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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Changes in black carbon emissions over Europe due to COVID-19 lockdowns" *by* Nikolaos Evangeliou et al.

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Anonymous Referee #2 Received and published: 4 December 2020 In this work the changes in black carbon emissions (BC) due to the restrictions imposed in response to the global pandemic are estimated through inverse modeling. Absorp- tion measurements from the Aerosol, Clouds and Trace Gases Research Infrastructure (ACTRIS) and the networks European Monitoring and Evaluation Program (EMEP) are used in a Bayesian inversion system to estimate weekly emissions over Europe for a given a priori emission inventory. Estimated posterior BC emissions are compared against dependent observations first to examine the optimal prior inventory to con- duct the study. Then, concentrations computed with the estimated emissions were compared against two ARCTIS stations not considered in the inversion and therefore representing inde-

pendent observations. Additionally, comparison was also conducted against reanalysis dataset of BC from MERRA-2 (Modern-Era Retrospective Analysis for Research and Applications Version 2). The results of this study suggest that BC emissions during the lockdown period were 10% smaller than for the same period of the previous five years. In addition, emissions decreased 10% over Europe when compared to the period before the lockdown. Although the study and the results are interesting, I find the article not well written. Before I can recommend it for publication it needs to be improved in terms of clarity and analysis. In what follows I'll present the general comments supporting my evaluation.

Response: We appreciate reviewer's effort to improve the manuscript.

General comments 1. Section 2 (Methods) is a description of different pieces used in the study without explaining the links between them and how they contribute to achieve the goal. The methodology was made clear while reading the Results. This should not happen; the full methodology should be clear to the reader while reading section 2. I suggest that an introductory paragraph should be included at the beginning of section 2 explaining the general methodology applied in the study and pointing to the individual sections where more information is provided. I believe it would also contribute if the inversion system is presented after all other elements used in the system have been presented.

Response: As suggested by the reviewer, we have added a full paragraph in the beginning of Methods section that describes in detail how each methodology and dataset used in the present is connected in conducting the assessment of the COVID-19 impact on BC emissions over Europe (L 109-126, p 5).

2. The authors provide an incomplete description of the inversion system. It is not clear from the text what the size of the state vector is? Are the emissions estimated for each grid illustrated in Figure 1 or are these grids aggregated to larger regions? Furthermore, how are the B and R matrices defined? Are the uncertainties of the prior

considered not to be spatially correlated and thus the B matrix defined as a diagonal matrix? What about the R matrix is it also considered to be a diagonal matrix? The authors describe how they validate the chosen values but do not present the values used in either matrix. This is relevant information that should be provided by the authors. If it is provided in another study it should nevertheless be summarized in the paper.

Response: The Bayesian inversion algorithm that was used in the present study has been previously used in several applications (see references therein). We thought that giving all the details of the method would be a repetition, since we give the main references that describe it (Thompson and Stohl,2014; Thompson et al., 2015; Stohl et al., 2010). However, as pointed by the reviewer, we were obviously mistaken and, therefore, have amended this part and now give detailed information on the several definitions used in section 2.3 (Track changes circa L 175 and below). Stohl, A., Kim, J., Li, S., O'Doherty, S., Salameh, P. K., Saito, T., Vollmer, M. K., Wan, D., Yao, B., Yokouchi, Y. and Zhou, L. X.: Hydrochlorofluorocarbon and hydrofluorocarbon emissions in East Asia determined by inverse modeling, Atmos. Chem. Phys. Discuss., 10(2), 2089–2129, doi:10.5194/acpd-10-2089-2010, 2010. Thompson, R. L. and Stohl, A.: FLEXINVERT: An atmospheric Bayesian inversion framework for determining surface fluxes of trace species using an optimized grid, Geosci. Model Dev., 7(5), 2223–2242, doi:10.5194/gmd-7-2223-2014, 2014. Thompson, R. L., Stohl, A., Zhou, L. X., Dlugokencky, E., Fukuyama, Y., Tohjima, Y., Kim, S. Y., Lee, H., Nisbet, E. G., Fisher, R. E., Lowry, D., Weiss, R. F., Prinn, R. G., O'Doherty, S., Young, D. and White, J. W. C.: Methane emissions in East Asia for 2000-2011 estimated using an atmospheric Bayesian inversion, J. Geophys. Res. Atmos., 120(9), 4352–4369, doi:10.1002/2014JD022394, 2015

3. The authors use 6 emission inventories as prior for the inversion and from those one (ECLIPSE v6) is chosen to conduct the study. Most, if not all, of these inventories do not provide yearly estimates but for larger intervals. ECLIPSE for instance provides estimates every 5 years. How do the authors deal with this? Do they linearly interpolate

between the available years to have yearly estimates? Furthermore, while 2015 in ECLIPSE correspond to an actual estimate, the emission for 2020 correspond to a projection. None of the priors for 2020 are an actual estimate but a projection of some kind. Do the authors attribute larger uncertainties to the prior of 2020 because of this? In other words is the B matrix defined differently for 2020? If not, why? What is the impact on the results due to this difference between emissions before 2020 and the projected emissions for 2020?

Response: As we demonstrate in the following figure (Figure 1), the anthropogenic emissions of BC in Europe between January-April 2015 and January-April 2020 differ by 3.4% only, and, therefore, we do not interpolate emissions in the years from 2015 to 2020. What we do is that we only add biomass burning emissions from GFED4 every year, as those have a spatiotemporal impact to the total emissions of BC. As regards to the uncertainty of the prior emissions, we do not consider inversions as a black box to give random uncertainty to the prior source, and we rather treat the errors very carefully. For example, inappropriate chosen prior uncertainties can cause spurious negative emissions. Especially if prior uncertainties are too large, there may be a problem of over-fitting the observations, where the observation uncertainty is smaller than the model representation error, such that the model cannot even with the "true" emissions possibly fit the observations within the uncertainty range. To choose realistic uncertainties, we use reduced chi-square value (the value of the cost function at minimum divided by number of observations and divided by 2) which should be around 1. The probability density function (pdf) of prior and posterior model-observation differences (i.e. BC concentrations) is compared with the pdf where 1SD is given by the measurement error. If all values have been well chosen, the posterior model-observation differences should fit (very) approximately the pdf

4. Different set of stations were used for the period 2015-2019 and 2020. As the authors state in sections 3.3, changing observations can have drastic impact on the posterior emissions. Why were different stations used for the two periods? How many

stations would be available for the inversion if only stations with data for both periods were used? How do the results change if only these stations are used in the inversion? The authors should discuss how the fact of using different set of stations impact the results. Ideally, the authors should demonstrate that the results are not impacted by using different set of observations between both periods.

Response: Unfortunately, it was not possible to have measurements from the same stations every year for different reasons (e.g. instrument failures and other deficiencies or simple late or no reporting of measurements). As explained in the Methods section, the different station measurements result in different average source-receptor matrices (SRMs) and, in turn, in different aggregated grid. To minimize the impact of using slightly different station measurements (due to different data availability), we used exactly the same grid for all inversions, as otherwise differences in the posterior emissions can partly be due to different grid geometries. Since it is not possible to use exactly the same stations for the 2015-2019 inversions as for the 2020 (due to different station data availability), we prove that the difference in the posterior is rather small when using slightly different station data. For this, we performed an inversion using the stations of our main inversion in 2020 (Table 1) and another after removing 4 stations in Spain and Italy (ES0018G, ES0020U, ES0022R and IT0004R, see Table 1). The spatial resolution of the stations is shown in the attached Figure 2. In this way, we used 17 stations in our standard inversion set-up and 13 (17-4) stations. This fraction of different stations is similar to this of the 2015-2019 set-up versus the 2020 inversion set-up (Figure 2). We found that the difference in posterior emissions of BC is tiny and does not affect the final result more than 1% (191 kt versus 189 kt). The posterior emissions in the 2 different set-ups are shown in Figure 3.

Specific comments Lines 50-52: The two periods considered in the analysis should be included.

Response: We do not understand what periods the reviewer means here. One period is considered (January to March 2020). We just give an indication about what

happened in the previous 5 years using the same methodology (inverse modelling combined relevant measurements). If the reviewer insists in this change with a more precise comment, we will change this in a next step.

Lines 59-62: The formulation of this sentence can be improved. The sentence is started with “Except” and then continued with “also”. What is the point of using “Except”?

Response: We have now tried to improve this sentence. All we tried to do here with the use of “except” was to link the apparent decrease in the 2020 emissions compared with previous years’ emissions with the decrease of the emissions when the period before and during lockdown was compared.

Line 74: Is “distancing” missing after “strict social”.

Response: No, there is nothing missing here: “. . . strict social, travel and working restrictions . . .”. “Strict social” is linked to “restrictions”.

Line 120: Acronym MAC needs to be defined.

Response: Thanks to the reviewer, the acronym is now explained where it appears first in the text (see Track Changes).

Lines 148-149: The reference corresponding to the study mentioned on this sentence should be provided. Furthermore, this work also examines the impact on the emissions of the strict restrictions between the lockdown period and before, so how do the results presented in this study differ or complement the other study? Additionally, how do the estimated emissions compare between both studies.

Response: We have added a sentence there to explain that the analysis was performed for the 5 years prior to 2020.

Line 178: the weighted sum of squares of what? Please clarify.

Response: Sentence has been corrected (see Track Changes).

Line 182: Where is this number coming from? Are those the total number of observations for the period 2015 to 2020?

Response: This is the total number of observations from the 17 stations. If all stations do not have gaps, each station would have 8 measurements per day (we use 3h averaged measurements of aerosol absorption) for 31 (Jan) + 29 (Feb) + 31 (Mar) + 30 Apr = 121 days. This gives a total 968 measurements per station for the period January to April 2020. We have 15 dependent stations (plus 2 stations that we used for validation), so 14520 measurements in total. However, due to the gaps in the measurements due to screening or instrument failure, we always have less measurements. In our case, we had 12538 in total.

Line 267: Is the B matrix changed for each test according to the prior used or are the uncertainties of the prior emissions considered the same for all prior emissions? If not, how does the ïA ÌÍcËĞïAšïAa ÌĘstatistic change for each test with a different prior?

Response: As we mention in section 2.3, we set uncertainties in each inversion in a way that we always get a x2 value equal to unity. Once we find the optimal uncertainties that result in x2=1, we use the same settings for each inversion with different priors. This gives consistency in our study, in the sense that we always give the same weight to observations and/or prior emissions. This is why, the resulting x2 values range between 0.8-1.5.

Line 283: Although country emissions might have decreased, figure 4 also shows an increase of emissions in center-south France. Also, please explain the geometry of the region with increased emissions over Poland. How can these straight lines be explained? What does it say about the inversion system?

Response: In Methods section, we explain that the different station measurements result in different average source-receptor matrices (SRMs) and, in turn, in different aggregated grids. To minimize the impact of using different station measurements (due to different data availability), we used exactly the same grid for the 2015-2019 and the

2020 inversions (see Figure 1), as otherwise differences in the posterior emissions can partly be due to different grid geometries. This approach has some consequences. For instance, it results to regions with a high resolution grid-cells, despite that no measurements are available there (e.g., Poland). If we also check the prior emissions (Figure 3), we conclude that the high-resolution grid in Poland is more strongly correlated to emission density than to emission sensitivity from the stations. This gives very much weight to the high emissions in Eastern Europe and less to the fact that emission sensitivities, there, are already quite low. And of course, there are somewhat more stations close to this area in 2015 than in 2020. We must admit that the rather poor data density for weekly timestep inversions push the method to the limit and one should be careful not to overinterpret the emission changes at smaller scales. We have added a sentence in the Conclusions section to make this limitation clear to the readers.

Line 289: The authors should clearly state the difference in the number of stations used for the period 2015-2019 and the year 2020.

Response: We now explain this in the beginning of section 2.3 (see Track Changes).

Lines 302-303: Does this mean that the inversion was also conducted for each year since 2015 using the same stations as used in 2020? Doesn't this contradict what is said at the beginning of this section. Please clarify.

Response: The reviewer has a good point here. This sentence causes a misunderstanding; hence, we have reformulated it (p 13, Track Changes).

Line 353: I suppose that by "before" the authros refer to previous studies presented in the provided references. What do these results show? Is the impact associated to these other sources small compared to MAC? The authors should summarize the impact of the other sources on the inversion compared to the results obtained in this study?

Response: We have corrected this sentence as "...have been studied before and sig-

nificantly smaller than the sources of uncertainty that are considered here (Evangeliou et al., 2018; Grythe et al., 2017)." See p.15 (Track Changes).

Line 354-355: Which period is considered in this analysis? Is it the entire period (2015-2020)? According to Figure 1, the network used changes between 2020 and the rest of the period. Given the uncertainty obtained it should be clarified which period is used.

Response: In this paper, we focus on the emissions of BC in 2020 and we only use emissions from previous years as an indication of how patterns in the 2020 emissions changed due to COVID-19 outbreak. Thus, the sensitivity of the emissions to the use of different MAC values for the conversion of absorption to eBC is only done for the 2020 emissions. We now give this detail in the relevant paragraph (see Track Changes in p14-15).

Line 356: How much does "increases dramatically" represent? Please reformulate.

Response: We added that the uncertainty can reach up to 100% far the observations (p 15, Track Changes).

Line 357: I do not think that "Accordingly" is the correct word to be used here. I suppose the tests were conducted independently by fixing the other parameters (ie, fixed emissions when testing uncertainty due to MAC).

Response: We have removed the word "accordingly" from the sentence. The reviewer has a good point here (p 15, Track Changes).

Line 363: Why were only two stations left out for the independent validation? Furthermore, both stations are located close to a station actually used in the inversion and since uncertainty "increases dramatically" far from the observations, using two "nearby" stations for validation represents the "best scenario" case. How independent are these two stations actually? How does the performance changes if isolated independent stations are used for validation?

Response: Since these observations were not included in the inversion to optimize

emissions, then they are independent, by definition. The reason why we chose these 2 stations for our inversions is explained in p. 15 ("Due to the higher measurement station density . . . that lack observations."). For your consideration, we had 17 stations available, with a poor coverage in Europe; 2 of them are practically adjacent to others. This means that if we sacrificed stations for independent validation at the edges of our domain (i.e., in Italy, Norway or Spain) the surface area without measurements in our grid would increase. We think that it was the wisest solution to exclude stations where station density is high. ; Line 367, Figure 7: Since the uncertainty is not included in the analysis below, I would suggest to remove the illustration from the error (MAC & Prior) in order to make the figure easier to read. In it's present state, it is difficult to identify the different lines.

Response: We believe it is important to show that posterior concentrations are associated with large uncertainties as shown in Figure 7. The better agreement of the posterior concentrations is proved by the statistics we show in the figure using the prior and posterior concentrations against observations. To improve the figure according to the reviewer's suggesting we have used more transparent colors for the uncertainties in posterior concentrations (see new Figure 7).

Lines 384-385: I have to admit I'm confused here. Figure 3 in the supplement shows that in all countries (except Scandinavia), MERRA-2 concentrations during lockdown in 2020 were not the lowest ones. How is this compatible with saying that and improved air quality was seen over Europe during lockdown based on MERRA-2 data?

Response: We have corrected this sentence to make it clear to the reader. MERRA concentrations generally decreased in Europe (as seen in the relevant figure at the bottom) since January 2020, and only after the lockdown was over, we saw a tendency to increase again. In addition, we also see the episodic (usually for no more than 1 week) peaks during the lockdown, that we think they are due to residential combustion.

Figure 8c: In figure caption please indicate the color used for each station.

Response: We have followed reviewer's suggestion and corrected the caption (see text with Track Changes).

Lines 414-421: I do not entirely agree with this analysis, Although the emissions were reduced in most selected countries, for France emissions for 2016 and 2018 were smaller than for 2020, at least for the first half of the lockdown period. Similarly, for Italy emissions in 2015 and 2017 were lower than for 2020. This is consistent with what is said at the beginning of section 4, where it is stated that increase light absorption measurement are observed at the beginning of the lockdown due to increased residential emission. Isn't it contradictory what is stated at the end of section 4 with what is said a the beginning of the same section?

Response: In the beginning of section 4, we try to link the patterns of the 2020 emissions of BC with what is seen in MERRA-2 assimilated concentrations. At the end, of the section, we compare the 2020 emissions during the lockdown with what is seen for the 5 previous years. We have explained previously that difference in emissions during the whole study period for the different years cannot be attributed to the lockdown and is rather due to reported measurements that could be increased or decreased from year to year. However, the differences during the lockdown from the period before is what it is of interest here, as this shows the real impact of the lockdown. In 2020, a clear decrease was pronounced opposite or larger than in the previous years. We have tried to make this part more consistent in the text.

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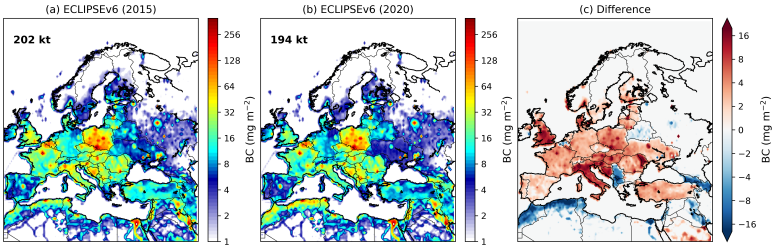

Fig. 1. Change in BC emissions of ECLIPSEv6 between 2015 and 2020 in the European inversion domain.

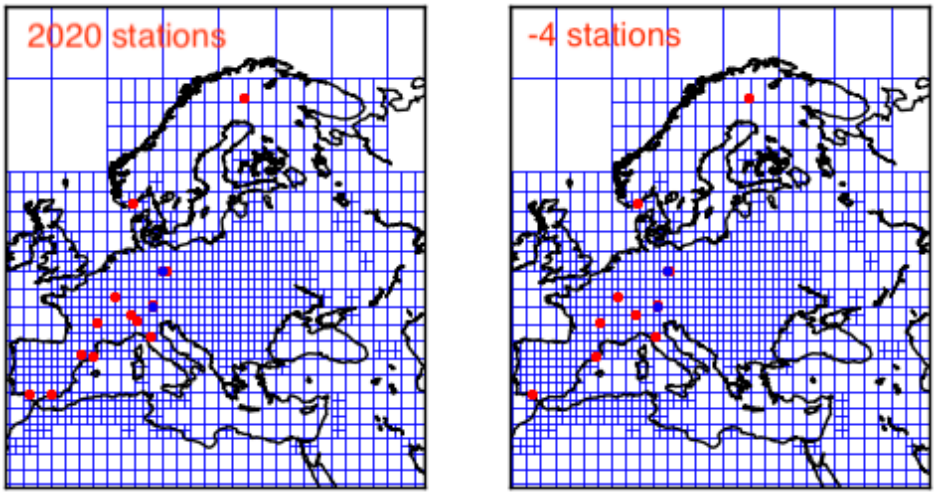

Fig. 2. Spatial resolution of the dependent stations used in the inversion and after removing 4 stations from central and southern Europe for testing the inversion results.

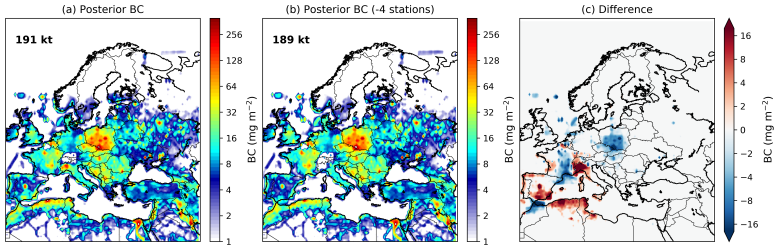

Fig. 3. Difference in the posterior emissions after using all stations (as in the 2020 inversion set-up) and after removing 4 stations from central and south Europe.