Responses to reviewers on

the manuscript "Ozone pollution during the COVID-19 lockdown in the spring 2020 over Europe analysed from satellite observations, in situ measurements and models" by Cuesta et al.

We would like to thank the reviewers for their suggestions that have improved the clarity of the paper. All their remarks have been addressed in the Revised Manuscript (RM). Their main suggestions have been followed by:

(i) Shortening and simplifying the paper and the figures (as requested by Referees #1 and #2), through suppressing from the main manuscript the results of the version C2 of the CHIMERE model, only showing results for only one period (1-15 April) and mainly in terms of daily averages. A single exception to this is one figure, which is important for comparing the results of the current paper with other publications. Some of the withdrawn results have been moved to the supplement as suggested by referee #1.

(ii) An additional figure (Figure 8) is added that briefly describes the link with the variability of ozone at the free troposphere and the link with the stratosphere, as well as the associated meteorological conditions at the free troposphere (as suggested by Referee #2).

(iii) A new figure (Figure 7) addressing the point stated by Amir Souri concerning the influence of meteorological conditions in near surface ozone over the Iberian Peninsula

(iv) Two additional Figures (Figs. 4b and 5d) and Table 3 show the model smoothed by the averaging kernel of the satellite approach and the direct comparison with respect to satellite data, as requested by Referee #1.

These main points are described in detailed in the following paragraphs, as well as all additional and specific points remarked by the Referees. All other points have also been clarified and addressed thoroughly.

Responses to comments from Referee #1

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: i) Compare tropospheric average satellite data with surface in-situ data, ii) compare observations with simulations, iii) compare two quite different model simulations, one of them (C2) giving unrealistic low changes between 2020 and 2019, (iv) compare two fairly similar (and even overlapping) periods in April 2020, with little or no significant difference and (v)

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.

Agreed and withdrawn from the main manuscript. As suggested by Referees #1 and #2, we have substantially shortened the main RM, limiting the description of the results in terms of one model setup, one period and one main metrics for surface ozone. Some of withdrawn material is provided as supplement. Additional specific comments on this topic are also provided in the next answer.

Major Suggestions

I strongly suggest shortening 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 NO2, and COVID-19 related NO2 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 dropping 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 retaining 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 dropping 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.

Agreed and withdrawn from the main manuscript. The original manuscript intended to show consistency or its absence between different periods, model setup and ozone metrics. We agree with the referees that presenting only the main results will simplify the manuscript and provide a clearer message for the reader. In consequence, the RM only shows CHIMERE simulation with the C1 setup which is simply called "CHIMERE", focused on the single period 1-15 April (where the greatest changes for the COVID-19 lockdown period are observed) and uses daily averages of surface ozone concentrations. The only exception to this is a figure (Fig. 3) that uses the same metrics and period as two other papers largely compared and cited in the RM (Ordonez et al., 2020; Souri et al., 2021).
Results for the whole month of April and the model setup C2 have been moved to the supplement. We have chosen to show C2 results in the supplement, as its comparison with C1 clearly reflects the uncertainties in the modelling tools, which is an original and interesting aspect of the paper.
The choice of daily averages for describing surface ozone distribution is supported by the fact that this metrics shows the same variability as satellite data of lowermost

tropospheric (LMT) ozone, a fair correlation, and a smaller average shift as compared to surface MDA8 ozone. This last aspect is shown in a new statistical comparison between satellite data and surface MDA8 ozone shown in Table 4. These results suggest a larger variability and a larger negative shift for MDA8 surface ozone than for daily averages, when compared with IASI+GOME2 LMT ozone. As suggested by the reviewer, we have substantially shortened and simplified the RM. Several analyses and comparisons have been moved to the supplement.
The English language of the whole manuscript has also been revised when rewriting.

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.

Clarified and comparison added. Yes, it is correct that Figure 1b only considered locations and days with coincident IASI+GOME2. However, very small differences are seen between the average of all in situ surface data and that coincident with IASI+GOME2. Thus, an additional figure is not necessary.
The clear similarity between these two averages is shown in a new case in Table 2 and lines 251-253 of the RM as " *… while Fig. 1b only considers in situ data coincident in time and space with satellite data (although very little differences are seen for the average of the whole in situ dataset).* " and lines 265-267 "*When considering the average of the whole in situ dataset, the only notable change is an increase in the standard deviation of 13% for the surface data (see Table 2).*".

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

Agreed and revised. The original manuscript does show the average of all days and locations for the CHIMERE model. Although very similar, we agree that for a more consistent comparison between Fig. 1 and 4, they all should use the same sampling as satellite and in situ data. Therefore, the new Figures 2b and 4 of the RM also show the average of model data coincident with satellite data. Table 2 of the RM shown the statistical indicators for data only coincident with IASI+GOME2.

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.

Agreed and revised. We agree that a difference between the surface measurements and satellite retrievals of LMT ozone is not a bias since they do not refer to the same atmospheric layers. We have corrected this indication in the whole RM, indicating it as a "difference" or a "shift".

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.

Agreed and added text and figure in the RM. We agree and appreciate the suggestion from the referee. In the RM, we have added a new Figure 8d showing IASI+GOME2 measurements of stratospheric ozone and a statement about the contribution of the reduction of stratospheric ozone in 2020 over Northern Europe, as compared to 2019, and the suggested references.

This sentence is given in line 281-284 of the RM *" This is mainly associated with the reduction of anthropogenic emissions at large scale during the pandemic lockdown in 2020 and in lower degree to a large 2020 springtime ozone depletion in the Artic stratosphere (less than one quarter of the observed tropospheric anomaly, see also Bouarar et al. 2021, Miyazaki et al. 2021, Ziemke et al. 2021). "* and lines 463-465 *"Over the North Sea, the reduction of upper tropospheric ozone at 6-12 km of altitude is strengthened by a depletion of stratospheric ozone occurring in 2020 (see in Fig. 8d as ozone anomalies with respect to 2019)."*

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?

Clarified and Added Figures in the RM. We agree that it is relevant to show the effect of satellite retrieval sensitivity and the vertical consistency between CHIMERE and the satellite product. The new Figures 4b and 5d shown model results smoothed by IASI+GOME2 averaging kernels. For total changes between 2020 and 2019, we remark that their main effect is smoothing and reducing the changes in ozone simulated by the model. However, we see a clear difference with respect to surface data when analyzing the simulated changes associated to COVID in terms of LMT ozone and also smoothing with IASI+GOME2 averaging kernels. In these two last cases, CHIMERE only simulates a weak reduction of ozone over all Europe and no ozone enhancements. This suggests a clear underestimation of the simulated changes associated to the pandemic lockdown averaged within the LMT (< 3km) as compared to IASI+GOME2 satellite observations.

This information is provided in the RM in lines 309-311 *"This is similarly found for LMT partial columns from CHIMERE smoothed by IASI+GOME2 averaging kernels (for accounting for the satellite vertical sensitivity, Fig. 4b), except for simulated enhancements over the Atlantic and Central-eastern Mediterranean."*

And lines 386-392 *"On the other hand, we notice clear differences in the simulated changes associated with the pandemic lockdown for model-derived concentrations integrated up to 3* *km of altitude (LMT) and also when smoothing with IASI+GOME2 averaging kernels (Fig. 5d). In these two last cases (see Table 3), CHIMERE only simulates a weak reduction of ozone over all Europe and ozone enhancements become negligeable. The range of variability of simulated concentrations decrease by more than a factor 10, although the correlation with respect to IASI+GOME2 data remains fair (around ~0.5). This suggests a clear underestimation of the amplitude of the effect of the pandemic lockdown simulated at atmospheric layers above the surface and within the LMT (< 3km) as compared to IASI+GOME2 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.

Agreed and clarified. We agree with these statements about the lack of background ozone reduction in 2020 due to the regional domain and the lack of variability between 2020 and 2019 for the modeled stratospheric contribution. They were implicitly considered in the original manuscript, but we agree to make a clearer statement.

In the RM, we provide more straight forward statements as (lines 46-48)*: "Moreover, a significant ozone decrease observed at large hemispheric scale is not simulated since the modelling domain is the European continent. As simulations only consider the troposphere, the influence from stratospheric ozone is also missing."* and lines 492-494 *"Furthermore, the model does not simulate the ozone decrease observed at large hemispheric scale nor the stratospheric influence, as the simulation domain covers Europe and the troposphere.".*

Figure 9 presents essentially the same information as Fig. 8. The only reason to keep Fig. 9 would be to also show satellite measured NO2 columns. Without those, I would drop Fig. 9 (or move to supplement).

Clarified and withdrawn from the main manuscript. The difference between Fig. 8 and 9 of the original manuscript was one is affected by vertical mixing (surface concentrations) and the other not (total column concentrations). Their comparison shows the role of vertical mixing within the atmosphere, which is the main explanation from differences between the simulations with the two model setups (C1 and C2, this last one shown in the supplement only). Therefore, the former Figure 9 of the original manuscript is withdrawn from the RM but included in the supplement.

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

Agreed and shortened. For avoiding redundancy, the introductory part of section 3 is substantially reduced in the RM.

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?

Clarified and Table 3 added. Direct comparisons between model simulations and satellite data were indeed previously omitted for sake of conciseness. For providing information about it while keeping the manuscript short, we have added an additional Table (Table 3) that provides the statistical scores of such satellite/model comparisons.

Detailed Comments

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

Agreed and corrected as suggested.

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

Agreed and corrected as suggested in the text and the figures.

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

Clarified. The averaging kernels of the IASI+GOME2 lowermost tropospheric ozone retrievals only reach the middle and sometimes the upper troposphere over ocean. However, they only extend within the lower troposphere (below 5 and 6 km of altitude) over land and they and peak around 2 km of altitude (see Cuesta et al., 2013).
Therefore, such a statement is added in the RM (31-32) *" … be explained by the fact the satellite approach retrieves partial columns of ozone with a peak sensitivity above the surface (near 2 km of altitude over land and averaging kernels reaching the middle troposphere over ocean). ".*

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

Agreed and corrected as suggested.

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 unusal cases, I would expect them to be consistent also after a subtraction or addition.

Clarified. The statement is no null since the correction for the two datasets is not the same. Surface data are corrected using model simulations of surface ozone and satellite data with model simulation of ozone integrated below 3 km of altitude (LMT) and smoothed by the averaging kernels. If those corrections were significantly different, corrected surface and satellite datasets could have differed.

This aspect is clarified in the RM as (line 36) " *Using adjustments adapted for the altitude and sensitivity of each observation* ".

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

Agreed and corrected as suggested.

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).

Agreed and accentuated. Those aspects were mentioned in the original abstract. These statements are rewritten in a more direct way in the RM as (lines 44-50) " *Moreover, a significant ozone decrease observed at large hemispheric scale is not simulated since the modelling domain is the European continent. As simulations only consider the troposphere, the influence from stratospheric ozone is also missing. Sensitivity analysis also show an important role of vertical mixing of atmospheric constituents, which depend on the choice of the meteorological fields used in the simulation, for better matching the observed changes of ozone pollution during the lockdown.* ".

Also: since the models perform so poorly, how can you be sure they get the meteorological changes from 2019 to 2020 right?
Clarified. A verification that the meteorological changes between 2020 and 2019 are sound is provided by a comparison with other independent studies from literature (particularly Ordonez et al., 2020 and Souri et al., 2021). The consistency of these corrections is thoroughly described in section 3.2 of the RM and Figure 3b of the RM.

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/10.1029/2021GL094204Miyazaki et al. (2021). Global tropospheric ozone responses to reduced NOx 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

Responses to comments from Referee #2

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 to 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).

Agreed and manuscript shortened. As suggested by both referees, we have substantially shortened the main RM, limiting the description of the results in terms of one model setup (the CHIMERE C1 simply called CHIMERE), one period (1-15 April with the greater changes in pollutant emissions during lockdown) and one main metrics for surface ozone (daily averaging). These aspects are more detailed in the second response to referee #1. The CHIMERE model setup C1 is chosen as it shows a clearly better agreement with observations. Unfortunately, this setup is based on a version of the CHIMERE code which is not compatible with the most recent emission inventory. Setting up CHIMERE C1 with such inventory would require a very heavy coding effort. On the other hand, the paper shows the influence of vertical mixing in the atmosphere for the two model setups is clearly larger than that from the year of the emission inventory. The C1 setup uses a vertical mixing scheme which provides simulations in better agreement with observations of both NO2 and O3 surface concentrations.

In addition, the use 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.

Clarified. Additional results provided in the RM (Table 2) show that IASI+GOME2 retrievals of LMT ozone provides overall consistency with both daily averages and MDA8 (maximum daily average over 8h) of surface ozone. Daily averages of surface ozone show the same variability of IASI+GOME2 data and an average difference (or shift) of about – 8 ppb (being satellite-derived concentrations lower). On the other hand, MDA8 surface ozone concentrations present a variability 20 % larger than IASI+GOME2 retrievals, a larger average difference in absolute values of – 12 ppb and a slightly higher correlation. This last might be associated with the larger variability of MDA8 data. Regional positive and negative anomalies between 2020 and 2019 over Europe are very similar for both MDA8 and daily averages (see below). We have chosen to avoid redundancy and only show results in terms of daily averages of surface ozone (whose variability and average concentrations are closer to the satellite data).

We have added the following clarification in the RM (lines 195-197) "*Surface MDA8 concentrations are closely linked with the daily maximum that occurs within the mixing boundary layer. These values show larger variability than satellite data and their average values present greater differences than with respect to surface daily averages. Therefore, these last ones are used for comparisons with IASI+GOME2.*"

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 at 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.

Agreed and two new figures added. We agree that the variability of ozone at the free troposphere may also be an important factor influencing near surface ozone, while being modulated by vertical mixing in the troposphere. For assessing its influence on the ozone anomaly between 2020 and 2019, we have added a new figure in the RM showing IASI+GOME2 measurements of ozone anomalies at the upper troposphere and at the stratosphere, the tropopause height and geopotential heights and winds. These new elements suggest that near-surface ozone is likely influenced by a rather large-scale reduction of ozone in the free troposphere in 2020 as compared to 2019. This reduction is partly associated with a large-scale reduction of ozone precursor emissions over the northern hemisphere (linked to lockdowns in numerous countries during that period) in

consistency with Steinbrecht et al. (2021) and over northern Europe also due to less abundant stratospheric ozone.
The influence of meteorological conditions was discussed in the original manuscript and the RM extends further these discussions. The enhanced anticyclonic conditions in 2020 with respect to 2019 are particularly seen north of 44°N by increased geopotential heights and lower windspeeds at 850 hPa. This situation favor subsidence and thus vertical advection of airmasses from the free troposphere down to the atmospheric boundary layer. As remarked by referee #2, two distinct behaviors are clearly observed over southern Europe (particularly the Iberian Peninsula) and other European countries further north, respectively corresponding to reductions and enhancements of surface ozone in 2020 as compared to 2019. This is shown in Figure 3b of the RM in terms of ozone anomalies simulated in 2020 and 2019 with the same emission inventory. This reduction of surface ozone over the Iberian Peninsula is probably linked to a clear reduction of insolation (enhanced cloudiness). However, this is likely limited by the effect of enhanced surface temperatures and lower windspeeds that would favor ozone production and by inhibited turbulent vertical mixing associated with less deep boundary layer heights. Inversely, the ozone enhancement in Central Europe is associated with a prevailing enhancement of photochemical production of ozone in clearer sky conditions, higher temperatures and lower windspeeds. Higher boundary layer heights in Central Europe are expected to enhance turbulent vertical mixing, thus reducing surface concentrations, and limiting the net enhancements of these concentrations.

These remarks are provided in the RM as (lines 434-465) : "*Other factors significantly affecting simulated concentrations of ozone and its precursors are clearly linked to the meteorological fields used by the model. This is shown in terms of changes on 2020 with respect to 2019 of ozone photolysis rates, surface temperatures and winds, and mixing boundary layer heights used by CHIMERE (Figure 7). Two distinct behaviors are clearly observed over the continent north of 44°N and over the Iberian Peninsula. North of 44°N, anticyclonic conditions prevailing in 2020 induced clearer sky conditions (thus enhancements of ozone photolysis rates), higher surface temperatures and lower windspeeds, which clearly favor photochemical production of ozone. This explains the frank positive anomaly of surface ozone over this region visibly simulated by CHIMERE, accounting (Fig. 3a) or not (Fig. 4a) for the emission changes during the lockdown. Over the Iberian Peninsula, reduced ozone photolysis rates (Fig. 6a) associated with enhanced cloudiness in 2020 is likely at the origin of the meteorology-associated decrease of ozone concentrations (Fig. 3b). However, other meteorological conditions likely produce the opposite effect: enhanced surface temperatures and lower windspeeds in 2020 are expected to favor ozone production and shallower mixing boundary layers to inhibit turbulent vertical dilution of ozone, thus inducing a relative enhancement of surface ozone concentrations in 2020. These effects are expected to compensate between them,* *explaining the moderate reduction of ozone simulated by CHIMERE over this region (-2.4% for the southwestern region in Table 5).*

Furthermore, the variability of ozone at the free troposphere may also be a significant factor influencing near surface ozone, depending on vertical mixing. The enhanced anticyclonic conditions in 2020 with respect to 2019 are particularly seen north of 44°N by increased geopotential heights and lower windspeeds at 850 hPa (Fig. 8a). This situation favor subsidence and thus vertical advection of airmasses from the free troposphere down to the atmospheric boundary layer. This is less clearly noted over the Iberian Peninsula and Eastern Mediterranean, where a transition between lower and higher geopotential heights is seen (Fig. 8a). Ozone anomalies at the upper troposphere are depicted by IASI+GOME2 retrievals between 6 and 12 km in Figure 8c. They mainly reveal an overall reduction of ozone concentrations in 2020 with respect to 2019, particularly over the North Sea and the Central Mediterranean. This is probably related with the large-scale reduction of free tropospheric ozone in 2020 observed by Steinbrecht et al. (2021), mainly related with the lockdowns-associated drop of precursor emissions over the northern hemisphere. Downward mixing of these ozone poorer airmasses probably contributes to the large-scale reduction of ozone observed at the LMT by IASI+GOME2 and its negative shift with respect to surface concentrations (Figs. 1 and 2a). Indeed, the only geographically coincident patterns observed both at the LMT and the upper Troposphere are the ozone reductions of ozone over the Mediterranean and the North Sea.

At the upper troposphere, a near zero variation is observed over North-eastern Europe and an ozone enhancement over Western Iberian Peninsula (Fig. 8c). This last one is probably associated with coincident lower tropopause heights (Fig. 8b), thus with a relatively larger contribution of stratospheric ozone. Over the North Sea, the reduction of upper tropospheric ozone at 6-12 km of altitude is strengthened by a depletion of stratospheric ozone occurring in 2020 (see in Fig. 8d as ozone anomalies with respect to 2019)."

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 NOx titration and dry deposition on ozone concentrations.

Clarified. We appreciate this suggestion. We have remarked a good consistency between IASI+GOME2 data of LMT ozone and both MDA8 and daily averages of surface ozone. We have preferred to use daily averages of ozone concentrations as they include the mi-day variations and show a better match with IASI+GOME2 satellite retrievals of LMT ozone, in terms of variability and mean values. This aspect is better discussed in the second answer to referee #2.

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.

Clarified. We agree that it could interesting to do such comparison with several years before 2020. However, we expect redundancy with respect to our current results comparing 2020 and 2019. This is shown by the comparison of our results and those from Ordonez et al., 2020, that use a 5-year period as reference. Both datasets show a clear consistency in terms of anomalies of surface ozone between 2020 and 2015-2019 and those between 2020 and 2019, differing by some large-scale shift in the background. Moreover, comparing 2020 with several years before would require a too heavy task of running the CHIMERE model and processing multispectral satellite data.

Comment from Amir Souri:

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:

1. 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 NO2 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(O3) 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)

Clarified and a figure added. We appreciate this interesting remark. For clarifying this aspect, we have added an additional figure in the RM (Fig. 7) presenting the meteorological conditions used by CHIMERE including ozone photolysis rates, surface temperatures and winds, and mixing boundary layer heights. Ozone photolysis rates are calculated according to cloud cover within the CHIMERE model package code. The new figure shows the changes in the meteorological conditions between 2020 and 2019 that can be related with the negative anomaly of surface ozone over the Iberian Peninsula simulated by CHIMERE in 2020 with respect to 2019 (using in both cases with the same standard emission inventories, Fig. 4b). This reduction in 2020 is clearly co-located with reduced ozone photolysis rates, which is associated with enhanced cloudiness. However, other meteorological conditions likely produce the opposite effect: enhanced surface temperatures and lower windspeeds in 2020 are expected to favor ozone production and shallower mixing boundary layer heights to inhibite turbulent vertical mixing thus inducing a relative enhancement of surface ozone concentrations in 2020. These effects are expected to compensate between them and thus CHIMERE simulations suggest that the anomaly of ozone surface concentration in 2020 associated with meteorological conditions is only moderate over the Iberian Peninsula (-2.4% for the southwestern region in Table 3).

This is added in the RM (lines 434-447) as *"Other factors significantly affecting simulated concentrations of ozone and its precursors are clearly linked to the meteorological fields used by the model. This is shown in terms of changes on 2020 with respect to 2019 of ozone photolysis rates, surface temperatures and winds, and mixing boundary layer heights used by CHIMERE (Figure 7). Two distinct behaviors are clearly observed over the continent north of 44°N and over the Iberian Peninsula. North of 44°N, anticyclonic conditions prevailing in 2020 induced clearer sky conditions (thus enhancements of ozone photolysis rates), higher surface temperatures and lower windspeeds, which clearly favor photochemical production of ozone. This explains the frank positive anomaly of surface ozone over this region visibly simulated by CHIMERE, accounting (Fig. 3a) or not (Fig. 4a) for the emission changes during the lockdown. Over the Iberian Peninsula, reduced ozone photolysis rates (Fig. 6a) associated with enhanced cloudiness in 2020 is likely at the origin of the meteorology-associated decrease of ozone concentrations (Fig. 3b). However, other meteorological conditions likely produce the opposite effect: enhanced surface temperatures and lower windspeeds in 2020 are expected to favor ozone production and shallower mixing boundary layers to inhibit turbulent vertical dilution of ozone, thus inducing a relative enhancement of surface ozone concentrations in 2020. These effects are expected to compensate between them, explaining the moderate reduction of ozone simulated by CHIMERE over this region (-2.4% for the southwestern region in Table 5)."*

2. Souri et al. [2021] and Barré et al. [2021] observed a large reduction from in-situ and TROPOMI tropospheric NO2 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 NO2 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.

Clarified. We have double-checked the validity of the in-situ surface dataset used in Figure 8 and flags indicate that they are valid measurements for background stations (of all categories urban, suburban, and rural). Positive NO2 anomalies in 2020 over Germany are particularly observed during the period 1-15 April (Figure 8) but are less marked for the average over the whole month of April (as analyzed by Souri et al. 2021 and Barré et al; 2021). They are also simulated by CHIMERE C2, at the surface (Fig. 8e of the original manuscript) and particularly evident for total columns (Figure 9d of the original manuscript). Using meteorology-only anomalies (with the same inventories), CHIMERE simulations show such anomalies strong positive anomalies very clearly (not shown). They are also observed in TROPOMI measurements of Souri et al. 2021 (figure 4), which show a full horizontal coverage. We think that the reason why this is not observed in the surface in situ figures from Souri et al. 2021 and Barré et al; 2021 is likely related to the period of analysis (the whole month of April for the previous studies and 1-15 April for the current) and also the choice of the in-situ stations which is probably much more restrictive is those publications and in the current one. Since the CHIMERE model and TROPOMI measurements also show positive anomalies of NO2 over Germany in 2020 as compared to 2019 and during the period 1-15 April, the surface in situ measurement we show are consistent with them.

We have added the following statement in the supplement of the RM “Additionally, surface measurement for some stations over Germany show a positive anomaly of NO2. This seems to be simulated by CHIMERE C2 at the surface and particularly evident for total columns, likely linked with meteorological conditions. These NO2 enhancements are particularly seen in the period 1-15 April, which could partly explain why they are not depicted by Souri et al. (2021) and Barré et al, (2021) for the whole month of April (as well as the particular choice of the in-situ stations for each of these studies).”.

Comment from Daniel Potts:

Very interesting study. Our study, https://doi.org/10.1088/1748-9326/abde5d, where TROPOMI, in situ surface measurements and the chemical transport model GEOS-Chem were used to investigate lockdown emission changes of NO2, Ozone and PM2.5 over the UK should be cited here as relevant work (introductory section lines 65-80).

Done.