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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A case study of the radiative effect of aerosols over Europe: EUCAARI-LONGREX" *by* A. R. Esteve et al.

Anonymous Referee #2

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This manuscript presents an analysis of airborne in situ data collected during a series of flights over western Europe. This is an interesting and useful analysis that investigates the sensitivity of direct radiative forcing to variations in parameters. That said, there are some issues with the analysis methodology that require modifications to the manuscript. The description of the techniques used is not complete, thorough or clear, and some assumptions are made that are not explained adequately. For this reason I suggest minor revisions to the manuscript.

Below are my primary concerns. Page numbers refer to the "printer-friendly" pdf version.

1) p. 3 line 21. Why is the un-humidified nephelometer "assumed" to represent dry conditions? The RH is typically directly measured (albeit not very well) within the TSI

nephelometers. And lacking an RH measurement, one could calculate it knowing the temperature change between the ambient conditions and the instrument conditions. An f(RH) value calculated when the "dry" condition is actually at 50% RH, for example, could be different by >30% from a true value measured with a dry scattering measured at RH<10%. What is the sensitivity of the analysis to this assumption?

2) p. 3 lines 23-26. This description of the calculating the ambient extinction is hard to understand. Reading Highwood et al. (2012) and Esteve et al. (2014) it appears that the humidified nephelometer is scanned through a range of RH values and a parameterized f(RH) equation fitted to the data. Then the ambient extinction is calculated using the parameterized curve and the ambient RH. Is this correct? Is it done for every second of flight? Please describe in more detail, but succinctly, how ambient extinction is calculated. What is the sensitivity of your analysis to the "dry" f(RH) value?

3) p. 4 lines 2-3. Why is the under-wing PCASP instrument assumed to measure a dry size distribution? This instrument is under the wing specifically to minimize perturbation (via inlet losses and thermodynamic changes) to the ambient aerosol. What is the sensitivity of your results to this assumption?

4) p. 4 lines 2-3. What is the error in the size distribution associated with using calibration particles of a fixed refractive index (latex beads? ammonium sulfate?) when the atmospheric aerosol has a different refractive index (which can be calculted from the AMS measurements)? If this is described in detail in an earlier publication, at least summarize the results here.

5) p. 4 lines 26-29. Why is the aerosol mass profile calculated from the scattering measurements and an (assumed?) mass extinction efficiency? The measurements include direct observations of aerosol mass (submicron at least) from the AMS and SP2 instruments. Why not use these measured mass values directly?

6) p. 6 line 6. Is this calculation for TOA? Please be clear.

7) p. 8 line 15. It is not clear how the choices of ranges of the various input parameters for the sensitivity calculations were made. Here it states, " The sensitivity to the various assumptions made in the radiative transfer model is estimated by repeating our calculations of the aerosol radiative effect using a different assumption, and then comparing the new results with the original values and calculating the difference between them." What were the ranges of input parameters, and how were they chosen? How many different perturbations were considered? Is this done by hand-chosing a few values, or was there a comprehensive Monte Carlo simulation done? Do the pertrubations tested represent the measured variation in that parameter, or just the uncertainty in the measurement, as is suggested by Table 2? What does it mean to vary the "size distribution"? Is the number varied, the mean diameter, or the standard deviation? What is the sensitivity to each of these components of the size distribution function? This part of the manuscript is the heart of the analysis and the methodology needs to be much clearer. If at all possible, I recommend that the sensitivity study be separated into portions due to experimental uncertainties and measured (geophysical) variability. This would be very interesting–how well can we determine the direct effect (measurement uncertainty) and how much does it vary (geophysical variation)?

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