The authors are grateful to Referee#2 for the helpful comments that helped improve the manuscript. Due to the strict link between this publication and the work recently submitted by van Dingenen et al. (submitted, 2018) about the TM5-FASST methodology, we offered the possibility to the Editor and the Reviewer to access the work of van Dingenen et al. (submitted, 2018) although not yet published in ACPD. We feel that we have been able to address all concerns, as outlined below. Thanks to the Reviewer's comments, we also realized that some methodological aspects of the TM5-FASST tool could have been further developed also in the publication of van Dingenen et al. (submitted, 2018). Therefore, discussions on the comparison between PM2.5 modeled concentrations vs. the measured ones, as well as further details about the extension of the "perturbation approach" to the attribution of sectors and sources will be included in the review phase of the paper by van Dingenen et al. (submitted, 2018). We feel that we have been able to address all concerns, as outlined below.

Anonymous Referee #2

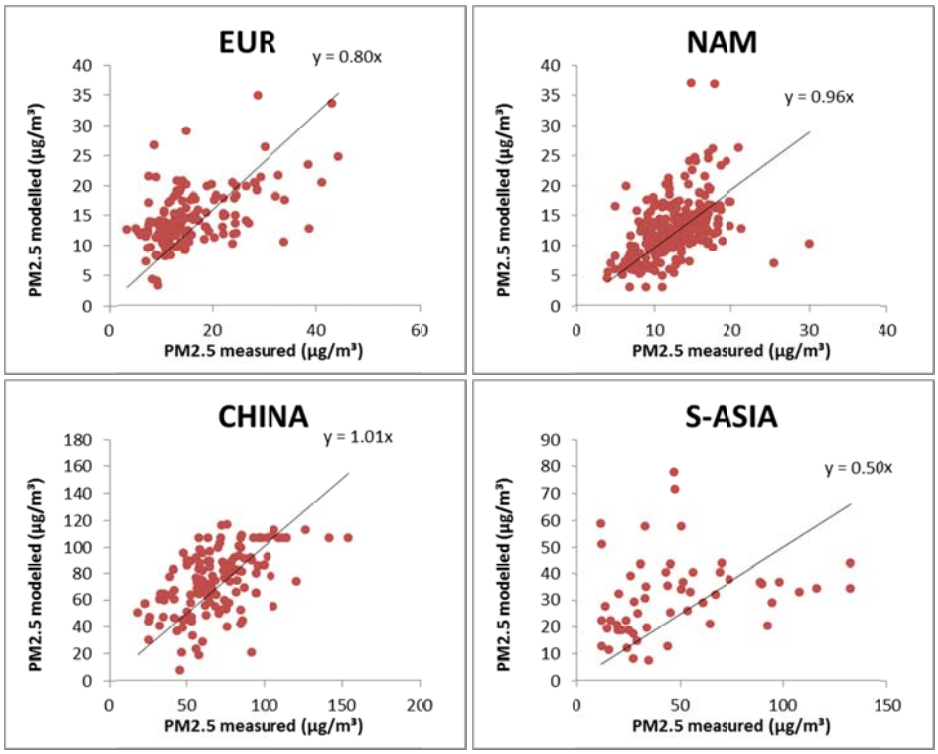
The authors coupled the HTAPv2.2 global air pollutant emission inventory with the global source receptor model TM5-FASST to evaluate the relative contribution of the major anthropogenic emission sources to air quality and health in 2010. They focused on PM2.5 due to its negative impacts on human health. The objective of this paper is to evaluate the emissions uncertainties at sector and regional levels, and their propagation in modeled PM2.5 concentrations and associated impacts on health. Although the authors state that they have two objectives, I do not quite understand the difference between the two. I find that what the paper is trying to do is important but there are some major problems that need to be addressed before this can be published in ACP.

First, if the objective is to understand the health impacts of PM2.5, I believe that the authors need to make sure that their model simulations match with the observations. I do not find the existing comparison in the paper (p. 7, l. 21-28) very convincing. The authors could have at least compared with the recent WHO database of annual PM2.5 concentrations at various cities (http://www.who.int/phe/health_topics/outdoorair/databases/cities/en/). For the US, there is much better database that could be used (https://www.epa.gov/outdoor-air-quality-data/interactive-mapair- quality-monitors). The authors seem to allude that it is ok to not include the natural emissions but I disagree and think that the natural emissions need to be included in the model.

We acknowledge the suggestion of the Reviewer about the comparison of the PM2.5 concentrations estimated by TM5-FASST with other databases in addition to what already provided in our manuscript.
The EPA air quality statistics for USA for the year 2010 (https://www.epa.gov/outdoor-air-quality-data/air-quality-statistics-report) report an annual concentration of PM2.5 of 12.0 ug/m3 which is higher compared to our estimate (7.8 ug/m3) because measured PM2.5 concentrations include all sources of PM (e.g. large scale biomass burning and SOA from anthropogenic sources which are not accounted in our study).
However, the TM5-FASST model developed by van Dingenen et al. (submitted) has been validated against concentration estimates derived from the WHO database and satetellite-based

measurements (excluding dust and sea salt). We report below some details about these comparisons which will be included in the submitted manuscript by van Dingenen et al.
Figure 1 shows the comparison between the PM2.5 concentrations modeled by TM5-FASST and the measured ones reported in the WHO database for different world regions (i.g. EUR=Europe, NAM=North America, China, S-ASIA=Southern Asia, LAM=Latin America, AFR=Africa). This includes measurement points as well as PM2.5 concentration estimates based on a fraction of PM10 measurements (e.g. almost all points for the comparison in China are based on this second method). Quite good agreement is observed for Europe, North America and partly China where measurements have been performed over longer time compared to developing countries and they are based on quite consolidated methods. The comparison for Latin America and Africa is much less robust and the scatter possibly highlights a non-optimal modeling of large scale biomass burning for the TM5-FASST model. Figure 2 reports the comparison of WHO regional average of urban stations against the FASST population weighted average of grid cells. Similarly to the findings of Figure 1, the comparison for industrialized countries is very good, while for other developing regions the agreement is less satisfactory both due to less accurate measurements (e.g. reported by WHO) and lower quality modeling of specific sources by TM5-FASST (e.g. large scale biomass burning).

TM5-FASST modeled PM2.5 concentrations have been also validated against satellite products (see Figure 3) which are based on aerosol optical depth measurements together with chemical transport model information to retrieve from the total column the information of PM concentrations in the lowest layer of the atmosphere (Boys et al., 2014; van Donkelaar et al., 2010, 2014).



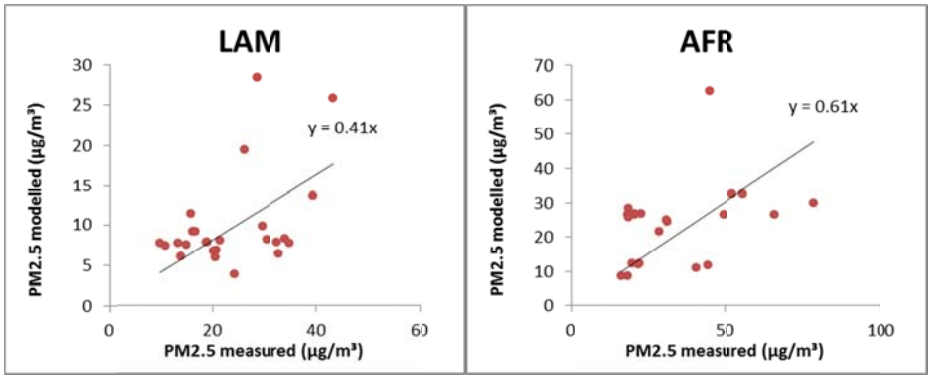


Figure 1 - TM5-FASST grid-cell mean (with urban increment parameterisation) versus individual monitoring stations (WHO consolidated database, including both measured and estimated PM2.5).

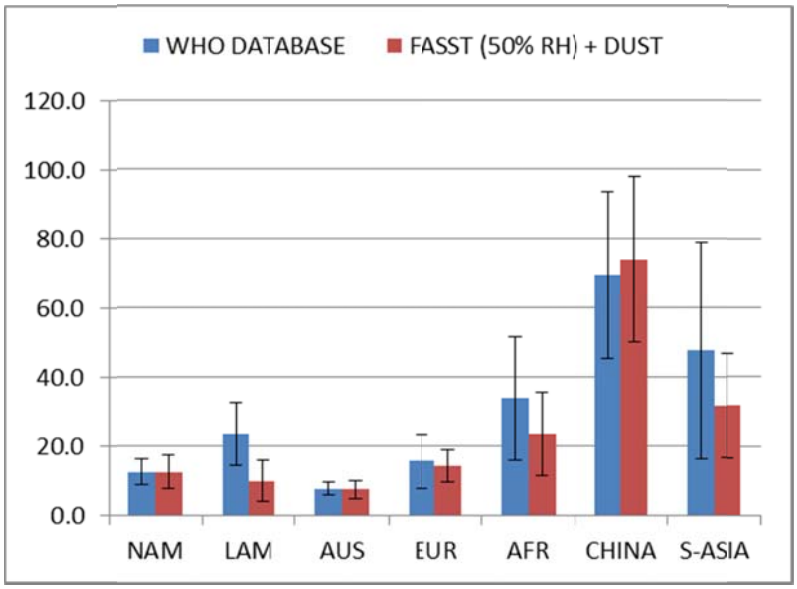


Figure 2 - WHO regional average of urban stations (+/- 1 stdev) and FASST population weighted average of grid cells.

EUROPE	USA

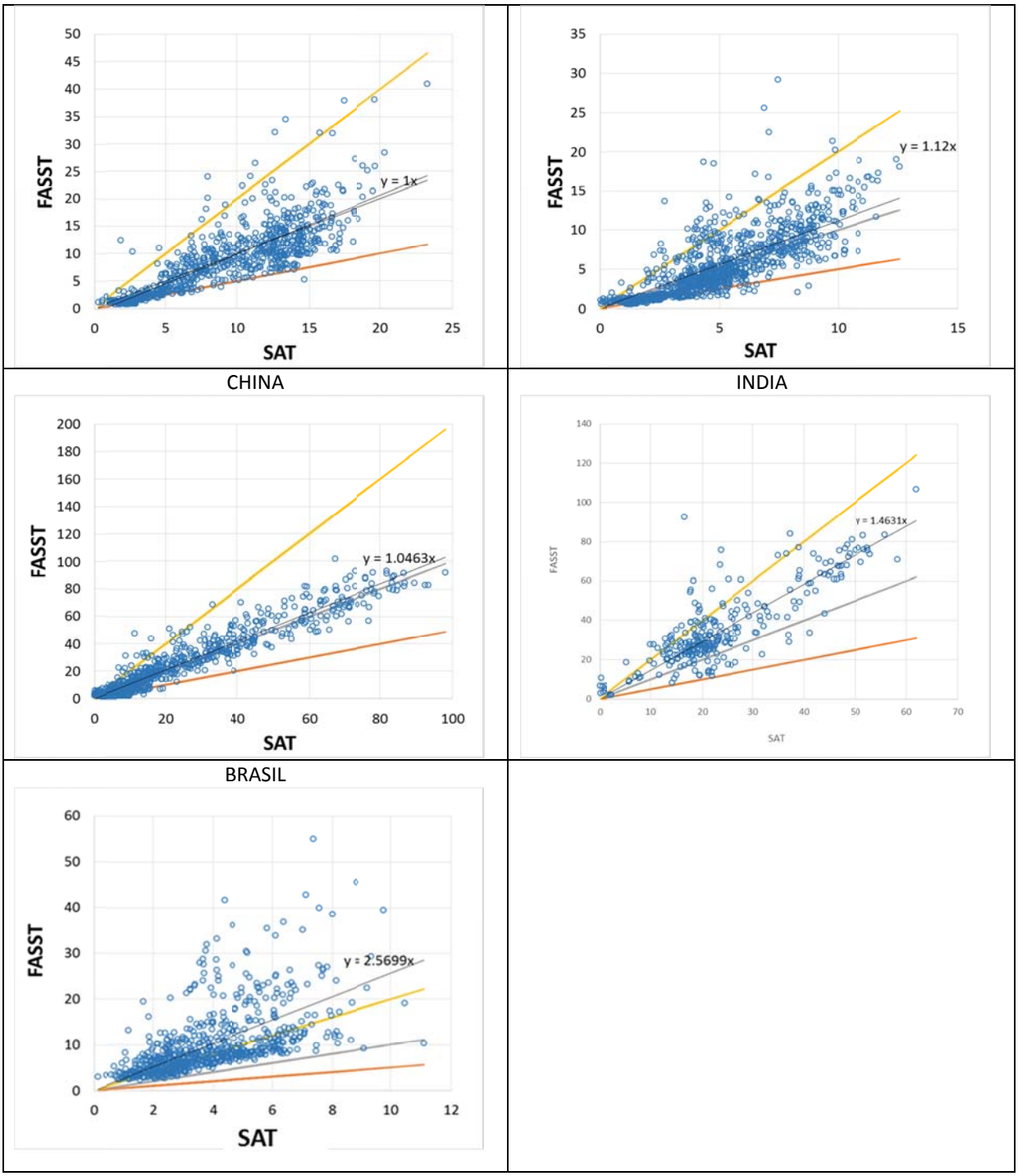


Figure 3 - Comparison between TM5-FASST and satellite products for world regions (Boys et al., 2014; van Donkelaar et al., 2010, 2014).

Therefore we added in the manuscript the following discussion in section 3.3:

"The TM5-FASST model developed by van Dingenen et al. (2018) has been validated against concentration estimates derived from the WHO database and satellite-based measurements (van

Donkelaar et al., 2010, 2014). General good agreement is found between the PM2.5 concentrations modeled by TM5-FASST and the measured ones reported in the WHO database for Europe (within 20% deviation), North America (within 5% deviation) and partly China due to the higher accuracy of the measurements. The comparison for Latin America and Africa is much less robust (40-60% deviation from the 1:1 line) and the scatter possibly highlights a non-optimal modeling of specific sources relevant for these regions by TM5-FASST (e.g. large scale biomass burning) by the TM5-FASST model. Similar results are also found comparing regional averages of urban stations from WHO against the FASST population weighted average of grid cells. The TM5-FASST modeled PM2.5 concentrations have been compared to satellite products which are based on aerosol optical depth measurements together with chemical transport model information to retrieve from the total column the information of PM concentrations in the lowest layer of the atmosphere (Boys et al., 2014; van Donkelaar et al., 2010, 2014). The regional comparison shows consistent results with the ground based measurements comparison (e.g. good agreement for EU and USA within 10% deviation, while lower agreement for developing and emerging countries)."

Second, I find that the emissions uncertainty estimate seems a little simplistic to only assess within the HTAP inventory, considering the existing differences among various inventories. Also, if the RCP emissions for the year 2000 are used as a baseline, to me it makes more sense to use RCP 2010 in their analysis, rather than switching to HTAP v2.2. Or if the HTAP is to be used, the uncertainty analysis should include the differences in estimates between RCP 2010 and HTAP v2.2. Also, it might be a good idea to compare with some other estimates in existing studies that have estimated emissions uncertainties for certain countries.

We would like to stress that the aim of this work is not to compare different emission inventories since this has already been done in other publications (specifically regarding the HTAP_v2 inventory, e.g. Janssens-Maenhout et al., 2015, Crippa et al., 2016), but we aim at addressing the uncertainty of sector specific emissions from this inventory in a quantitative way as well as the differences we observe from one region to the other, based on the uncertainty of activity data and emission factors. There are several reasons to use HTAP_v2.2 and not e.g. the RCP2000 as the basis for our assessment of emission propagation. The TF HTAP aims at bringing policy relevant information, and to this end, it has compiled a policy relevant emission inventory (HTAP_v2.2) for the most recently available year. While the RCP2000 was at the basis of the FASST calculations, and presented the best community emissions effort at the time, we feel that it is now superseded by the more accurate HTAP_v2.2. Given our focus on regional (and not so much gridded) results, we feel that this choice is justified.

Therefore we added the following explanation in Section 2.1 of the manuscript:

"The aim of this work is to address the uncertainty of sector specific emissions from this inventory in a quantitative way as well as the differences we observe from one region to the other, based on the uncertainty of activity data and emission factors. As discussed in the next section, the reason to use HTAP_v2.2 and not e.g. the RCP2000 as the basis for our assessment of emission propagation is that the TF HTAP aims at bringing policy relevant information, and to this end, it has compiled a policy relevant emission inventory (HTAP_v2.2) for the most recently available year. While the RCP2000 was at the basis of the FASST calculations, and presented the

best community emissions effort at the time, the HTAP_v2.2 inventory is now day much more accurate in particular given the focus on regional (and not so much gridded) emission analysis of our work."

Differently from CO2 for which emission uncertainties are much better know, literature studies dealing with uncertainty of emission inventories of all air pollutants show a lack of information on the corresponding uncertainties (while intercomparisons among different inventories are often shown). In addition, literature studies often make use of region- and sector-specific emission inventories and they do not provide a global view on all pollutants, sectors and regions (Hoesly et al., 2017).

However, we took into account the Reviewer's comment including some references with literature studies on emission inventory uncertainties.

Page 9, line 16: Smith et al. (2011) report a range of regional uncertainty for SO2 up to 30% while our estimates are slightly higher (up to 50%).

Page 9, line 24: "Among all air pollutants, represent one of the most uncertain pollutant due to very different combustion conditions, different fuel qualities and lack of control measures (Klimont et al., 2017)."

Third, I also find it problematic that important details and assumptions of TM5-FASST methodology are described in the paper that is still under preparation. I am assuming that the ΔPMref and ΔEref in Eq. 1 refer to the difference between the TM5-FASST simulation results for PM2.5 (and also PM10 as well?) using the RCP baseline and the perturbation (-20%) and the emissions themselves, respectively. However, I find it troublesome that these stay constant when the emissions change for all regions and sectors. We know that PM2.5 formation is a non-linear process and I do not believe it would work in a linear form for every region for every sector. If it does, maybe that is because simulation uses too coarse of a resolution and the result does not seem realistic.

The paper by van Dingenen et al. (submitted) has now been submitted to ACP. It contains a detailed description on the methodology and documents the validity of the linearity assumption for PM2.5 (the simulations were done only for PM2.5 and not PM10). Unfortunately anthropogenic SOA is not explicitly modeled in TM5 but treated as a pseudo-emission. In the manuscript we clarified the concept of dE and dPM as following:

"The reduced-form model TM5-FASST is computing the concentration resulting from an arbitrary emission scenario E_i using a perturbation approach, i.e. the difference between E_i and $E_{i,ref}$ *(dE)* is considered as a perturbation on E_{ref} and the resulting concentration is evaluated as a perturbation *dPM* on the reference concentration."

Also, it seems problematic that no explicit treatment of anthropogenic SOA is considered.

Unfortunately we do not have estimates of the contribution of anthropogenic SOA, as the gas phase chemical degradation scheme didn't include emissions of the relevant precursor gases. The importance of anthropogenic SOA ranges regionally widely, as demonstrated by a recent study by Farina et al. (2010) indicating a global source of 1.6 Tg, or ca. 5.5 % of the overall SOA formation. The relatively importance, however, may dependent regionally, and is deemed higher in regions with less VOC emission controls. Overall, we feel that the uncertainty stemming from our knowledge in SOA formation is higher than the omission of anthropogenic SOA. We would also like to mention that the development of the volatility-based SOA formation approach, means that the boundaries between 'primary' and 'secondary' SOA are disappearing, making it difficult to attribute organic aerosol to either primary, secondary (or natural-anthropogenic), as they strongly interact. Nevertheless, we speculate that the inclusion of SOA would possibly lead to a higher role of the transboundary pollution mainly for those sectors emitting PM and VOCs (e.g. residential, and to some extent transport and industry).

Therefore we added the following sentences to the manuscript:

"The importance of anthropogenic SOA ranges regionally widely, as demonstrated by a recent study by Farina et al. (2010) indicating a global source of 1.6 Tg, or ca. 5.5 % of the overall SOA formation. The relatively importance, however, may dependent regionally, and is deemed higher in regions with less VOC emission controls. We speculate that the inclusion of SOA would possibly lead to a higher role of the transboundary pollution mainly for those sectors emitting PM and VOCs (e.g. residential, and to some extent transport and industry)."

Is it correct that TM5-FASST simulations were run for each sector separately and also for all the sectors combined? That is how it looks like from Figure 4. If so, can the authors confirm that the sum of concentrations from each of the sectors run separately are similar to the values when the simulation was done including all the sector emissions together? It would be a nice test to check the linearity in the model. If the simulations were done in this way, then what was the reason equation 1 had to be used? The authors could have easily calculated the impact of each sector using these simulations instead?

In general, the reduced-form model TM5-FASST is computing the concentration resulting from an arbitrary emission scenario E_s using a perturbation approach, i.e. the difference between E_s and E_{ref} (dE_s) is considered as a perturbation on E_{ref} and the resulting concentration is evaluated as a perturbation dPM on the reference concentration, hence:

$$PM(E_s) = PM(E_{ref} + dE_s) = PM_{ref} + dPM = PM_{ref} + SRC \cdot dE_s \quad \text{(a)}$$

Where $dE_s = E_s - E_{ref}$ and E_{ref} is the RCP reference scenario from which the SRC have been computed.

The contribution of a single sector j is calculated as the difference between the concentration including all sectors, and the concentration from the emissions excluding the single sector j

$$PM(E_{s,j}) = PM(E_s) - PM(E_s - E_{s,j}) = SRC \cdot [dE_s - d(E_s - E_{s,j})] = SRC \cdot E_{s,j}$$

If the linearity holds, the sum of $PM(E_{s,j})$ over all sectors j should be equal to $PM(E_s)$, or:

$$\sum_j PM(E_{s,j}) = PM_{ref} + SRC \cdot (E_s - E_{ref})$$

In Figure 4 we compare both sides of the equation to demonstrate that indeed the linearity assumption holds sufficiently well.

A caveat of TM5-FASST is that no sector-specific SRC have been computed (except for international shipping which was evaluated separately), and consequently our single sector analysis implicitly assumes that the spatial distribution of pollutant emissions at the resolution considered here (1°x1°) is similar for all sectors within each source region. Taking into account that

(1) the spatial distribution of primary anthropogenic emissions is commonly generated using population density as the major proxy (except for large scale biomass burning) – e.g. domestic burning, transport, industry
(2) in many cases, the emission of secondary pollutant precursors is dominated by a single sector (e.g. NH_3 mainly from agriculture, NO_x mainly from transport, SO_2 mainly form energy production)

we deem that the spatial distribution of the individual sectors can be estimated sufficiently accurately for the present analysis, as shown in Figure 4 which has been obtained from the 'total' SRC, applied on single-sector emissions. A similar approach has been recently implemented by Liang et al. (2018) based on the HTAP2 source receptors.

The TM5-FASST runs were performed for different scenarios, comparing the reference HTAP_v2.2 emissions with a scenario where emissions from one single sector were subtracted from the total emissions. Then comparing the reference case and each scenario (REF-$sector_i$), the contribution of each sector to PM2.5 concentrations is estimated. This approach is based on the assumption that the individual sector contributions add up linearly to total PM2.5, as mentioned in the paper. The paper by Van Dingenen et al. describing the whole TM5-FASST methodology has just been submitted to ACP (van Dingenen et al., submitted) Equation 1 represents the basis of the TM5-FASST method, since it describes how a variation in the emissions (delta emissions) determines a delta in PM2.5 based on the source receptor relationships.

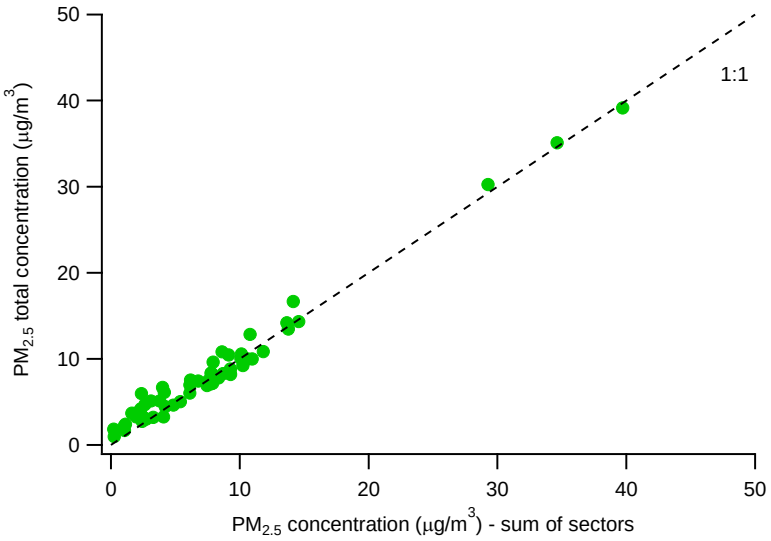


Figure 4 – Comparison between the total modeled PM2.5 concentration and the sum of the sectors.

The following discussion on how to apply the "perturbation approach" on the sector and source attribution will be also included in the paper by van Dingenen et al. (submitted, 2018):

Equation (2) expresses the 'perturbation' approach applied in the linearized TM5-FASST model, i.e. an arbitrary emission scenario is evaluated as a deviation from the base emission scenario, and the resulting pollutant concentration is obtained as the sum of the base concentration and a delta term, the latter proportional to the emission deviation from the base case (Figure 1).

A particular application of TM5-FASST is the attribution of the (anthropogenic) pollutant concentration to individual source regions or sectors. Due to the fixed contribution of the base concentration which does not contain information on the originating sources, Eq. (2) is not immediately suitable for such an analysis. Instead, we calculate for each individual source the contributing part by first evaluating all sources together ('total' simulation'), and subsequently subtracting the individual source emissions (E_s) from the total, evaluating the resulting pollutant concentration (C_{minus_s}), and making the difference with the 'total simulation' to obtain the single source contribution (C_s).

$$C_{j,tot}(y) = C_{j,base}(y) + \sum_{n_x}\sum_{n_i} A_{ij}[x,y] \cdot \left[E_{i,tot}(x) - E_{i,base}(x)\right] \quad (2)$$

$$C_{j,minus\_s}(y) = C_{j,base}(y) + \sum_{n_x}\sum_{n_i} A_{ij}[x,y] \cdot \left[E_{i,tot}(x) - E_{i,s}(x) - E_{i,base}(x)\right] \quad (4)$$

$$C^{*}_{j,s}(y) = C_{j,tot}(y) - C_{j,minus\_s}(y) = \sum_{n_x}\sum_{n_i} A_{ij}[x,y] \cdot E_{i,s}(x) \quad (5)$$

We can now reconstruct $C^*_{j,tot}$ as the sum of the individual source contributions:

$$C^*_{j,tot}(y) = \sum_{n_s} C^*_{j,s}(y) \quad (6)$$

$C^*_{j,tot}(y)$ is equivalent to $C_{j,tot}(y)$ in Eq. 2 only if

$C_{j,base}(y) = \sum_{n_x}\sum_{n_i} A_{ij}[x,y] \cdot E_{i,base}(x)$, in other words if the emission-concentration relation is perfectly linear and passes through the origin.

In reality there is some degree of non-linearity in most emission-pollutant relation as illustrated in Figs. 3 and 6. Figure A shows for each of the FASST regions the total $PM_{2.5}$ concentration obtained by Eq. 6 versus the TM5 base simulation result, illustrating the non-linearity error resulting from the application of Eq. 6. For 43 out of 56 regions, the deviation from the base simulation is less than 30%, only 3 regions (former Soviet Union, New Zealand and Pacific) deviate more than 50% from the TM5 model result. Consistency with the 'perturbation approach' is restored by simply rescaling the individual source contributions:

$$C_{j,s}(y) = \frac{C_{j,tot}(y)}{C^*_{j,tot}(y)} \; C^*_{s,j}(y) \quad (7)$$

This approach is valid for evaluating the attribution by sector as well as by source region.

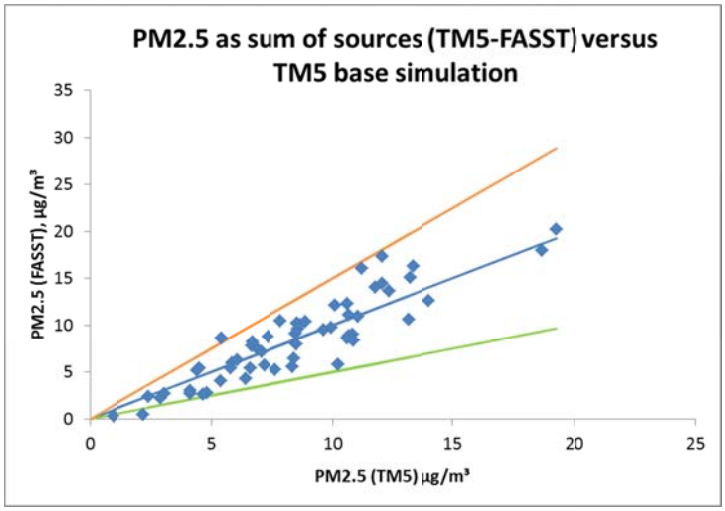


Figure A: Scatter plot of regionally averaged PM2.5 concentration (including all anthropogenic components) obtained as the sum of individual source region contribution by linear scaling of their respective emissions with TM5-FASST source-receptor coefficients (Eq. 6), versus the regional average obtained by the full TM5 model.

I have a hard time understanding the sentence on p. 9 l. 5-8. How do the authors determine the relative contribution to total emission inventory uncertainty? Are the authors using the

uncertainty for a specific sector over the total uncertainty for a specific pollutant as the "average sector relative contribution to total emission inventory"? If so, this does not necessarily take the magnitude of emissions into account and so maybe just looking at this value and deciding which sector to focus on might be a little too simplistic?

As discussed in Section 2.3, "uncertainties have been estimated for each emission sector for every country/region and pollutant. Then an overall uncertainty has been estimated using equation 5 (shown below) from the EMEP/EEA, 2013 Guidebook and which accounts for the weighted contribution of each sector to the overall uncertainty. Then the contribution of each sector to the overall uncertainty is given by the weight of each term of the equation compared to the others, so it does not correspond to the "average sector relative contribution to total emission inventory".

We rephrased as following:

"The complete overview of allTM5-FASST regions is provided in Fig. S2, where the share of each term of the sum of Eq.5 $\left(\sigma_{EMI\ i,c,p} * \frac{EMI_{i,c,p}}{EMI_{tot,c,p}}\right)^2$, representing the sector contribution to the uncertainty of each pollutant in each region, is reported."

Are the upper and the lower boundaries of PM2.5 concentrations (Table 2 and Figure 5) calculated based on the linear relationship between emissions per region? In other words, are they simply calculated from emissions, rather than running the simulation again in a chemical transport model?

To calculate the upper and lower boundaries of PM2.5 concentrations we used the TM5-FASST model and so they are based on the linear relationship between emissions per region. However, new emission datasets including the upper and lower range of uncertainty have been given as input for new TM5-FASST runs which gave us the upper and lower range of PM2.5 concentrations.

We added a sentence at the end of paragraph 2.3 to clarify our approach:

"Based on the upper and lower emission range per region, new TM5-FASST model runs have been performed per source region to retrieve the corresponding range of concentrations in receptor regions (therefore the total number of computations is 56*2 for the uncertainty analysis)."

Minor comments:

1. I would like to see a figure that shows the 10 aggregated receptor regions, as it is unclear, for example, what China+ region includes. Does it just include Mongolia? Or also Korea and Japan?

   Table S2 of the Supplementary material already includes this information for all aggregated regions. China+ includes China and Mongolia+North Korea. We do not aim at having another Figure in the supplementary material about the regions aggregation, in order to avoid

repeating information already provided in a Table and to avoid misunderstandings with the map about the 56 TM5-FASST regions used for the model runs.

2. Why are some European countries lumped together in Figure 2 (Austria and Slovenia, for example), whereas others are not?

   The following explanation has been added in the Supplementary Material (S1) to explain the TM5-FASST regions aggregation.

"The 56 TM5-FASST regions were chosen to obtain an optimal match with integrated assessment models such as IMAGE (Eickhout et al., 2004; van Vuuren et al., 2007), MESSAGE (Riahi et al., 2007), GAINS (Höglund-Isaksson and Mechler, 2005) as well as the POLES model (Russ et al., 2007; Van Aardenne et al., 2007). The grouping of small countries was motivated by (a) finding a compromise between spatial resolution and computational effort required to obtain the set of source-receptor matrices for TM5-FASST and (b) avoiding inaccurate mapping of small individual countries that are represented by only a few 1°x1° grid cells.
Most European countries are defined as individual source regions, except for the smallest countries, which have been aggregated."

3. Why are there more countries in Figure 3 than in Figure 1?

   Figure 1 represents the global view using the 10 aggregated world regions, while figure 3 shows a disaggregated view making use of the original 56 TM5-FASST regions. The reason behind the aggregation to 10 regions is explained at page 4 of the manuscript: "In order to make smaller regions (e.g. European countries) comparable with larger regions (like USA, China and India), in this work an aggregation procedure to 10 world regions (refer to Table S2) has been applied (China+, India+, SE Asia, North America, Europe, Oceania, Latin America, Africa, Russia and Middle East)."

p. 2. l. 30-34: The sentence is too long and difficult to understand. Please rephrase the sentence.

The sentence has been rephrased as following:

"The objective of this study is to evaluate the relevance of uncertainties in regional sectorial emission inventories (power generation, industry, ground transport, residential, agriculture and international shipping), and its propagation in modeled $PM_{2.5}$ concentrations and associated impacts on health. We also investigate the uncertainties in $PM_{2.5}$ from within the region to extra-regional contributions."

p. 2 l. 36-37: The authors state that a second objective of the analysis is to "inform local, regional hemispheric air quality policy makers on the potential impacts of less known emission sectors or regions" but they are focusing on the "6 major anthropogenic emission sectors (l. 6-7, p. 3)." What do they mean by "less known emission sectors" then?

Less known emission sectors (and less regulated ones in terms of emissions) are the residential and agricultural sectors, so the sentence has been rephrased as following:

"A second objective of this analysis is to evaluate the importance of emission uncertainties at sector and regional level on PM2.5, to better inform local, regional and hemispheric air quality policy makers on the potential impacts of sectors with larger uncertainties less known emission sectors (e.g. residential and agriculture) or regions (e.g. developing and emerging countries).

p. 3. l. 19-20. This sentence is not finished.

The sentence has been corrected as following:

"In order to calculate PM2.5 concentrations from the HTAP_v2.2 emissions, we use the native 1°x1° resolution source-receptor gridmaps obtained for TM5-FASST_v0 (Van Dingenen et al., 2018, submitted)".

p. 3. L. 22. Why was such a coarse resolution used, when HTAPv2.2 is much finer?

At the time of creating the TM5-FASST Source receptor relationships (ca. 2007-2010), 1x1 degree global resolution was still of unprecedented high resolution (given hundreds of simulations) and more common was resolutions around 2 to 3 degrees (T42). Only since recently more global models are running on 1x1 degree or somewhat finer, but it is still difficult to make 100s of SR calculations. The 0.1x0.1 HTAP_v2 resolution is employed only in full by regional model studies that used global model results as boundary conditions.

The following sentence has been added for clarity in the manuscript:

"TM5-FASST uses aggregated regional emissions (i.e. one annual emission value per pollutant or precursor for each of the 56 regions + shipping), with an implicit underlying 1°x1° resolution emission spatial distribution from RCP year 2000 which was partly based EDGAR methodology and gridmaps."

p. 3 l. 30 relativey ->relatively
correction done

p. 7 l. 37-39 Perhaps a reference to Bauer et al. (2016) would be appropriate here.

Some changes have been made in that section, adding also more references:

"In order to understand the origin of global PM2.5 concentrations, we look at sector specific maps (Fig. 4). The power and industrial sectors are mainly contributing to PM concentrations in countries having emerging economies and fast development (e.g. Middle East, China and India), while the ground transport sector is a more important source of PM concentrations in industrialised countries (e.g. North America and Europe) and in developing Asian countries. The residential sector is one of the most significant sources of PM all over the world, potentially also affecting indoor air quality (Ezzati, 2008; Lim et al., 2013; Chafe et al., 2014)."

p. 11 l. 36. It is unclear to me where this value (7% for the global non accidental mortalities) is coming from. Can you clarify or cite the source?

We cited the source of our estimates as following:

"We also estimate that 7 % of the global non accidental mortalities from the Global Burden of Disease (http://vizhub.healthdata.org/gbd-compare; Forouzanfar et al. (2015)) are attributable to air pollution in 2010;"

p. 12 l. 10 such the Gulf -> such as the Gulf

correction done

p. 19 Table 2. How do you quantify the uncertainty for a certain pollutant for a region?

The methodology behind the uncertainty estimates for a certain pollutant and region is described in Sect. 2.3 of the manuscript and with the equations 3 and 4.
Table S3 of the Supplementary material also provides region- and pollutant- specific emission uncertainties.

Reference:

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