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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Mapping the uncertainty in global CCN using emulation" *by* L. A. Lee et al.

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Generally comments:

The paper look at parametric sources of uncertainty in CCN formation in a sophisticated aerosol cloud micro-physics simulator coupled into a chemical transport model. It makes use of a space filling design and statistical emulation to represent the spatial impact of individual and combined effects of parameters on CCN formation. The subject of the work is highly topical, addressing the sources of uncertainty in CCN is a key contributor to current uncertainty ranges in aerosol indirect effects. The tools used to do this are also novel, and will be of wider interest to the community. The main criticism of any substance that I would raise, relate to the presentation of the background context and perhaps how some of the results are presented within the conclusions. The background presented in the current introduction misses the link between current aerosol forcing uncertainty and this study, and misses a number of key papers using a similar approach as that followed here. The conclusions include good discussion but miss some of the important implications of this work. The paper should certainly be published if these issues can be addressed, and would fit very well within the ACP framework.

SPECIFIC POINTS:

ABSTRACT: The abstract, quite correctly, frames the work within the background of the current very large uncertainty of the radiative forcing contribution of aerosols, one of the critical uncertainties in current climate science. It then jumps straight to how emulation was done. As it stands this paper is missing important context, within which to see this work.

(A) IPCC uncertainties estimates are radiative forcing estimates, where as what is emulated is the cloud condensating nuclei (CCN) in this study. The link between the two, first needs to be made (a key uncertainty within IPCC aerosol forcing estimates arises from indirect (aerosol-cloud micro-physics) effects – this study focuses therefore on CCN (a cloud property closely tied to these indirect effects)). I am not overly familiar with GLOMAP but assume that CCN was chosen as GLOMAP has not yet been coupled to an atmospheric radiative scheme?

(B) This work makes use of GLOMAP-mode, which is a aerosol micro-physical model of much greater sophistication than the aerosol representation in IPCC assessments. The capabilities of this model need to be emphasised/contrasted with the existing/simpler schemes used in GCMs. I don't have a feel for, nor did I pick up from this manuscript, how coupled the aerosols scheme is with the cloud micro-physics (if at all). Are clouds resolved in this configuration (and hence do aerosol rain out processes get explicitly represented)?

Both of these points are important as there is an established body of literature that does look at parametric uncertainty to aerosol forcing contributions within current GCMs.

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See points about Introduction below. There is very clear differences between what this paper is doing compared to previous studies, but being clear on the above two points is important to establish these differences for the reader.

INTRODUCTION: There are been a number of studies which have looked at model parametric uncertainty. Ackerley et al, JGR, 2009, explored uncertainty within parameters determining aerosol emission, formation and removal pathways within a GCM framework. They found a large impact on sulphate burden but a small impact temperature (dominated by emission uncertainty). [Incidentally, this study generated ensemble sizes sufficient to apply an emulation approach.] In contrast Haerter, et al, GRL, 2009, Lohmann and Ferrachat, ACP, 2010 both showed that aerosols were sensitive to parametric uncertainty in parameters which controlled atmospheric properties, especially clouds, within the ECHAM model. While Lohmann and Ferrachat only looked at 2 parameters, Haerter, et al showed that there was a much wider sensitivity to a broader range of parameters. The introduction to the Lee manuscript implies a vacuum of multiple parameter approaches to aerosol uncertainty. The existing literature needs to be acknowledged and the originality of this contrasted against this work [my impression is this paper is a more targeted assessment (on CCN), using a more sophisticated aerosol modelling framework, and makes use of emulation to make firmer statistical inferences about the relative impact and interactions of the parameters explored].

The introduction spends a lot of time contrasting this approach to OAT SA, but does not provide an example reference. I am not familiar with these studies and question whether contrasting against OAT is that informative. Noting that multi-perturbations and emulation enables interactions to be explored is sufficient motivation surely? This is a common approach. e.g Collins et al, Climate Dynamics, 2011 use a space filling design (also Latin Hypercube) to facilitate emulation (e.g. Sexton et al, Climate Dynamics, 2011). The motivation for this is discussed in the Collins et al paper.

I initially got lost in the discussion of Monte Carlo simulations. What I think the authors mean is that: the model parameter space could in principle be sampled using a large

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number of simulations (e.g. using MC design) however due to the v. high computation cost is can not be done in practice. This should be clarified in the text.

Statistical Methods:

I am a bit out of my depth here. The method describes using a guassian process emulator and notes (line 5, page 14094) that the emulator provides an estimate of the model output (at any point X) with uncertainty. Is the guassian emulator making use of a nugget [e.g. Andrianakis and Chandler, Computational Statistics and Data Analysis, 2012]? Otherwise, doesn't the guassian emulator fit the value of GLOMAP output points exactly and represents greater uncertainty away from these points?

Results:

Figure 1 shows comparisons of mean emulated and modelled CCN – with regional breakdown of uncertainty ranges. There is currently a lot of interest in the role of indirect effects (e.g. Booth et al, Nature, 2012) in the North Atlantic. Could this be one of the regions for which the authors show uncertainty ranges?

The high CCN, low frequency tails in regions remote from emissions are interesting (Figure 1). Are there simulated points which fall into this range, or are these tails solely emulated? I guess I am asking whether these tails are real or could they be an artefact of extrapolation by the emulator?

I got myself tied up (e.g. page 14096, line 9-11) with which uncertainty was being referred to. The first sentence referred to the broader range of CCN values over polluted land masses (e.g. Central Europe has a larger range in CCN than the S. Ocean, Figure 1). What therefore is the "coefficient of variation" (Figure 2)? Are you just saying that the relative uncertainty is larger (signal to noise is lower) in the lower CCN regions? The only clue I had to what this was was the sigma/CCN in the figure caption.

page 14096, line 19: "In order to identify how the CCN can be better constrained" is far too vague. I agree with the sentiment, and this paper is an important step in this

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direction. However the link between identifying which parameters are most important and better constraints, is never really explained. I would just motivate this by saying something like: Our experimental framework enables us to identify in which parameters uncertainties are most significant for each region.

Page 14097, lines 4-5: I am not sure what significance the authors are placing on the greatest “coefficient of variation”. It is not clear, to me, that regions of small CCN (but have greater relative uncertainty) are more important than regions of larger CCN means and ranges. Perhaps this discussion could be motivated from a more physical perspective (e.g. what impact are each of these uncertainties likely to have on radiative forcing), which should help to reader identify what is more important. If it was linear, then large CCN ranges (with smaller “coefficients of variation”) would be more important than uncertainties in regions of small CCN (with relatively larger “coefficients of variation”). [To put this more strongly, I think the “coefficients of variation” are a Red Herring – and not interesting from a uncertainty in a physical system perspective. What am I missing?]

Page 14098, lines 9-24: I don’t see how an emission can be the dominant control on CCN but the CCN is insensitive variations around this emission. Is there a confusion here between emissions (which must ultimately account for the presence of aerosols) and the parameter controlling the mean CCN? The discussion contrasting Merikanto and this study is interesting but I am not convinced it has anything to do with apparent nuanced differences between controlling parameter and the parameter which the CCN is most sensitive to. Isn’t this just saying that when NUC_THRESH is explored within this framework, it is less dominant than Merikanto et al estimated perhaps because the moisture limits reduce the impact in the drier free troposphere?

Page 14100, line 26-27: I have problems with this inference. This statement implies that this parameter (a representation of large area aggregate property) can be known exactly. Some of these parameter uncertainties are as much to do with trying to represent sub-grid heterogenous effects by a single parameter, as they are to do with

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uncertainties in observing them, and are therefore at least partly irreducible. e.g. the model emission uncertainty parameter does not have a single correct number – as there is a spatial component to this uncertainty that is not accounted for in a single global scaler.

Conclusions:

The conclusions include very useful discussion, including a very nice discussion on structural weaknesses. What I think is missed is whether this manuscript provides a step towards quantifying and reducing current aerosol forcing uncertainty raised in the first lines of the abstract? There are two parts to this. Firstly, quantifying the uncertainties. This has been aptly demonstrated here for CCN with GLOMAP (with some caveats about un-elicited priors) and illustrates how this technique could be extended to a wider model framework. The second aspect relates to reducing uncertainties. This is really only alluded to (one throwaway line in the Abstract), but is perhaps the most existing potential of this approach. (Much like the discussion about Calibration) if the emulation was extended to also emulate these observable properties and each emulated sample then weighted by how well it compared to the observations – many of the parameter uncertainty ranges would be expected to be reduced where as others may remain large. This information would be very useful as the resulting spread of aerosol responses would tell us much more about what fraction of current uncertainty is reducible vs irreducible. This is the information that the climate projection community is crying out for.

TECHNICAL COMMENTS:

Figure 4: scale: the scale was selected to provide a colour spectrum spanning the range of variances. However as most of the values are in the <100 range, very little spatial structure can be detected (e.g. larger role of oxidation activation diameter in maritime stratocumoluous regions (page 14097, line 22) can not really be seen in this figure). I'd suggest plotting colour ranges for 0-100 and supersaturating those few

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regions with values above this.

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