Review of the "Towards inverse modeling of cloud-aerosol interactions – Part 1: A detailed response surface analysis" by Partridge et al. (Atmos. Chem. Phys. Discuss., 11, 4749–4806, 2011, www.atmos-chem-phys-discuss.net/11/4749/2011/)

General comments – overview

The authors describe and explore a methodology to infer aerosol properties from observations of cloud microphysics, the cloud droplet size distribution in particular. The method is illustrated with an application to a synthetic data generated with an adiabatic air parcel model. It is show that the use an advanced search algorithm, described in the manuscript, is needed to capture the "optimal" solution to the posed inverse problem. Also, even though this is not clearly stated, it is shown that the inverse problem may not have a single, well-defined solution.

Even though I have some minor reservations regarding the approach taken in the manuscript, the methodological basis seems to be solid and the approach is novel (according to my knowledge). However, the question remains if Atmospheric Chemistry and Physics is suitable journal where the work should be published. Namely, the authors do not have or even do not outline (possible) atmospheric applications but illustrate the method with a simplified, synthetic test case. Therefore, it remains unclear why in the title of the manuscript appear terms "towards" and "part 1". So, towards what exactly and what do the other parts of the series will contain? At the current form, a journal focused on inverse problems would be more relevant forum to the author's work.

My second major reservation is that at the current form, the manuscript seems to be an excerpt from a monograph and not a separate research article. In particular, the discussion in section 3 is largely on a qualitative level and should be compressed heavily to make the paper more readable.

I believe, however, that the authors can modify the manuscript to address my major concerns. In the following, I will give more concrete comments and recommendations, but at this stage, I think that giving technical comments is preliminary. However, I'm happy to give more detailed comments once the authors have addressed the points given in below.

Major comments

1. Introduction. In the last section, the authors will give a hint regarding the potential applications of the method. Please make this more concrete – what are exactly the applications (what measurements are needed and what aerosol properties can be inferred)? Why the manuscript is named as "Towards…"? What will the other parts of the series contain?

2. Method validation. The authors base their approach on using an adiabatic air parcel model. This also implies that the method is only applicable to measurements conducted under adiabatic conditions. This point should be mentioned. Also, a discussion regarding

how to verify this (i.e. which measurements are needed to ascertain adiabacity) should be included.

3. Choice of the objective function. Please provide an explicit equation for the chosen objective function. Now it is extremely hard to understand the discussion in the last two paragraphs of section 2.3. The same applies to the exact definition of the objective function. Please re-write the section to make it more accessible. In particular, it is hard to understand how two different quantities are included in the objective function. Also, because of the moving sectional approach, the diameters at a certain bin i do not match when comparing the model and measurements/synthetic data. How did the authors overcome this problem? This is crucial to the manuscript so clarify this point when discussing about "X and Y components of the size distribution" (the term itself is rather vague).

A second issue which I could not grasp is how the authors could have problems when interpolating the data to a grid with a different resolution. In common modeling applications, adiabatic air parcel models are run with much more number of bins compared to the resolution of the applied instruments. This should make the interpolation rather trivial without causing any dents/spikes to the re-mapped data. Also, have the authors tested different interpolation algorithms found in the literature? To me, this seems more like a technical problem rather than a fundamental one.

4. Section 3. As mentioned above, please compress all sub-sections so that the main points are conveyed. Also, this should save some space to include a discussion about atmospheric applications – what measurements (including aerosol related) are needed to constrain what parameters? The authors need to address this question as because it is known already from forward-modeling that different parameter combinations yield the same cloud droplet or CCN number concentrations. Related to this, can the authors identify parameters that cannot be constrained using the information that the current aerosol instrumentation provides?

Minor and technical points

Detailed comments will be given after/if the authors submit a version that addresses the above mentioned points.