

Atmos. Chem. Phys. Discuss., referee comment RC1  
<https://doi.org/10.5194/acp-2021-785-RC1>, 2021  
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## Comment on acp-2021-785

Anonymous Referee #1

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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.,  
<https://doi.org/10.5194/acp-2021-785-RC1>, 2021

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### General Remarks

This paper describes changes in atmospheric pollutants over Europe in April 2020, attributable to the COVID-19 lockdowns and 2020 atmospheric conditions. The data and methods used are valid, and the results appear generally reasonable and plausible. The paper fits well into the scope of ACP. It adds some new insight to the large body of existing literature on atmospheric changes due to the COVID-19 lockdowns, and is in principle suited for ACP. I found the paper interesting but hard to read. Presentation and conciseness should be improved substantially. To me, there is way too much non-essential information in the paper, and a lack of focus on clear take-home messages. Essentially, the paper is trying to do too much:

Compare tropospheric average satellite data with surface in-situ data.

Compare observations with simulations.

Compare two quite different model simulations, one of them (C2) giving unrealistic low changes between 2020 and 2019.

Compare two fairly similar (and even overlapping) periods in April 2020, with little or no significant difference.

Compare surface daily averages with maximum surface 8 hour averages, again with no major differences.

In the end, this becomes very confusing, and I can't see a clear storyline. I think this paper needs major revisions, and should become substantially shorter and more concise.

### Major Suggestions

I strongly suggest to shorten the paper considerably and to remove a lot of the non-essential material. The removed material could either be dropped completely, or could be moved to a supplement. In a supplement it would still be published and available for people needing the extra information. Shortening will allow a much clearer and concise paper, focused on the major points.

The CHIMERE model version C2 seems to underestimate NO<sub>2</sub>, and COVID-19 related NO<sub>2</sub> reductions by a large margin. C2 also shows unrealistically small COVID-19 related ozone changes. What is the point of having simulation C2 in the paper? I suggest to drop all C2 related information (and possibly move C2 related information to a supplement).

Is there any major take home message for the difference between the two periods April, 1 to 15, and April, 1 to 30? I don't see neither a large nor an important difference. Therefore I suggest to retain only the April, 1 to 15 period (with the clearest COVID-19 effects) in the main paper, and to drop the April 1 to 30 period (or move it to a supplement).

Is there any major take home message for the difference between surface MDA8 and daily averages? I don't see neither a large nor an important difference. Suggest to drop MDA8 (or move it to a supplement).

In this way, the paper would be shortened considerably, and become much more concise and focused. While rewriting, the English should be improved in many places as well.

### **Minor Points**

Is it correct that the surface data are only taken at the locations and days of largely clear-sky IASI+GOME2 measurements? Is there a large difference to taking all surface data? Might be necessary to mention that, if necessary even show a plot.

Are the model simulation data sampled at the satellite locations and days, or at the groundbased locations and days, or are all model data used?

The -8 ppb shift of the 2020-2019 delta in the IASI+GOME2 data compared to the same delta in the surface data should not be called "bias". It is not a "bias", it is a larger observed difference, and in section 3.1.1 the authors mention a number of possible reasons. One thing not mentioned is ozone reduction in the upper troposphere, resulting from the Arctic stratospheric "ozone hole" in March and April 2020. Given the wide satellite averaging kernels, this may well contribute to the larger 2020 to 2019 difference seen in the satellite data. See also Steinbrecht et al. 2021, Bouarar et al. 2021, Miyazaki

et al. 2021, Ziemke et al. 2021 for more context. These references should generally be considered more to provide context for the paper.

Fig. 7: What would the modelled 2020-2019 difference look like for the simulation(s), if the wide satellite averaging kernels were applied? Would that result in larger negative anomalies more like the satellite observations?

One drawback of the regional model simulation domain is that it does not account for the hemispheric scale emission and background ozone reductions (and for changes in the stratosphere?). This may help to explain why all observed anomalies seem to be substantially larger than the simulated anomalies in Fig. 7 (see also discussion of Fig. 5 around line 360). CHIMERE C2 looks kind of useless with nearly no simulated anomaly- just drop it.

Figure 9 presents essentially the same information as Fig. 8. The only reason to keep Fig. 9 would be to also show satellite measured NO<sub>2</sub> columns. Without those, I would drop Fig. 9 (or move to supplement).

There is a lot of duplication / redundancy between Section 2 and introductory paragraphs in Section 3. I suggest to drop or shorten these text parts in Section 3.

Fig. 5 and other places. I am missing a direct comparison between modelled 2020-2019 differences (with averaging kernels?) and satellite observed differences. Was this not done, or was it omitted for the sake of conciseness?

### **Detailed Comments**

line 23: replace "particularly enhanced" be "better"?

line 30: "bias" is the wrong word. "difference"?

line 31: add "and averaging kernels extending into the upper troposphere"?

line 34: replace "for withdrawing" by "by subtracting"?

line 36: Is this not a null statement? Before you have said that both observational datasets are more or less consistent. Now you have subtracted the same meteorology from them, and they are still consistent. With the exception of a few really unusual cases, I would expect them to be consistent also after a subtraction or addition.

line 39: replace "highlight the" by "provide".

line 48: since the models underestimate so much, you should explain possible causes in the abstract as well (e.g. missing reductions in emissions and background ozone outside of the model domain). Also: since the models perform so poorly, how can you be sure they get the meteorological changes from 2019 to 2020 right?

### **Additional References**

Bouarar et al. (2021). Ozone anomalies in the free troposphere during the COVID-19 pandemic. *Geophysical Research Letters*, 48, e2021GL094204.  
<https://doi.org/10.1029/2021GL094204>

Miyazaki et al. (2021). Global tropospheric ozone responses to reduced NO<sub>x</sub> emissions linked to the COVID-19 worldwide lockdowns. *Science Advances*, 7, eabf7460.  
<https://doi.org/10.1126/sciadv.abf7460>

Ziemke et al. (2021). Evaluation and Validation of Tropospheric Ozone Hourly and Daily Maps Measured from EPIC, OMPS, OMI, and MLS Satellite Instruments. Presented at CEOS AC-VC 17 meeting. [https://ceos.org/document\\_management/Virtual\\_Constellations/AC-VC/Meetings/AC-VC-17/3.Wednesday-Ozone/3.04\\_ziemke\\_v1.ppt](https://ceos.org/document_management/Virtual_Constellations/AC-VC/Meetings/AC-VC-17/3.Wednesday-Ozone/3.04_ziemke_v1.ppt)