Second review of "Four-year long-path monitoring of ambient aerosol extinction at a central European urban site: dependence on relative humidity" by Skupin et al., 2015

First of all, it should be acknowledged that the authors have attempted to answer most of the reviewers questions and remarks. However, there are still a few major issues which have to be clarified and mentioned in the revised manuscript before it can be published in ACP.

1. The knowledge of the dry state (here the dry particle extinction coefficient) is an inevitable requirement when studying the hygroscopicity of aerosols. I understand that this is not a simple task for this kind of measurement technique, but it should have been more thoroughly discussed. This is especially important for the conclusions of this work since the majority of the results are based on this crucial assumption. In the revised version it states that 'Various measures were taken to ensure this precondition', however, it is highly questionable that backward trajectories and shorter time periods are applicable to ensure a constant dry extinction coefficient.
   The authors state that these effects will be small and state an uncertainty of 20-30 %, however, this is pure speculation and most likely a significant underestimate. Looking at the PBL dilution effect alone, the authors already show in their first case study that the extinction coefficient decreased by 30 % after the PBL height increased from 1300 to 1900 m. In addition to the variation of the PBL, other aerosol effects will have a large influence on the light extinction coefficient and thus increase the here stated uncertainty above 30 %. For example, changes in local emissions, gas-to-particle partitioning effects, new particle formation and other photo-oxidation processes. Again, this has to be clearly discussed and mentioned especially in the conclusions.

2. Similar to the point above, I find it surprising that the AOD in the second case study (27 August 2009) stays more or less constant while the extinction coefficient at the surface decreases significantly. So if the loading is constant, as the authors state, why does the decreasing surface extinction (from approx. 0.4 to 0.1 km^{-1}) not cause the AOD to decrease as well? For this particular day most likely really both the PBL dilution and the hygroscopicity were impacting the magnitude of the particle extinction coefficient measured at the surface.

3. Since in the revised version only Eq. 3 is being used, I would suggest to remove Eq. 4 and 5 and the description of it.

4. In the reply letter the authors state that only the spectral data from 2009 to 2010 was quality assured. Why is the 550 nm channel 'more robust'? The authors need to justify this statement if the data for only this channel is to be shown for the entire 4 years. I ask because there is a clear difference between the 2009-2010 and 2011-2012 data (Fig. 7).

Some more minor issues:

- Page 3: There is a lonely '?' before Draxler and Hess, 1998
- Page 3: I would suggest to replace 'particle optical depth' by 'aerosol optical depth'
- Page 2, right column, third sentence: Remove 'us'
- Fig. 7: There is a Latex error in the caption.