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Comment on acp-2021-785

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Community comment on "Ozone pollution during the COVID-19 lockdown in the spring of 2020 over Europe, analysed from satellite observations, in situ measurements, and models" by Juan Cuesta et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-785-CC2, 2021

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The manuscript caught my eye because the analysis focused here is on the same time period, the same area, and the same atmospheric compounds as those in Souri et al. [2021]. Our draft was cited more than 16 times in the current manuscript, extensively expressing a strong degree of agreement, particularly in terms of the surface ozone enhancement over central Europe. Nonetheless, there are two striking differences in this study compared to those in Souri et al. [2021], Ordóñez et al. [2020], and Barré et al. [2021], both of which were not thoroughly justified. A disagreement is far more interesting than an agreement, but if it results from a negligent model (or faulty data), it should be rectified. Two substantial differences follow:

- Surface MDA8 measurements based on UV photometry (which are highly accurate) show a large reduction in the Iberian Peninsula (called southwestern Europe in the manuscript). This tendency coincided with Souri et al. [2021] and Ordóñez et al. [2020]. The studies of Souri et al. [2021] and Ordóñez et al. [2020] came to the same conclusion showing that the large reduction of surface ozone was due to meteorology. In particular, Souri et al. [2021] showed that the anthropogenic factor played a very marginal role in shaping the decline over the area. The major reason behind the reduced ozone is assumed to be cloud causing photochemistry to dampen, shown in Figure 2 in Ordóñez et al. [2020] and the last column of Figure 6 (the ratio of photolysis rate below clouds to a clear-sky) in Souri et al. [2021]. Our analysis using TROPOMI NO_2 showed that the frequency of the satellite observations is 2.5-3 times as large in April 2019 as those in April 2020 due to more overcast in the latter. The numerically resolved $P(O_3)$ values (Figure 12 in Souri et al. [2021]) were found to be relatively low over the Iberian Peninsula suggesting that the anthropogenic emissions are not sufficiently high enough to become the main driver of ozone anomalies observed by surface measurements. This manuscript, on the other hand, claims that 4.5 ppbv out of 5.0 (90%) of the reduction in the MDA8 surface ozone is solely due to the emissions (Table 3). How did the models perform with respect to cloud optical thickness and cloud fraction? It may be worth calculating J values (an approximate value can be derived based on https://www.cmascenter.org/cmaq/science_documentation/pdf/ch14.pdf)

- Souri et al. [2021] and Barré et al. [2021] observed a large reduction from in-situ and TROPOMI tropospheric NO_2 columns measurements over central Europe in March-April-May 2020 with respect to a reference (e.g., 2019). Figure 8 shows a substantial enhancement of NO_2 over Germany in this manuscript, which strongly contradicts two other studies. This discrepancy has not been well justified. While we did see some disagreement between the satellite and the surface observations, it is highly unlikely for the surface measurements to be substantially different among all these studies. Please double-check the data (their validity flag) or your code to see if this is caused by a bug. If this is true (which is extraordinary), please dedicate a paragraph to discuss why.

Best,

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