, particularly because it leads to claims in

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the Abstract and Conclusions that suggest that the authors are unaware of it.

7. Figures: there are way too many figures, and too much information on the figures – the eyes glaze, there's a lot of repetitiveness. It's hard to care about Sscore.

8. Line 306: “airborne campaigns can capture global baseline O₃ values”. Didn't we already know this from the GTE campaigns?

9. Lines 364-366, also 399-400, 409-411: we do know that ozone can be >100 ppb in biomass burning plumes over the South Pacific in spring (several papers coming out of PEM-Tropics A in Sep-Oct1996), seems like ATom/HIPPO just didn't hit them.

10. Lines 452-453 – uniformity of free tropospheric ozone at northern mid-latitudes has been known since Logan 1985 and has been shown in countless models.

11. Lines 496-497: continental influence on ozone over the North Pacific is hardly new.

12. Lines 516-, Conclusions: the conclusions list the same unwarranted claims of “valuable new insight into O₃ distributions in remote regions” (lines 520-521) as the Abstract. All of the features presented here about the global ozone distribution have been amply documented in the literature.

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-315>, 2020.

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