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Comment on acp-2021-785

Anonymous Referee #2

Referee 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.,
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Overview

The paper deals with the influence of the COVID pandemic on ozone pollution over Europe during the general lockdown in spring 2020. In my opinion, the manuscript is generally scientifically sound, presenting a lot of interesting information and analysis regarding in-situ and satellite measurements as well as modelling simulations during the lockdown period and it deserves publication in ACP, in principle.

Although the manuscript content is interesting, I think that the presented information is too dense making the paper reading rather difficult. For this reason, I would encourage the authors to try to reduce the length of the paper and be focused on the most essential points. I would suggest indicatively to reduce the modelling part by presenting only one modelling simulation including, if possible, the more recent emission inventory as well as the more important vertical mixing (see also comments below). In addition, the use of 8h-average maximum daily surface ozone concentrations would be sufficient and more appropriate as this parameter is more representative than the corresponding daily averages for comparison with the free-tropospheric satellite measurements.

General comments

Some remarks regarding the analysis of observational data are presented below, which I think that they would improve the clarity of the interpretation of the results:

As the IASI+GOME2 satellite measurements are most sensitive at 2-3 km height, it has to be noted that based on relatively recent publications (Kalabokas et al., 2013; Doche et al., 2014; Zanis et al., 2014; Akritidis et al., 2016; Kalabokas et al., 2017, Gaudel et al., 2018), the variability of free tropospheric ozone over Central Europe and even more over the Mediterranean basin could be better understood if the variability of synoptic meteorological conditions, affecting especially vertical ozone transport are taken into account. This process would allow the assessment of the influence of either higher tropospheric layers, usually richer in ozone, or boundary layer, usually poorer in ozone.

Based on the above, I would suggest examining, at least, the corresponding charts of Geopotential height, vector wind speed and omega vertical velocity for the lockdown period as well as their anomalies relatively to the average long-term climatology. By checking these charts, it comes out that in fact the two examined periods in April 2020 were very different from the meteorological point of view than the corresponding periods for April 2019 as higher atmospheric pressures and temperatures associated with enhanced downward vertical transport and indicating strong tropospheric influence to the boundary layer and to the surface were observed over most of the European continent, with the exception of its southwestern and southeastern parts. In these areas (Iberian Peninsula and Eastern Mediterranean) upward air movements were observed suggesting boundary layer influence to the free troposphere, which are usually associated with lower ozone concentrations. I think that this information might help explaining better the differences in ozone levels observed in Tables 2 and 3, over the examined European areas.

Specific comments

Figures 1, 2: As mentioned above, given the best sensitivity of the IASI+GOME2 satellite at 2-3 km altitude, I would suggest using mid-day ozone concentrations (like MDA8 used in later Figs), instead of morning ozone corresponding to the satellite passage time, for a more representative comparison between in situ surface and free tropospheric satellite ozone measurements, as at mid-day the tropospheric influence to the boundary layer gets its maximum, minimizing at the same time the effects of NO_x titration and dry deposition on ozone concentrations.

In relation to the above comments and given the high meteorological variability between the examined years, the comparison of the year 2020 with the 3-4 previous years would be more representative, instead of 2019 alone. It could be at least shown for the surface in-situ measurements, as I understand that for satellite measurements it would be a heavy task.