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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Stratospheric ozone trends for 1985–2018: sensitivity to recent large variability" *by* William T. Ball et al.

Anonymous Referee #1

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This manuscript is an update and extension of a similar analysis by Ball et al. (ACP, 2018), which reported evidence from satellite data that ozone in the lower stratosphere at latitudes less than 60 degrees has continued to decline since 1998 even though upper stratospheric ozone has started to recover in response to the Montreal Protocol bans of many ozone-destroying substances. Since much more ozone is in the lower stratosphere than in the upper stratosphere, the lower stratospheric decline dominates. On the other hand, evidence for an increase in tropospheric ozone was reported that could potentially cancel out the lower stratospheric decline. In that 2018 paper, no conclusion about the cause of the continued lower stratospheric decline was drawn and possible explanations involving both dynamics and chemistry were only briefly discussed.

In this manuscript, the trend analysis is slightly updated by the addition of more data and a stronger conclusion is drawn that the cause of the continued decline in the lower stratosphere is dynamical in nature rather than chemical. Specifically, the manuscript agrees with a recent study by Chipperfield et al. (GRL, 2018) that a strong positive ozone anomaly in 2017 was driven by short-term dynamical transport of ozone. However, it is further shown here that this short-term increase was caused by the stratospheric quasi-biennial oscillation and that a long-term gradual decline in lower stratospheric ozone remains even when the QBO-related variability is taken into account. The manuscript also agrees with the Chipperfield et al. conclusion that short-lived chemical depletion cannot explain the long-term decline, which must therefore be dynamical in origin.

I have two main comments on the manuscript, at least one of which can be considered as major. If the authors can satisfactorily address these comments in their revision, publication can be recommended.

(1) The magnitude of the long-term trends in zonal mean extrapolar lower stratospheric ozone is not clearly stated in the abstract or in most parts of the text. This leaves readers in the dark about how important the declines are. According to Figure 2a, the overall quasi-global decrease over 1985-2018 (33 years) is roughly 3.5 DU, which is in the range of 1.0 to 1.5%, or roughly -0.5% per decade. For comparison, the increase in tropospheric column ozone in the same latitude range estimated by Ball et al. (2018; their Figure 4) is +1.68 +/- 0.11 DU per decade or roughly +0.5% per decade. Could the authors please add numbers like this to the abstract and conclusions sections? The present manuscript notes large uncertainties in the actual tropospheric ozone increase (lines 38-41). However, at least the magnitude of the estimated lower stratospheric ozone decreases and the strong possibility of compensating tropospheric ozone increases should be more explicitly stated. Then readers can judge for themselves the importance of the observed lower stratospheric ozone decline due to dynamical processes.

(2) In this manuscript, as well as in the published work of Ball et al. (2018), the seasonal and longitude dependences of the observed zonal mean ozone trends are not evaluated. By not evaluating these characteristics or at least reviewing previous work on these characteristics, the authors are missing some important clues for understanding the origin of the observed zonal mean ozone trends in the lower stratosphere. Beginning in the 1990's, a number of authors have found that there is both a seasonal and a longitude dependence of column ozone trends, most of which originates in the lower stratosphere, especially at middle latitudes in winter and spring. Some of these authors concluded that these dependences are a consequence of decadal variability of quasi-stationary ultra-long Rossby waves that propagate from the troposphere into the stratosphere (Hood and Zaff, JGR, 1995; Peters and Entzian, Meteorol. Z., 1996; Peters et al., Beitr. Phys. Atmos., 1996; Hood et al., JGR, 1997; Hood et al., JGR, 1999). Decadal climate variability was therefore implicated. This was an extremely controversial and unpopular conclusion at the time because the prevailing view was that the observed ozone trends in both the polar and extrapolar regions in the lower stratosphere were dominantly chemical in origin. Note that these quasi-stationary waves are not necessarily linear (even though most mathematical treatments make this assumption) so that the zonal mean ozone change is not necessarily zero. Moreover, there is an associated trend in the BDC, which also affects ozone amounts, because the BDC is driven by Rossby wave breaking and absorption in the stratosphere (e.g., Hood and Soukharev, JAS, 2005). The continued decline of lower stratospheric ozone reported in this manuscript may therefore imply that there is a long-term component of the variability of these ultra-long waves. Schneidereit and Peters (Atmosphere, v. 9, p. 468, 2018) have recently investigated the zonally asymmetric component of ozone trends over 1979-2016 and find negative trends over Europe in winter that are twice as large as the zonal mean trend. A strong negative trend in one area such as this could produce a net change in the zonal mean ozone if the waves are non-linear. These authors suggest that long-term changes in the Rossby wave train that propagates out of the tropics and is linked to Arctic warming may be the ultimate driver. If the authors

are not able to investigate zonally asymmetric ozone trends or long-term changes in quasi-stationary waves and their breaking behavior in the stratosphere for the present manuscript, then they should at least include some discussion of this previous work with references to some of the above papers.

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