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comment**

Interactive comment on "Changes in the domestic heating fuel in Greece: effects on atmospheric chemistry and radiation" *by* Eleni Athanasopoulou et al.

Anonymous Referee #1

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Review of the paper "Changes in the domestic heating fuel in Greece: effects on atmospheric chemistry and radiation" by Eleni Athanasopoulou and co-authors.

This is an interesting and well written paper. In general, the approach that was followed is sound and the assumptions can be followed. There are some points that the authors need to explain in more detail. They need to better justify the approach and the model setup they have chosen. I also suggest some modifications w.r.t. the wording as well as to figures and tables.

Major comments:

In section 2.2 and table 1 you should better explain the model setup. It is not clear

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[Discussion paper]

to me which areas were modeled with COSMO-ART, if there is a coarser grid around Athens that was modeled with a CTM as well. It looks like the area you ran COSMO-ART on is about 30x40 grid points. This is rather small, in particular when you use boundaries for the gaseous species which are on a 2.5° x 1.9° grid. In this case one grid cell of the outer grid would be bigger than your entire model domain. In addition, the horizontal resolution of your emissions is much coarser than the model resolution, by a factor of 2.5 x 5. The question arises why you did not make any attempts to use spatial surrogates like population density to redistribute the emission data and provide them in a higher resolution as model input.

page 2, line 21 and in the following: You use BC and EC as it is the same. You need to explain this a bit, e.g. provide definitions of both of them and their relation to what you call "soot". This is of particular importance because the radiative effects of the aerosol is in close connection to the BC/EC concentrations. page 5, line5: "The atmospheric pressure and precipitation parameters were optimized with respect to the high spatial resolution of the current application (0.025ĚŽ)". This could be anything, therefore you need to explain what you did with the data.

page 5, line 16: If I understand it correctly, you model 2013/14 but you used 2009 emissions for all species and sectors, except for wood combustion. This can be well justified if no big changes in the emissions from year to year can be expected. However, you argue that "non-solid fuels" became very expensive (page 2, line 2/3) and that the financial crisis changed the emission pattern quite drastically. Shouldn't this be considered in the emissions of other sectors than wood burning as well?

page 5, line 21: "For Greece, it is calculated that almost all mass (i.e. 98%) of the total PM10 emissions from this source category reflects wood combustion (i.e. open fireplaces)": Is this given in the TNO emission inventory? Or is this given in the IIASA publication which you cite later (line 25)? You should state this more clearly.

page 5, line 31-page6, line 11: This change in the temporal profile is an important

modification of the main PM emission pattern. I think it would be good to explain in some more detail how you did that, e.g. by showing the long term BC measurements you mention. Can you be sure that the BC observations are dominated by wood combustion and not heavily influenced by traffic?

page 6, line 21: Although you modified the temporal profile of the RWB emissions you did not improve the spatial distribution, e.g. by using higher resolved population density maps. Why not? Shouldn't this make a significant difference? Did you make an attempt to couple the day-by-day variation of the emissions to the ambient temperature?

page 6, line 26-31: I didn't get what the assumptions are concerning the magnitude of the emissions. If I understand you correctly, you took the emitted PM10 mass from the TNO inventory without further changes, although you explained before that the assumptions about the fuel split made in this inventory are not valid for the time period you investigate here. In the end, you argue that the magnitude of the emissions is ok because the agreement between model results and observations indicates that. This seems to be inconsistent.

page 6, line 15: What is the reason for the differences between your Mie calculations and the values used in COSMO-ART? Is the conclusion that the optical properties for soot are incorrect in COSMO-ART?

page 6, line 20: "The period studied can be characterized as a relatively mild winter period.": Why did you choose this period when you can expect that the heating activities will be comparably low?

page 8, line 24/25: Is the intense smog period the entire period from 19 Dec to 21 Jan? The mean concentration values for case 2 agree better to the observations than those from the baseline although the total emission amount was the same. Is that correct? You should make this clear.

page 9, line 12 and line 15: You should avoid descriptions like "satisfactorily repre-

sented" and "nicely captured". Each reader might think differently about what is satisfying or nice. Obviously, for case 2 the temporal profile of the concentrations fits better to the observations than for case 1 and you should just describe this. You could give a correlation coefficient to show this in numbers.

page 10, line 33: "The peaks observed at the urban core, as revealed from this model application, are somewhat displaced from the exact location of the site. This finding is in line with the characterization . . .": This is a bit over-interpreted, having the coarse emission map in mind. The grid cells with the highest emissions are simply north of Thissio.

page 12, line 9/10: " . . . for a pre-crisis period or significant change in dominant heating fuels, the TNO-MACC_II emissions from residential heating should be further adjusted.": Why for a pre-crisis period? Didn't you say that the fuels changed significantly during the crisis? Why is the TNO inventory then wrong in a pre-crisis period?

page 14: I do not agree with the last part of the conclusions. In particular: - "Thus, human health implications, as well as policy making, when the fireplaces are in use to cope with high heating demand conditions (HD > 7.5 °C), can be satisfactorily estimated and planned with the aid of such a tool.": You did not estimate any human health effects with your model system. How will help to estimate them? What can be planned with the help of the model results? - "For mild winter conditions (Tmin > 8-9 °C), a post-processing of model results according to the linear regression between HD and model bias, can further improve the quality of the model system.": Wouldn't it be better to improve the emission estimates than to correct the model? - "Alternatively, an interactive treatment of RWB emissions, i.e. their online adjustment according to the actual temperature conditions of the simulation period, is proposed as a means to further enhance the reliability of operational forecasts of online-coupled atmospheric models." I agree to this, but how would you know how much wood is used in comparison to oil or other fuels?

Minor comments

page 1, line 17: "... accurately predicts ...": Qualifiers like "accurately" are always a bit difficult if do not give numbers for the deviations.

page 2, line 2: "exorbitant" should be replaced by "very high".

page 3, line 7: Denier van der Gon

page 2, line 21 and in the following: You use BC and EC as it is the same. You need to explain this a bit, e.g. provide definitions of both of them and their relation to what you call "soot"

page 3, line 32: " ... which was, however, nonlinear.": In which way nonlinear?

page 8, line2: "mean maximum nighttime PM10": over which period was the mean taken? Was this hourly?

page 9, line 1: "Overall, the revised run improved more than the 70% of the day and nighttime PM10 peaks during the intense smog period.": This is not well formulated and therefore unclear. What exactly is improved?

page 9, line 10/11: "which leads to the improvement of the half PM1OA and of all PM1BC the daytime peaks during the intense smog period." This sentence is obscure.

page 9, line 28-30: "Thus, the aerosol chemical composition during the economic crisis is completely altered with respect to the chemical profile of wintertime aerosols beforehand." What was the chemical profile before the economic crisis?

page 10, line 2/3: "The comparison with available measurements during increased wood burning in the Alpine area (Szidat et al., 2009) reveals similarities": Where are these measurements shown? Are they given as the observations in Table 3? Please add the values they found somewhere.

page 10, line 27: "PM1BC is found below 8": The unit is missing.

page 12, line 2/3: "Inversely, during the cold days (Tmin below 8 °C) that the HD is increased (>7.5 °C), and consequently no model overestimation occurs." Omit the word "that". In addition: It is true, that no overestimation occurs. Instead, you see an underestimation.

Table 3 is really hard to read. It looks unstructured I would suggest to split into two or three different tables. One suggestion could be to have the ratios in the bottom (OC/BC, …) in separate table and also the peak analysis in another separate one. For the ratios in the bottom, it is unclear how they are given. OC/BC = 2.8 makes some sense, but what about the other values (BC/TC, …)? Are they given in %? BC/TC = 28 makes no sense. In addition, it seems that you derived them from case2/case 3 differences but you do not say this in the caption or anywhere else in the table itself.

Figure 7: The caption is wrong. It contains parts that belong to Fig. 8.