Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., community comment CC1
https://doi.org/10.5194/acp-2021-664-CC1, 2021

use of potential vorticity

Heini Wernli

Community comment on "The drivers and health risks of unexpected surface ozone enhancements over the Sichuan Basin, China, in 2020" by Youwen Sun et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-664-CC1, 2021

The way how potential vorticity (PV) is used in this study as a tracer of stratospheric air is very confusing and most likely flawed. Many previous studies used potential vorticity changes along air parcel trajectories to identify events of stratosphere-to-troposphere transport, which can significantly affect near-surface ozone concentrations (e.g., Wirth and Egger, 1999; Stohl et al., 2003; Lefohn et al., 2012; Škerlak et al., 2014). These and many other studies clearly show that it is important to consider the vertical structure of the PV field and the evolution of PV along trajectories associated with observations of enhanced surface ozone. However, in the study by Sun et al. it is not clear at what level and how PV has been evaluated. The abstract mentions "downward potential vorticity" (p. 1 line 34), but PV is a scalar, it has no orientation, therefore "downward PV" does not make sense to me. Then an "increase in PV" is mentioned (p. 2 line 29), but it is not clear where this increase should occur. When stratosphere-to-troposphere transport down to surface occurs, then typically the originally high stratospheric PV values is reduced during the transport by diabatic processes and therefore the air parcel may arrive at the surface without high PV but still with elevated ozone concentrations. Near the page break of p. 10/11 it is mentioned that vertical convection "will continuously intensify the cyclonic vorticity over the Sichuan basin" – it is not clear to me whether this sentence refers to relative vorticity or to PV? And is it about the downward transport of stratospheric PV or about the diabatic production of low-level PV? The latter process would most likely not lead to enhanced surface ozone values. And finally, Fig. 6a shows "differences in PV … between 2020 and 2019". How were these PV fields calculated and at what level is PV considered here? The field shown in Fig. 6a is not useful when investigating a potential influence of stratospheric intrusions on near-surface ozone. The differences are on the order of 0.1 PVU (1 potential vorticity unit = 10^{-6} K m^{-2} kg^{-1} s^{-1}), which is very small. I don't understand how the authors conclude from Fig. 6a that "the meteorology-induced surface ozone increase is mainly attributed to significant increases in temperature and downward potential vorticity" (p. 14 line 4).

References:

Lefohn, A. S., H. Wernli, D. Shadwick, S. J. Oltmans, and M. Shapiro, 2012: Quantifying the importance of stratospheric-tropospheric transport on surface ozone concentrations at

high- and low-elevation monitoring sites in the United States. Atmos. Environ., 62, 646–656.

Škerlak, B., M. Sprenger, and H. Wernli, 2014: A global climatology of stratosphere-troposphere exchange using the ERA-Interim data set from 1979 to 2011. Atmos. Chem. Phys., 14, 913–937.

Stohl, A., H. Wernli, M. Bourqui, C. Forster, P. James, M. A. Liniger, P. Seibert, and M. Sprenger, 2003: A new perspective of stratosphere-troposphere exchange. Bull. Amer. Meteor. Soc., 84, 1565-1573.

Wirth, V., and J. Egger, 1999: Diagnosing extratropical synoptic-scale stratosphere–troposphere exchange: A case study. Quart. J. Roy. Meteor. Soc., 125, 635–655.