Dear referee#1,
thank you very much for your review of our manuscript GMD-2019-07. After our short comment we would like to reply to your review in detail. In the following, referee comments are given in italics, our replies in normal font, and text passages which we included in the text are in bold.

This paper presents an analysis of simulations at various horizontal resolutions, with emissions at various resolutions, and different emissions inventories.
Reply: To be more precise, our analysis focuses on diagnosed ozone contributions and uncertainties of these contributions, which arise due to model limitations (e.g. resolution, parametrisations), limited resolution of emission inventories, and uncertainties of the emission inventories. To make this more clear we revised the manuscript at several points (see below) and add also an addition Section (Sect. 2.1) which discuss the source apportionment in more detail.

The research is technically sound, and the application of source tagging and attribution is well illustrated. However, the paper does not seem to have any new results. The models and tagging technique used have all been published previously. The majority of their conclusions confirm previous work. Their strongest conclusion seems to be that different emissions inventories making the largest difference in ozone simulations, which I think is well known, but they do not offer any assessment about which might be more accurate. If the authors feel they have more compelling results, then they should make them much clearer.

Reply: First of all thank you very much for honouring our work. Indeed our analysis is very technical and focuses on the impact of technical limitations of models on the results of source apportionment diagnostics. However, we do not agree with referee#2 that our manuscript does not show any new results. Clearly, the dependence of simulated ozone concentrations on the resolutions of model and emissions are well known (see p1l4f, p2l8ff of our manuscript), and where appropriate we cite previous literature. The focus of our manuscript, however, is not on simulated ozone concentrations but on diagnosed contributions to ozone. We are not aware of any previous publication, which investigates the impact of these factors on the results of a source apportionment (e.g. tagging) method. Further, we are not aware of any similar model system allowing for such an analysis, as it requires a consistent global-regional model chain applying the identical source-attribution method on the global and regional scale. Previous publications applying source attribution on the regional scale (e.g. Dunker et al., 2002; Li et al., 2012; Kwok et al., 2015; Valverde et al., 2016; Karamchandani et al., 2017) considered only the contributions as simulated by the regional model and are not able to attribute ozone transported from the stratosphere or across the lateral borders of the regional model domain to specific emission categories.

In addition, we would like to remark that publications in GMD are not primarily about presenting new scientific results. Publications in GMD are mainly to document model developments, document experimental set-ups of model simula-

tions, document evaluation of model systems, present model evaluation strategies and to present technical analyses of model systems. We think that our manuscript documents the influence of model and emission inventory resolutions on source attribution results. This is clearly important to asses source apportionment results and their related uncertainties, also for other model systems.

To make the importance of our study also for other modelling communities more clear, we revised especially the conclusion (and the abstract) as discussed below in more detail.

It is not apparent why the authors thought GMD was the best journal for this work. It does not seem to have any new model development, or even quantitative evaluation of the model.

Reply: The main goal of our manuscript is to analyse the impact of technical limitations (e.g. model and emission inventory resolutions and/or the applied model) on the simulated contributions to ozone. Accordingly, our research questions are rather technical and focus on the impact of differences due to model limitations and/or differences due to input data. This does not necessarily imply in new scientific results, but it yields certainly important new insights for other researchers in the same field. Therefore, we chose GMD instead of ACP as journal and choose "development and technical paper" as manuscript type. These type of manuscripts also includes: '[...] papers relating to technical aspects of running models and the reproducibility of results' (GMD website).

The paper reads very much like a technical report for MESSy users. For example, it would help the general reader if 'ONEMIS' was defined and explained on p.5.

Reply: Of course the paper should not read as a technical report to MESSy users. Even tough the specific results we discuss are only valid for the specific model system and set-up (as it is common for most model studies) the general conclusions (see next paragraph) are also important to other researchers using source apportionment methods in a variety of models (e.g. CMAQ, WRF). As discussed in the next paragraph, we revised the manuscript in such a way that the general findings, which are important for the whole community, will become more clear.
For the revised manuscript we have carefully checked the manuscript again and describe specifics of the MESSy world, which are not defined in detail. For your specific example of ONEMIS on p5l1f we write: 'Emissions of soil-NOx and biogenic isoprene (C5H8) are calculated by the MESSy submodel ONEMIS (Kerkweg et al, 2006), which uses the parametrisations of Yienger and Levy (1995) for soil-NOx, and Guenther et al. (1995) for C5H8.'

While I see no errors in this work, I feel significant revisions are required to

make it suitable for publication. The paper should emphasize new results, not the confirmation of previous results. It would also be valuable to include comparisons to observations, and perhaps then conclusions can be drawn as to how fine does model horizontal resolution need to be to reproduce observations, and to reproduce accurately physical phenomena (e.g., vertical transport) that affect ozone distributions.

Reply: First of all we would like to thank referee#1 that she/he generally confirms that our analysis does not have any errors. As mentioned above, we think that our study offers important new results, which are also important for communities outside the MESSy community. In particular, our research offers insights into uncertainties of diagnosed ozone contributions. These new results are:

- Diagnosed contributions of anthropogenic emissions are rather robust on the continental scale. Differences due to the applied model, model and emission inventory resolutions and anthropogenic emissions are 10 % at maximum.
- Uncertainties of contributions at ground level due to downward transport of ozone are rather large. We find differences of up to 30 % on the continental scale.
- On the regional scale differences in contributions of land transport emissions are rather large and can reach up to 20 % and more, due to different reasons. Therefore fine resolved models and fine resolved emission inventories are important for regional assessments of ozone source apportionment.
- Source attribution diagnostics are a valuable tool to better understand inter-model differences.

However, the comment from referee#1 also clearly shows that we did not clearly bring up these new results. Therefore,we have highlighted the most important findings in more detail in our conclusion (and the abstract). The changed conclusion reads:

Apart from many model specific findings of this study, its results have important implications for other modelling studies and modellers applying source apportionment methods. These implications are:

- **First, our study shows that average continental contributions of anthropogenic emissions are quite robust with respect to the used model and the used model resolution. This means that global models at coarse resolution can be used to perform ozone source apportionment in this global context.**
- **Second, our results also show that on the regional scale, the differences either caused by different models, but also by model resolution are much larger. These effects arise mainly near hotspot**

regions like the Po Valley or near major shipping routes in the Mediterranean Sea. However, especially in these areas, contribution analyses of anthropogenic emissions are very important and spurious effects, such as artificially increased ozone levels and contributions caused by the coarse resolution of models and or emission inventories should be avoided. Hence, for regional analyses fine resolved models and emission inventories are required.

- **Third, our results clearly indicate how large the spread between models with respect to STE is. The importance of stratospheric ozone, both in the global and the regional model, corroborates the necessity of tracing the contributions of stratospheric ozone to ground level ozone explicitly by the source apportionment methods. However, only few currently available methods used on the regional scale account for this process.**

Further, we agree with referee#1 that a detailed comparison with observations is very valuable. However, ozone contributions cannot be measured directly. Therefore, more complex evaluation strategies involving proxies, which can be measured, are needed. This, however, is beyond the scope of this manuscript, because we here focus on the influence of technical aspects and try to estimate uncertainties, which arise only due to technical limitations. In a follow up study we work on a more detailed analysis involving detailed observations of specific measurement campaigns, which are confronted with simulated mixing ratios and diagnosed contributions to further constrain uncertainties of source attribution results. However, as also referee#2 asked for a section on model evaluation we added Sect. 3, with a basic model evaluation section focusing on ozone. This evaluation clearly indicates, that the vertical mixing of CM50 is too strong and CM50 likely overestimates the contributions of stratospheric ozone at the surface.

p.4, l.17: 'to calculate' should be 'calculation of'
Fixed. Thanks!
p.4, l.31 and elsewhere: 'lighting' should be 'lightning'
Indeed. Thanks!
p.6, l.17: See -¿ Sea
Fixed. Thanks!
p.14, l.27+: use "%" instead of "percentage points"; also 'respectively' is unnecessary.
We removed the respectively, but we stay with the percentage points. The difference in percentage points are obvious from the figure. Calculating % from the percentage-points might lead to missunderstandings.
p.15, l.1: 'effect' -¿ 'affect'
Changed
p.15, l.12: 'to quantify' -¿ 'for quantifying'

Fixed.

We are looking forward to your reply,
Mariano Mertens
(on behalf of all co-authors)

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