We would like to thank reviewer #2 for the positive assessment, constructive criticism and helpful comments. Below we reply to the raised issues point by point, where the comments from the reviewer are typed italic.

General comments:

This manuscript describes a unique data set of anthropogenic NOx point emissions derived from nearly two years of TROPOMI data, explains the methods used to create it, and discusses its limitations and uncertainties.

The data set presented is limited in several ways: it is incomplete because of the method used, it is only partly quantitative as it suffers from a known but poorly constrained low bias in the current TROPOMI lv2 data, and it is potentially biased because the assumption has to be made, that emissions are constant over the time period used as input. Therefore, this data set should not be used as emission input for CTM runs or for the verification of reported emissions.

In spite of these limitations, this is a very valuable data set as it provides observational evidence for the position and approximate strength of a large number of NOx point emissions sources worldwide. It is a unique data set and fully independent of other data sets of NOx point sources and therefore very well suited for semi-quantitative verification of other inventories, in particular if up-to-date information is needed.

The data set has the potential to be improved by future updates when more TROPOMI data is available for better statistics, and when improved TROPOMI lv2 data versions have been released for reduction of uncertainties.

The manuscript describes the generation of the data set in a clear and sufficiently detailed way, highlighting the differences to the scientific study describing the original method. The uncertainties and limitations are discussed in detail and give users a good idea of how to use the data, and which caveats to consider.

Detailed comments:

The authors introduce a number of ad hoc abbreviations such as PP for power plants, which in my opinion make the text less accessible and do not really help with length. I would suggest using them only in the figures.

We now write out "power plants" and "point source" throughout the document and skip the respective abbreviations.

Page 2, line 7 – 9: I am confused by this description of AMF and Averaging Kernel. To me, this should be Jacobians or box AMFs instead of Averaging Kernels as the latter are the box AMFs divided by the total AMF derived for the a priori profile. Please reconsider.

For the introduction, we simplified this sentence into
".. and applying the so-called air mass factor (AMF) that depends on the NO2 profile shape as well as on viewing geometry, surface albedo, aerosols, and particularly on clouds."
In addition, we extended section 3.7 ("AMF correction") by introducing box-AMFs.

Page 2, line 31: "flux increases discontinuously" – I find this formulation a bit awkward as a discrete data set is discontinuous by definition

We modified this phrase to "the NOx flux increases abruptly".

Page 3, line 18: TROPOMI spatial resolution has already been introduced

This is indeed some redundancy. Still we think that it is important to give the resolution already in the introduction, but would like to keep it in the section about TROPOMI as well.

Page 3, line 27: Mention native resolution of model data

We revised this paragraph to
"Until August 2019, ERA-Interim data are used with a truncation at T255, corresponding to ~0.7° resolution. Since September 2019, ERA-5 data are used with a truncation at T639, corresponding to ~0.3° resolution."

Page 4: Mention altitude of O3 extraction

As the divergence is sensitive for the added NOx at the source, the relevant NOx/NO2 ratio is that close to ground. We thus took O3 concentrations from the lowest model layer and added this information to the revised manuscript.

Page 5: More details on the gridding would be useful: I assume it is a 2d linear interpolation?

Yes. We added this information to the manuscript.

Is there a risk of low bias using this approach? Is this approach reducing the spatial resolution of the original data? Mention that is done by orbit (this is explained later but I asked myself here if this is done daily, monthly or on orbits)

We thank the reviewer for raising these issues. We have checked exemplarily how far the gridding by linear interpolation might introduce a low bias. For the power plant plume of PP9 northeast of Riyadh on 17 December 2017 (Fig. 1a in Beirle et al., 2019), the average plume VCD from interpolation yields almost the same value than for conventional gridding (1% lower). For the peak maximum, however, which is more relevant for the divergence than the mean, interpolation is 6% lower. We have added this aspect to section 5.2 (Uncertainties and accuracy).
As we use a grid with high resolution, we see no loss of the original data resolution.
In the revised manuscript, we clearly state that all processing steps are done by orbit, and hope that confusion with monthly means is now avoided by shifting the description of the selection mask to the Supplement (see also reply to reviewer #1).
One further aspect related to the 2D-interpolation, which we noticed during the case study above, is the occurrence of gaps, e.g. due to clouds. Missing values have to be set to NaN, as already stated in section 3.2. This results in gaps in the gridded product that are far larger than for conventional gridding. This is probably also one reason for the poor statistics over e.g. Germany. We added this aspect to section 3.2.

Page 7: Is there a risk that the requirement for a high dynamic range excludes some point sources with little variation in wind direction?

For a point source with persistent wind, this indeed might principally happen. However, already slight variations in wind speed or direction are sufficient to cause high variability of the column density around the source at least for some grid pixels.

From the resulting selection mask, which includes all potentially stationary sources plus 1° around, we don't see indications that significant NOx sources are missing.
Still, we might choose a less strict selection mask for a future update of the catalog.

Page 9: The upscaling to NOx would benefit from a bit more discussion – NO2 columns are inserted where NO2 concentrations should be used, and in particular when [O3] is taken at varying altitudes, this raises some questions about the validity of the approach. For high altitude regions, the factor between NO2 column and NO2 concentration will also change – does this have an effect?

We have modified the respective paragraph as follows, with additions in bold type:
"**Near-surface** Ozone mixing ratios are taken from a climatology based on the ESCiMo model simulation and converted into concentrations based on T and p from ECMWF.
The derived values for L represent conditions for surface-near pollution. For background NOx in the upper troposphere, the partitioning would be shifted towards NO.
However, any additive background is automatically removed by the calculation of the divergence.
Thus, the partitioning derived for near-surface concentrations is appropriate also for correcting the added column caused by a point source. "

Page 10, line 22: Is this really the best reference here – Eskes et al. introduced the column AK, not the AMF.

We revised this paragraph and added the term "box AMFs", plus a reference to Wagner et al., 2007:
Wagner, T., Burrows, J. P., Deutschmann, T., Dix, B., von Friedeburg, C., Frieß, U., Hendrick, F., Heue, K.-P., Irie, H., Iwabuchi, H., Kanaya, Y., Keller, J., McLinden, C. A., Oetjen, H., Palazzi, E., Petritoli, A., Platt, U., Postylyakov, O., Pukite, J., Richter, A., van Roozendael, M., Rozanov, A., Rozanov, V., Sinreich, R., Sanghavi, S., and Wittrock, F.: Comparison of box-air-mass-factors and radiances for Multiple-Axis Differential Optical Absorption Spectroscopy (MAX-DOAS) geometries calculated from different UV/visible radiative transfer models, Atmos. Chem. Phys., 7, 1809–1833, https://doi.org/10.5194/acp-7-1809-2007, 2007.

Page 11: I would suggest to already here identify x and y as latitude and longitude

x and y have been defined as distance along longitude/latitude in section 3.2.

Page 12, line 15: neglects of the => neglects the

Corrected.

Page 14, line 27: above > 30% => above 30%

Corrected.

Page 15: Maybe in India, there is a lack of distributed NOx emissions such as from traffic, helping to reduce the background levels

From Fig. 1 it can be seen that background levels of NO2 in India are lower than in China or Western Europe, but they are not really low. Thus we are reluctant to write about a "lack of distributed NOx emissions" over India.
One important difference between India and e.g. Germany is the strong seasonality. In India, the dry season provides very good observation conditions, while for Germany, the frequent occurrence of patchy clouds regularly cause gaps in the input data which are magnified by the 2D interpolation.
We have added this aspect to the revised manuscript.

Page 17, line 7: Ukraina => Ukraine

Corrected.

Page 22, line 9: I am not convinced that larger deviations of wind direction will be readily visible in the divergence as this is applied to the averaged flux where wind direction errors could cancel

We agree that errors would at least partly cancel for statistical deviations. We modified the sentence to
"Larger systematic errors in wind direction would cause visible artifacts in the divergence map and would thus be captured and removed by the check for negative divergence during candidate classification. In case of larger random effects, the artefacts of individual days would at least partly cancel in the mean flux. But the wind speed component in the actual wind direction would be significantly underestimated, as well as the resulting divergence."

Page 25, acknowledgements – isn't there a standard statement required when using EU Copernicus data?

We have used TROPOMI data provided by ESA and wind fields from ECMWF, as already mentioned in the acknowledgements. We have extended the acknowledgements and also added the doi for TROPOMI NO_2 data. However, we are not aware of a standard text required for the datasets we are using. If our acknowledgements are still insufficient, we would appreciate a concrete hint about what is missing.