Atmos. Chem. Phys. Discuss., 15, C9415–C9423, 2015
www.atmos-chem-phys-discuss.net/15/C9415/2015/

Atmospheric Chemistry and Physics Discussions

ACPD

15, C9415–C9423, 2015

Interactive Comment

Interactive comment on "Development of the Ensemble Navy Aerosol Analysis Prediction System (ENAAPS) and its application of the Data Assimilation Research Testbed (DART) in support of aerosol forecasting" *by* J. I. Rubin et al.

Anonymous Referee #1

Received and published: 17 November 2015

This paper describes the development of a new data assimilation system (ENAAPS-DART) for global aerosol. This system uses ensemble simulations for perturbed meteorology and perturbed aerosol sources to assimilate MODIS AOT observations (using an ensemble Kalman filter) to obtain optimal AOT distributions. While the base technology (DART) and application (aerosol assimilation) are not new, this paper describes various experiments that are new and probe the limits of ensemble data assimilation for aerosol. Also, the authors compare the results from this new system to that of an operational 3D-VAR system. This paper is suited for publication in ACP.

General comments

I have two issues with the methodology in this paper. Neither invalidates the work of the authors but makes it hard to properly understand the results. First, no results are shown for a standard run of the model without assimilation. Hence the improvement due to assimilation is unknown and the differences between various assimilation setups can not be properly judged. Second, no proper attempt at filter tuning is done. In particular, ensemble size and localisation length-scale are not systematically varied and their effects studied. In this respect Fig 9 is slightly worrying: panel d (which shows differences between a 20 and 80 member run) shows similar or larger differences than the sensitivity experiments for a 20-member ensemble (a,b and c).

The authors at times generalize too much from their own (limited set of) experiments:
- while the possible problem due to constant inflation is worth mention and analysis, no other authors have come across this and it is possible this is entirely due to very a specific system (ENAAPS-DART).

- the relative importance of source vs meteorology perturbation is hard to assess given that source perturbations are always generated with a 25% spread. This uncertainty seems optimistic at hourly and gridbox scales.

Sometimes there are quite lengthy descriptions of results, region by region, while the same results are efficiently summarised in Figures and Tables. Maybe the authors can try to make their text more concise.

Specific comments:

Apparently inconsistent acronyms: AOT and NAVDAS-AOD

The paper by Schwartz et al JGR 2014 deserves mention as it also compares 3D-VAR and ensemble Kalman filter methods for aerosol assimilation.

Abstract: please consider my general comments

ACPD

15, C9415–C9423, 2015

Introduction: a major advantage of ensemble DA systems over others is the relative ease of implementation and maintenance, especially in view of the fact that many aerosol and aerosol-cloud processes can be modelled in different ways.

p 28073, l 13: "In order to increase understanding of forecast uncertainty and aerosol forecasting dependencies on underlying meteorology, a 1 resolution, 20 member ensemble version of NAAPS (ENAAPS) was created". The exact meaning eludes me. Does this refer to a one-off experiment or is it an on-going activity? What was learned from this?

p 28074, l 17: "a brief synopsis is provided here, noting a few key differences". While I agree with this level of detail, I think the text might be clearer in specifying what are the differences. E.g. "Likewise, the sea salt source is dynamic in nature with emissions as a function of surface wind speed (Witek et al., 2007)." suggests there are no differences wrt seasalt so why mention it? It doesn't help that a brief (and necessary) explanation of basic aerosol description is interjected ("A combined anthropogenic and biogenic fine aerosol species (ABF) is represented in the model which accounts for a combined sulfate, primary organic aerosol and a first order approximation of secondary organic aerosol."). I suggest to reorganise this in two paragraphs: the first a very brief overview of essential NAAPS characteristics (e.g. basic aerosol description + emission datasets and parametrisations), the second the key differences of the version used in this paper.

p 28074, l 17-21: What is meant by a MODIS-only version? FLAMBE is completely ignored? Or only MODIS data are used for a specific FLAMBE version?

p 28075, l 9-17: ENAAPS is in principle independent of (aerosol) assimilation, no? So the "exception of data assimilation" is a bit confusing. The distinction between 'deterministic' and 'ensemble' meteorology fields is also confusing. I'm guessing this is in-house jargon? The ensemble meteorology fields are also the result of deterministic models. How is this ensemble produced (e.g. what is perturbed, a very brief description of McLay et al would be good)? What does "truncated to 1 degree" mean (is NOGAPS

ACPD

15, C9415–C9423, 2015

Interactive Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

a spectral grid model)? Why match the deterministic (!) NAAPS reanalysis? It will be used with ENAAPS, not?

p 28076 l 1: "requires a priori assumptions". It can be argued that ensemble DA methods also require a-priori assumptions on the model forecast error, in that they assume a-priori uncertainties in meteorlogy and emissions and from that calculate the ensemble forecast.

p 28076, l 4: "is considered to be a random draw from the probability distribution of the model's state given all previously used observations." This sentence completely ignores a-priori error sources in the ensemble, even though they are the essence of the system.

p 28076, l 5: "The use of ensembles to sample the error allows the error to evolve non-linearly in time with the flow-dependent covariances between different state components determining how observations impact the ensemble estimate" Shouldn't there be a comma after 'in time'?

p 28076, l 17: It is not entirely clear how EAKF and DART relate? EAKF is part of DART, and I think it is the only ensemble DA in DART. What does DART offer beyond EAKF?

p 28076, l 20-25: Apparently DART does not include an observation operator H, but uses ENAAPS calculations of AOT. As AOT will depend on humidity (which will be different in different ENAAPS members), doesn't this imply that the effective observation operator used in DART is non-linear instead of the linear operator assumed in a Kalman filter? (That is: across the ensemble, AOT cannot be generated from a form like H x, with x the aerosol state vector and H a matrix).

p 28077, l 27: Why usually in the prior? Won't this distort any covariances that have been built up during the short-term forecast? Can't it be applied to the posterior? I thought that was the more common way to use inflation.

p 28079, l 3-5: "The effectiveness of the ensemble data assimilation system is highly dependent on having sufficient spread in the ensemble members in order for the observations to impact the model forecast." This suggests that the biggest issue is to have as large a spread as possible. I would argue instead that the spread should be an indication of forecast uncertainty (both know uncertainties, ie meteorology and emissions and unknow uncrtainties, e.g. due to model errors).

p 28079, l 5-15: Maybe the generation of the emission ensemble should be discussed before the inflation/localization? The latter are after all solutions to limitations in the first.

p 28079, l 13: Why 25% and not 10 or 100%? For sea-salt and dust, arguably perturbing emitted particle size/windspeeds can be just as important?

p 28080, l 6: It would be good to have a brief explanation how rank histograms are created and what their purpose is? They are not a standard test in aerosol ensemble DA (but possibly should be).

p 28080, l 8: Why is the prior a stronger indication of assimilation? I guess because they show how well a previous analysis pulled the system to the truth. An analysis will agree (fairly) well with observations by construction. Still, a bit more explanation or references are welcome. Do your data actually bear this out: i.e. does the prior show stronger signal to variation in experimental setup than the posterior? This would be very interesting to show.

p 28081, l 8: Maybe change "incorporate" to "assimilate"?

p 28082, l 1: So which ENAAPS-DART assimilation experiment is shown here? What has been perturbed here? Has the system been optimised or not (inflation/localization)? What is the purpose of this Section? If it is to show global aerosol features, isn't this better shown during the comparison with NAAPS/NAVDAS? It might be clearer to first discuss the optimization experiments and only then discuss the global

ACPD

15, C9415–C9423, 2015

features seen in the best setup.

p 28084, l 13: Why now the posterior AOT? Earlier you argued that the prior AOT should be used for comparison against observations.

p 28084, l 17: Higher dust AOT is probably due to some higher windspeeds in the meteorology ensemble and the thresshold windspeed for dust emission? What drives the increased AOT over wildfires?

p 28085, l 11: This is an interesting discussion of the role of inflation. It seems to me that the discrepancy between prior and observations is due to either: 1) observational biases; 2) model biases. A Kalman filter assumes that both are unbiased. Your results suggests that adaptive inflation serves to camouflage such biases (unless they become too big and the syetm crashes). This warrants some discussion by the authors.

p 28085, l 18: prior of inflation equals its posterior from previous cycle: this is also known as persistence modelling.

p 28086, l 2: "issues occur with the constant covariance inflation where there is limited observational coverage". See my previous comment, I believe this could be equally due to biases in observations or models than coverage.

p 28086, l 8: "the normalized standard deviation", that is: 1 ? Ah, Figure 4 suggests it is normalised by the mean. Please indicate this in the text as well.

p 280866, l 22: "The growth in spread in the Southern Pacific Ocean for the constant inflation experiment is a result of having continuous inflation with no observations to bring the ensemble back to reality". I think it is important here to note that this may be a feature solely found in DART-EAKF. To my knowledge, no other studies (e.g. Sekiyama et al, Schutgens et al, Dai et al) have found this growing ensemble spread. It may be related to the fact that in DART, inflation is applied 1) to the prior; 2) even when there is no reason for inflation (i.e. when there are no observations).

p 28087, l 2: "Although spatially and temporally constant covariance inflation has been

ACPD
15, C9415–C9423, 2015

the chosen method for aerosol applications in the past, it is not recommended since aerosol observations are spatially heterogeneous. On the other hand, adaptive inflation increases ensemble spread where there is observational information available, producing stability, a desirable characteristic for an ensemble system". This statement is far too bold with little evidence to back it up. Your analysis suggests this to be true for DART-EAKF but as I said before, it hasn't be noticed by other authors. I suggest rephrasing this to something like: "It is suggested that particular attention is paid to the temporal evolution of ensemble spread in case a constant inflation factor is used, because our results suggest . . . ."

p 28087, l7: "These findings are consistent with idealized experiments and NWP applications of ensemble systems where a temporally and spatially varying inflation is recommended over a constant inflation approach (Anderson, 2009; Li et al., 2009; Miyoshi et al., 2011)." Obviously there are other reasons why AI may be preferential to a constant inflation factor. I believe the listed authors discuss the issue of model biases that are effectively dealt with by AI. Note that model biases are really the bane of DA and AI is essentially a way to sweep them under the carpet (or conversely: a way of studying them by tracking the evolution of the inflation factor).

p 28087, l 23: "In particular, a large increase in spread is found at dust source regions." Presumably because of the windspeed threshold for dust emission? How much bigger than 25% is the spread?

p 28088, l 1: "the meteorology ensemble increases spread for sea salt aerosol" Seasalt emission is presumably not governed by a windspeed threshold, although it will have a non-linear dependence on windspeed. Is this effect therefore larger for dust than seasalt?

p 28088 l 5: "The meteorology ensemble appears to be the main driver of ensemble spread." It may be good to remind the reader that you have assumed a 25% uncertainty in emissions. I find this rather low especially because this is uncertainty on short time-

ACPD
15, C9415–C9423, 2015

Interactive Comment

scales (hourly, daily). Already at longer time-scales (months, year) Granier et al 2011 and Huneeus et al. 2011 find larger uncertainties over large regions.

p 28088, l 15: Regarding stabilisation of ensemble spread, this is not obvious for WCONUS

p 28088, l 20: I suggest using brackets instead of commas to delineate "the square root of the sum of the ensemble variance and the observational error variance"

p 28092, l 3: couldn't this be due to insufficient ensemble spread at low AOT? Several authors have pointed out that a positive variable like AOT can only have a large spread at small values if the distribution is allowed to be very skewed (i.e. non-Gaussian, contradicting a basic assumption in a Kalman filter). The small spreads that occur in ensemble runs are a direct result of small source perturbation at low mean source values. I believe this is an unresolved issue.

p 28092, l 7: The case of too small a spread at high AOT may also be the result of missing causes of uncertainty. E.g. you don't perturb deposition processes. Perturbing them will have a bigger impact at high AOT than at low AOT because (again) AOT cannot go below zero.

p 28093, l 17: "since they are independent." The prior and the observations are also independent so this cannot be the reason to choose the posterior.

p 29095, l 7: "performance gains" The authors are undoubtably aware that this comes at a hefty cost: 4x more CPU requirements. I think that 'performance' may not be the best word here as it implicitly suggests some optimal cost/benefit ratio.

p 28096, l 1: It would be good at this stage to point out that NAVDAS-AOD does not include perturbed meteorology (as far as I understand it). I.e. something like Fig 10 is unlikely to be seen for NAVDAS-AOD

Sect 3.3 & 3.4 and Table 3 etc: an evaluation of a base model run (control) should be part of this analysis. Is there even a substantial improvement in AOT due to assimilation

ACPD
15, C9415–C9423, 2015

(either 3DVAR or EAKF)?

p 28100, l 14-19: "On the other hand, the forecasts initialized with the EAKF fields do a better job capturing the leading edge of the dust front with the ENAAPS-DART version being smoother than the deterministic counterpart along the dust front. This demonstrates that the sharpness achieved in the ensemble data assimilation propagates in the forecast and is an advantage of using the EAKF initial conditions over the variational initial conditions for the short-term forecast." The use of 'sharpness' and 'smooth' confused me initially. Unless I am mistaken, they are not juxtaposed but describe different aspects. Consider repharsing this sentence.

p 28100, l 5-19: I think it should be pointed out that a substantial part of the plume (eg the northern edge) is missed by all four forecasts. Please discuss possible causes.

Section 4, Discussion: I suggest removing this Section in its entirety. It is not really a discussion but an extended summary. Its main points have already been discussed (in detail) in the main text. Important conclusions in this Discussion that are not yet in the Summary should be moved there and phrased more consisely.

Section 5, Summary: consider my general comments.

Fig 6: Not quite clear what is shown here. This is essentially the model forecast covariance? So it is with respect to a single location? Presumably the black dot in the top row (there are no dots in the lower rows)? It is the correlation in the AOT fields?

Fig 15: What does "Not all availabale MODIS observations are assimilated" refer to? I realise that the NRL-MODIS dataset is a subset of the official Coll 5 product. But why show here a different product than that which you have assimilated?

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 28069, 2015.