Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., community comment CC1
https://doi.org/10.5194/acp-2021-1028-CC1, 2022

Comment on acp-2021-1028

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Community comment on "Tropospheric ozone production and chemical regime analysis during the COVID-19 lockdown over Europe" by Clara M. Nussbaumer et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-1028-CC1, 2022

Dear Clara,

Many thanks for this interesting study. Below I would summarize my understanding about your work and provide some comments if you find them useful.

The study aims to investigate the changes in the vertical distributions of atmospheric species over Europe before and during the COVID-19 pandemic. The pre- and intra-pandemic vertical distributions were measured by 3 aircraft campaigns: UTOPIHAN campaigns in 2003/2004, the HOOVER campaigns in 2006/2007, and the BLUESKY campaign in 2020. The model ECHAM5/MESSy2 Atmospheric Chemistry (EMAC) is run in a pre-pandemic scenario (also known as the no-lockdown scenario); the model data subsampled along with the flight tracks of the three campaigns areused to compare with the observations. The model data are first validated against HOOVER and are found to reproduce the HOOVER observations, including the trends. Then, assuming that the pre-pandemic atmospheres remain the same, this study compares the intra-pandemic observations by BLUEKSY with hypothetical pre-pandemic BLUESKY measurements constructed using the model data. A major finding is that in addition to the significant drop in major pollutants at the surface that are related to car exhausts such as NO_x and CO, there is also a significant drop in NO_2 in the upper troposphere at 10 km, which is likely due to the reduced air traffic. Nonetheless, this study finds that the production rate of O_3 in the upper troposphere remains unchanged despite the NO_x change. Another major finding of this study is that the chemistry regimes in the upper troposphere might have changed from a VOC-limited chemistry in the pre-pandemic era to a NO_2-limited chemistry in the intra-pandemic era.

I have a few minor comments and hopefully you may find them helpful:

If I understand it correctly, in both Figures 2 and 3, there is only one model simulation: the ECHAM5/MESSy2 Atmospheric Chemistry (EMAC) that was run in the no-lockdown scenario. But Figures 2 and 3 may give an impression that there were different simulations separately for HOOVER and BLUESKY. Similarly, calling the subsampled model data on the BLUESKY flight path as BLUESKY-NL also made me think that there was another BLUESKY campaign before the lockdown. Would something like EMAC(on HOOVER path) and EMAC(on BLUESKY path) be clearer?

Since you found that there was more NOx in the upper troposphere before the pandemic,

have the possible self-contamination due to the NOx emission of the aircraft itself been removed or calibrated in order to establish the robustness of the NOx decrease from the pre-pandemic era to the post-pandemic era?

Figure S4–S6 are important results of this study. Especially, Figure S4 demonstrates the impact of air traffic in the model, which is one of the two major conclusions of this study. I strongly think that these 3 figures should be put in the text. The x-axis range of Figure S5 could probably be either re-adjusted or re-plotted using the log scale for better data representation.

The conclusion "While the NOPR did not change under lockdown conditions due to compensating effects in the NOx chemistry, we can expect impacts on tropospheric ozone from changes in VOCs (including CH4) relevant for future emission scenarios." Maybe a little more justification may help support this statement. For example, the impact of aviation NO_2 on O_3 and CH_4-related species in the upper troposphere and lower stratosphere during the pre-pandemic era have been discussed previously, e.g.

Khodayari, A., Tilmes, S., Olsen, S. C., Phoenix, D. B., Wuebbles, D. J., Lamarque, J.-F., and Chen, C.-C.: Aviation 2006 NOx-induced effects on atmospheric ozone and HOx in Community Earth System Model (CESM), Atmos. Chem. Phys., 14, 9925–9939, https://doi.org/10.5194/acp-14-9925-2014, 2014.

Khodayari, A, Seth C. Olsen, Donald J. Wuebbles, Daniel B. Phoenix, Aviation NOx-induced CH4 effect: Fixed mixing ratio boundary conditions versus flux boundary conditions, Atmospheric Environment, 113, 135-139, https://doi.org/10.1016/j.atmosenv.2015.04.070, 2015.

I think by adding some discussions of these literature may help strengthen your study. In addition, have you tried changing upper tropospheric CH_4 in the ECHAM5/MESSy2 model and test its impact on upper tropospheric O_3?

Overall, this is a very interesting study. Thank you for your work and good luck!

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