

Interactive comment on “Estimating lockdown induced European NO₂ changes” by Jérôme Barré et al.

Anonymous Referee #1

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Barré et al. (2020) describes estimates of the magnitude of NO₂ reduction that can be attributed to COVID-19 lockdown measures in Europe in 2020. The paper is highly relevant given the continuation of COVID-19 and associated restrictions, interesting and well written. The argument for needing meteorological normalisation is, I think, sound, but the introduction would benefit from some more discussion why this is so, focussing on the role of different meteorological variables on NO₂ concentration. The methodology employed to achieve the meteorological normalisation is sound for surface estimates. I think it would make much more sense to use surface mixing ratio estimates from TROPOMI rather than column values, given the fact that (a) the paper focuses on urban air quality where exposure by large populations is at the surface level and (b) surface temperature and wind are key predictor variables. The CAMS mod-

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elling section is well presented, but so far lacks punch. It would substantially benefit the paper to discuss the emission reductions further as this would demonstrate not just that the NO₂ did reduce, but why. Discussing the sectors responsible for emission reduction and therefore NO₂ concentration reduction, and whether this is consistent across Europe, would provide the chemistry focus required for Atmospheric Chemistry and Physics.

Overall, I would recommend publication once these comments are addressed. Expanded comments on the above discussion are made below, followed by minor technical comments.

Expanded Comments:

1. The introduction needs to provide the reader with some more context on why studying NO₂ during the lockdown is important. I think a paragraph could be included to this end, outlining the unique nature of this real world emissions-reduction experiment and its potential to help us understand potential broadscale impacts of future pollution reduction measures.
2. The introduction highlights that considering meteorology is important for NO₂ prediction – I agree, but the introduction would benefit from a little more context on why this is the case. Just a couple of sentences are necessary on, e.g., boundary layer heights and NO:NO₂ temperature dependent ratios that make this point clear.
3. Figure 2: Temperature is a really important factor for the NO_x partitioning and warm temperature anomalies are highlighted in the text for early 2020. Therefore, I think Fig. 2 would benefit from showing the distribution of temperature as well as PBLH and wind speed.
4. Line 180-182: Please consider this sentence: “This illustrates that such “before-during” type of satellite comparisons is misleading and unfit for assessing the effects of COVID-19 lockdown because it is very sensitive to seasonal variations of weather

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regimes and emissions.”

What you have shown, to this point in the paper, is that different baselines provide different results for the ‘lockdown NO₂ change’ and that the weather was different. You have not strictly proven, yet, the link between the two. At this point in the paper, you either need to prove the causal link with data, provide references for the statement ‘because it is very sensitive...’ or mute the sentence to something like ‘This illustrates that such ‘before-during’ satellite comparisons clearly provide very different results as to the effect of lockdown on European NO₂. This led us to investigate weather considering meteorology may provide a more consistent picture’.

5. Why did you use TROPOMI column NO₂ rather than surface mixing ratios? The mixing ratio can be determined from the NO₂ tropospheric vertical profile. This would seem to me more relevant for urban air quality than tropospheric column values, and would provide consistency with surface observations. In addition, this would make your surface meteorological predictor values much more relevant, at the moment (effectively) surface temperature and wind are being used to predict the whole column.

6. Why were NO₂ modelled concentrations in the predictor variables? I think to demonstrate the importance of meteorological normalisation, you should show that the GBM gives good prediction independent of NO₂ concentration estimates.

7. Table 3: Please clarify whether the average changes in this table are means or medians, ideally consistent across all comparisons.

8. Line 283-284: “Using the last three years is long enough to capture weather variability at each site, but not too long with regards to long-term reduction of NO₂ happening as a result of policy measures across Europe” – perhaps the authors could clarify this rather vague statement by indicating expected (or citing known) NO₂ trends across Europe as a result of policy measures. In the previous paragraph, the authors note that their method underestimates NO₂ in the pre-lockdown period by 8 %, could this be partly due to decreasing NO₂ trends driven by policy or emissions?

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9. I think the strength of the paper would be improved by discussing in more detail what led to the NO₂ decrease – the modelling section seems to offer this opportunity. Was it reduction in industry, aviation, road transport, all of the above equally or something else that was primarily responsible for the NO₂ change, and was this consistent across Europe?

10. It would be interesting to discuss how this kind of weather-normalisation ‘business as usual’ prediction could be implemented for air quality forecasting, in addition to event/emission change analysis.

Technical comments:

1. Be consistent with subscripting of “x” in NO_x

2. Line 72: please revise the first sentence of the paragraph, ‘very changing’ is poor grammar – perhaps ‘highly variable’ would be better

3. I’m not sure Table 1 is necessary, it is so small and the information is clearly stated in the text anyway.

4. Line 123: strange font difference in 0.1x0.1o

5. Figure 3 (and subsequent similar figures): Given that you consider urban areas down to 0.5 million inhabitants, I recommend adding some more circles to your population circle-size legend (maybe 0.5 m, 1 m, 2 m, 4 m, 8 m)

6. Figure 3: (and subsequent similar figures) Subscripts please on the NO₂ in the colour bar label

7. Figure 3 (and subsequent similar figures): please just clarify, the % change is relative to each baseline scenario? – I suggest including this clarification in the figure caption.

8. Table 2/line 245: are the outliers included in the statistics presented in table 2? If they’re included, might they explain the significant RMSE?

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9. Lines 263-265: Perhaps my personal choice, but I would write “X % reduction” not the double negative “-X% reduction”. This would also be consistent with the way it is written in the paragraph starting Line 347.
10. Line 307: should be “...measurements do not directly translate to...”
11. Line 323: model rather than models
12. Line 338: I’m curious if there is a metric which could help determine the stringency of lockdown measures in different countries? At the moment, knowledge of the scale of lockdowns and COVID-19 consequences are fresh in our minds, but people may not have a feel for that reading this in the future. I think some discussion of what constitutes a ‘more stringent’ vs ‘less stringent’ lockdown is warranted.

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