Comments to the manuscript: “**The impact of temperature inversions on black carbon and particle mass concentrations in a mountainous area**” by Glojek et al.

This manuscript aims at studying the influence of ground temperature inversions on air pollutants concentration in a hilly rural area (Retje karst hollow, Slovenia) impacted by wood combustion aerosols. eBC, PN and PM mass concentration measurements were performed at two air quality stations in the area of study (one placed at the bottom of the depression [Retje village] and one at the top of the hill [Tabor site]). Moreover, measurements were also performed by means of a mobile monitoring platform along a 6 km long route along the hollow.

Mobile eBC concentrations and temperature measurements along the hollow (from Retje (715 m a.s.l.) to Tabor (815 m a.s.l.)) and potential temperature measurements at three stations at different elevation (Retje: 715 m a.s.l., Hrib: 775 m a.s.l., Tabor: 815 m a.s.l.) were used to determine the inversion days and inversion heights by means of the gradient method.

The experiment providing the data for this manuscript was well designed and the experimental approach robust and well described. My detailed comments below.

Comments:

- **Comments on Introduction:**

- I agree with the authors about the difficulties that air quality models have in predicting dispersion and pollutant concentrations in complex relief characterized by strong inversions and strong stability of the lower atmosphere. I have seen strong underestimations of pollutant concentrations by models in such environments. Another feature of these atmospheric conditions is that these can increase the toxicity of ambient air through a progressive accumulation of PM (mostly primary), gaseous compounds, etc. The authors may want to cite a paper where the authors demonstrated that the MH is a key parameter to take into account since it is able to influence all-cause daily mortality more than PM (*Pandolfi et al., Science of the Total Environment 494–495, 283–289, 2014, http://dx.doi.org/10.1016/j.scitotenv.2014.07.004*).

- **Comments on Figure S13:**

- For the mobile measurements (December runs) the profiles of the potential temperature (and not of the actual temperature as it seems from the Figure S13/caption) should be reported in the Figure (light blue vertical lines). Moreover, it is not clear if T-MH (blue horizontal lines) represents the largest positive gradient of the potential temperature or of the actual temperature.
- The largest positive gradient of the potential temperature profile can be used for MH determination, as the potential temperature is higher above the inversion than below (positive gradient). However, it seems that T-MH (blue horizontal lines) is sometimes associated with negative gradients. For example: 171218C or 171224A profiles. Moreover, sometimes T-MH is associated (apparently) to a region where the temperature (light blue vertical line) is constant with height (for example: 171219B and 171225B profiles). Also, it seems that the T-MH was not calculated for some of the vertical profiles (for example: 171223C and 171231A profiles).

- If the gradient method was used for MH determination, then the vertical profiles of the T and eBC first derivatives should be also presented in supporting material.
- Three eBC-MH horizontal lines should be reported in the Figure (black, dark grey and light grey horizontal lines). However, I only see two of them.
- I do not understand how the final MH (red line) was obtained.

- **There are two Figures S12 in supporting material**

- **Comments to Table 5, Figure 5 and related text:**

- In Table 5 the reported MHs were retrieved using the eBC vertical profiles (negative gradient) from selected morning, afternoon and evening runs. Are these MH heights the same that were represented as red horizontal lines in Figure S13? Moreover, it is surprising to me that the authors were able to appreciated 5 meters difference between the evening runs and the other runs. Figures S12 (Correlation between T-MH and eBC-MH for temperature inversion runs in December) clearly shows that the correlation between eBC-MH and T-MH is not very high and that a bias is present. My main question is: how can the authors use eBC vertical measurements to determine the MH if eBC emissions can occur locally along the slope? The bias between eBC-MH and T-MH is actually quite high. Moreover, in Figure 3 it can be seen that eBC concentrations during inversions are higher along the hill than in Retje (pag. 16, lines 404-406).

- **Comments to section 2.3.1:**

- In this paragraph (and in many other part of the manuscript) the term “temperature” is used instead of “potential temperature”. Please, change the text accordingly.
- The authors may want to cite a paper were the first derivative of the potential temperature vertical profiles was used to study how the unstable, stable or neutral atmospheric conditions of the atmosphere alter the distribution of aerosol backscatter ($\propto$ PM concentration) with height (*Pandolfi, M., Martucci, G., Querol, X., Alastuey, A., Wilsenack, F., Frey, S., O'Dowd, C. D., and Dall'Osto, M.: Continuous atmospheric boundary layer observations in the coastal urban area of Barcelona during SAPUSS, Atmos. Chem. Phys., 13, 4983–4996, https://doi.org/10.5194/acp-13-4983-2013, 2013*).
- I do not agree with the nomenclature used. I agree with the authors if they classify a run as “stable atmosphere” when the potential temperature increased with altitude along the depression. However, the authors defined the atmosphere as “mixed” when the potential temperature decreased (negative gradient) with height. In the presence of a negative gradient the atmosphere should be defined as “unstable” and not as “mixed”. By definition a mixed atmosphere defines a region (the mixing layer) where the potential temperature is constant with height ($d\theta/dz = 0$). However, looking at Figure 2, the periods between two inversion periods (shaded areas) are characterized by equal (or very similar) potential temperature values at Tabor and Retje, thus indicating actually a mixed atmosphere (i.e. with $d\theta/dz = 0$).

- **Comments to section 2.3.2:**

- The authors defined the height that separates the boundary layer from the free troposphere (FT) as MH (mixing height) irrespective of the time of the day. I would rather use, for example, PBLH (planetary boundary layer height) as a more general definition of the height separating PBL and FT as MH is usually referred as the PBLH when the atmosphere is well mixed (midday).
- Given that the estimation of the eBC-MH could be hampered by eBC amissions along the slope, I would suggest the authors to compare eBC-MH also with AH-MH (AH = absolute humidity) during mobile run measurements.
- My suggestion is to include in this work the AH too (of course if RH measurements are available during mobile measurements).

- **Comments to Figure 2:**

- As in my previous comment. Looking at Figure 2, the periods between two temperature inversion periods (shaded areas) were characterized by equal (or very similar) potential temperature values at Tabor and Retje, thus indicating actually a mixed atmosphere (i.e. with $d\theta/dz = 0$). However, in the main text the authors defined the atmosphere during these periods as periods with $d\theta/dz < 0$ (whereas it seems from Figure 2 that $d\theta/dz = 0$). So, I have not understood how the authors defined a "mixed" atmosphere: when $d\theta/dz = 0$ or when $d\theta/dz < 0$?
- Interestingly, no rain was observed during temperature inversion periods. Very often, rain was detected just before and just after the inversion periods (shaded areas in Figure 2). Any comment about this?
- During stable atmospheric conditions the eBC concentrations were higher than usual at the top of the hill too (Tabor site). Was this relative increase due to some eBC transport from below (e.g. Retje)? or was due to eBC sources along the hill accumulated during stable conditions?