Response to comments of Anonymous Referee #2

1. Considering the sensitivity of BC and PM concentrations to the near-surface meteorological conditions (i.e. 10-m wind speed, 2-m temperature), I would expect to see a short evaluation of the performance of the CAIRDIO and COSMO models for these variables (can be added as supplementary material). Such an evaluation will allow the reader to have confidence in the ability of the CAIRDIO and COSMO models to correctly predict the near-surface atmospheric forcing and consequently the BC and PM concentrations. Moreover, it could also enhance discussion/explanation of model results for the selected stations.

As requested, a model evaluation for the meteorological part was added. We evaluated the mesoscale simulation with a rich set of observations, which consist of remote-sensing data for horizontal wind, stratification and PBL height (referring to the 4th comment). To respond more specifically to the request, we evaluated near-surface wind speed, wind direction and air temperature at two urban air monitoring sites (for which the data was available). The result of the wind-field evaluation was in line with the evaluation of air pollution concentrations in respect to that the downscaled simulation with CAIRDIO is in better agreement with observations than the much coarser COSMO simulation. The limitations with the 40m grid spacing inside narrow street canyons are also again evident. Still, for a more meaningful evaluation, we think much more wind observations are needed (both a higher observational density in space and time), which are unfortunately not available. Still, we think that the mesoscale forcing applied was quite accurate. Referring to the air temperature evaluation, we observed only marginal improvements (if any at all) with the high-resolution CAIRDIO simulation, which is likely due to the diagnostic downscaling method applied for the surface variables. This finding also supports the development an own surface model for CAIRDIO in the future (apart from your remarks in the 3rd comment).

2. It is still not clear how the hybrid boundary approach at the model top for domain L0 treats the transport of TKE and the other variables. Is there inflow from the model top based on the forcing from the larger-scale COSMO model (i.e. vertical transport of Theta or TKE)? It would be beneficial to further elaborate on this, either in the methodology section or in the appendix. On a same topic have the authors tested the impact of different heights for the domain model top on the model performance?

As suggested, we dedicated now an own Section in the appendix to this hybrid boundary approach, in order to describe it in more detail and to include also a small sensitivity study with the domain height and boundary conditions changed. Basically the approach works like a turbulence recycling scheme, in that existing turbulence near the boundary is “copied” and “pasted” on the mesoscale wind field. All other variables like TKE or Theta (which are prescribed at the domain top from the COSMO model by a Dirichlet conditions) are then actively transported across the boundary by the turbulence. While the approach can still be improved (e.g. more accurate scaling of the turbulent intensities), the sensitivity experiment clearly shows its feasibility for vertically limited domains.

3. How accurate is the use of surface temperatures from the meso-scale model for the L0 domain? Although the surface temperatures from Figure 7 seems to be okay, from a qualitative perspective, they might not be accurate as surface temperatures very much depend on the atmospheric stability and the surface energy balance. In the CAIRDIO model

atmospheric stability and turbulence will be different than in the meso-scale model, and thus surface temperature and moisture may be different. This can adversely impact the near-surface turbulent transport and thus the simulated concentrations of BC and PM. I think it would be appropriate to include a discussion on this limitation, and in the future include a land-surface parametrization to compute the surface energy balance, temperature and moisture in the CAIRDIO dispersion model rather than downscaling the surface fields from the meso-scale model.

We think the first question can now be answered with the evaluation of near-surface air temperature. We did not expect an increase of accuracy by this simple downscaling approach, which we considered as a working solution to include buoyancy effects from heated surfaces. At least on a larger scale, this seems to work quite satisfactorily. However, we agree that at the LES scale, this can be considered as quite inaccurate, as it neglects the feedback from the atmosphere on the surface. Also partial shading inside street canyons cannot be represented by this approach, which is probably why the CAIRDIO air-temperature is not more accurate than the COSMO temperature at the station sites. We elaborated on these limitations more in the paper in Section 2.4.2.

4. In Figure 9d, the boundary-layer height seems rather low during the second day of the case study period, considering also the value of the Richardson number (< -1). Have the authors evaluated the performance of the COSMO model in simulating the boundary-layer height for this period. Such an evaluation can be very beneficial as it can help with the discussion of the simulated BC and PM values.

It is true that the boundary layer was in fact much higher (~1km) on the second day as it can now be concluded from the lidar observations. We think the gradient-Richardson method, which we previously used to derive the PBL height, gave systematically too low values. After consulting some more literature on this topic, we concluded that the Parcel method is the preferable choice under convective conditions and it in fact gave quite similar PBL heights as those observed. For the non-convective periods the PBL height is now determined with the bulk-Richardson method, which computes a bulk-Richardson number for the entire boundary layer, by using differences between the surface and PBL top. This method seems more suitable for vertically inhomogeneous PBL (e.g. with intermittent stable layers) than the gradient approach, and also results in a better agreement with the observations. Nevertheless, the unanimous tenor in literature is that none of the methods mentioned is very accurate.

5. How does the CAIRDIO model ensures that turbulence in generated in the domain during the model initialization? Does the model perturbs the input fields (i.e., wind speed) to faster generate turbulence in the domain or is this done using the subgrid-scale TKE from the COSMO model.

Turbulence is initialized by disturbing potential temperature using the cell-perturbation method described in Muñoz-Esparza et al. (2015). This information was added in Section 2.4.2.

Minor comments:

It might be better to plot the vertical y-axis without the log-scale in figure 14 (and in some other figures), as the log-scale can mask height differences, making it difficult for the reader to distinguish differences in the profiles.

As suggested, we changed the logarithmic y-axis to a linear scale. We are, however, not sure if this is better. The intention with the log scale was to emphasize more the differences within the lower part of the boundary layer and to see the logarithmic height dependency of some variables like wind speed or concentration. But we also understand the demur.

How does the CAIRDIO model treat the intra-urban vegetation? Are trees represented as diffused obstacles as well?

Urban trees are represented by the surface-roughness approach, which uses the available land-use data in 5m resolution. This piece of information was added under Secion 2.4.1. The surface-roughness approach for urban trees is for sure not the most accurate one. Urban trees will likely be improved in a future model version.

It would be nice to include the root-mean-squared error (RMSE) in Table 3.

This error metric is now also included in Table 3. It did, however, not lead to a different conclusion.

There are some errors in Figure 14 labels (u wind labels are (a,i) in the figure but are referenced in the caption (a,j)). Please adjust.

This was adjusted.

In Figure 14 it would be nice to include the vertical profile of the heat flux as well.

The vertical heat flux is now included in the Figure and discussion.

It might be preferable to change the word "grid resolution" to "grid spacing" in the manuscript as the actual horizontal resolution is larger than the grid cell size.

We completely agree therein and changed the terminology when it refers to the nominal grid spacing.