

Interactive comment on “Measuring the atmospheric organic aerosol volatility distribution: a theoretical analysis” by E. Karnezi et al.

Anonymous Referee #1

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This paper presents work in an important area of considerable current interest. The retrieval of volatility distributions from thermodenuder measurements is reliant on a number of assumptions and is subject to uncertainties in a range of parameters. The manuscript attempts to investigate a number of these uncertainties using a reasonably appropriate combination of modelling approaches with error propagation to conclude that the particle volatility distribution is underconstrained. The authors then propose a combination of dilution and heating as an improvement to using a thermodenuder alone, as previously suggested and demonstrated by Grieshop et al., 2009.

The work is largely well-conceived and the problem appropriately framed in terms of the literature up until early 2011. Thereafter, much literature directly of relevance to the current work has been ignored (e.g. Cappa & Wilson, 2011; Fuentes & McFig-
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gans, 2012; Saleh et al., 2011; 2012 and later citing references, amongst others). The area has developed considerably since the most recent literature on thermodenuders cited in the current work. It is a little unfortunate that the authors are apparently unaware of these studies which previously investigated many of the questions in the current manuscript (in some cases in greater depth and quite robustly). This is particularly surprising, since one of the authors also co-authored a recent paper that cited the first three papers listed above (Hakkinen et al., ACP, doi: 10.5194/acp-12-10771-2012, 2012). The lack of recognition of literature is quite troubling throughout: the suggested combination of dilution and heating as constraint on a volatility distribution is not novel, but the Grieshop et al. (2009) paper where it has been demonstrated was again uncited. I understand that this is a theoretical analysis, but there is no reason not to recognise where the technique has been applied experimentally.

Whilst the application and some of the conclusions of the current work have some novelty, I feel that it would be in the authors and the broader community's interests for the manuscript to be framed in terms of the recent directly relevant literature. The authors should especially identify the particular areas of novelty of their work in the context of this literature and where their approach confers new insight or advantage over previous work. I am not saying that the current work has no value, just that it is hard to take it seriously when conducted in an apparent vacuum. I was close to recommending outright rejection, but there is valuable work included in the manuscript. I would be happy to re-review a resubmitted paper in which the novelty were more easily identified and contextualised.

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