AeroCom-Aerosat Board for keeping discussions, comments, question and answers
DAY 1 / 12 October 2020
Edit your name, or put your name in front of a comment
Use the agenda to insert comments, Q&As in a chronological order
Let us develop this document jointly, in a stuctured way - can serve as useful minutes taker tool
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Plenary Session 1 - Experiment Status [90 min]
purpose: get an overview per AeroCom experiment of submissions, analysis , papers, participants, plans Intro then 12 talks à 5 minutes then 10 min discussion (further discussion in breakout 1)
Moderator: Philip Stier // Rapporteur: Ross Herbert
- Michael Schulz: Welcome and AeroCom Overview [20 min]
Aerocom Experiment Status Summaries
- Jonas Gliss, Augustin Mortier, Maria Burgos, Mian Chin, Dongchul Kim, Hongbin Yu, Gunnar Myhre, Xiaohua Pan, Wenying Su, Maria Sand, Duncan Watson-Parris, Paul Kim (5 min each) [60min]
- Discussion of status [10 min]

Michael Schulz(1400 - 1420)
Hello folks. The board is live! (Bjørn)
Kostas: One thing to note, the mean and median models by Jonas are now available on the AeroCom server as "models" with 1x1 and 2x3 resolutions, if I remember correct.
I wonder if, in times of open data, we should make this more easily accessible, i.e. on an open data repository?

- Michael: What do you mean here?
- Kostas: I am also puzzled. The data are freely available and easily accessible already.

1.
Mian Chin (1420 - 1427)
MS: Great - I thought there was no progress on tracer work !
Valentina Aquila: I didn't understand if all simulations are nudged. Does the nudging impact the vertical transport? Could it be totally different if it was free running?
Kostas: Valentina, yes it might. There are also non-nudged experiments in the control runs (CPL I think?) so maybe worth checking. In general though nudging happens only on the horizontal component of the winds, but it strongly affects vertical.
Haruka Hotta: What is the vertical resolution of models? I guess MIROC might use rather coarse vertical resolution.
Mian Chin: GFDL does have nudged (nSST) and free GCM with fixed SST (fSST). They are quite different in vertical transport. I will send you my slides.
Mian Chin: Vertical layers: GEOS=72, GFDL=33, MIROC-SPRINTARS=40
Toshi: I found a bug in the VOL0 experiment in MIROC-SPRINTARS today. I'll rerun as soon as possible.
Toshi: I'll check why CO concentration underestimates in MIROC-SPRINTARS. Probably it's also a bug (I'm participating in too many experiments..., so my chck is lax).
Qian: in the vertical curtain plot comparison, GEOS and GFDL model has some very similar feature.What is their similarity in model design?

Jonas Gliss (1427 - 1433)
Nick Schutgens: did you look at AP2 as well? Bit surprised that BC loads were high in AP1, low in AP2 and at middle ground in AP3
Jonas Gliß: Unfortunatly, we focussed on comparison with AP1. Perhaps changes between AP2 and AP3 are related to treatment of mixing (internal vs. external) of BC in the models and associated changes in deposition efficiency. We found that in AP1, more models treated BC as externally mixed, while most of the AP3 models assume internal mixing (e.g. with organics or sulphate).

Paul Kim (1435 - 1441)
Zak: What do the number concentration plots from ERA-Interim and ERA-5 represent? These are purely meteorological reanalyses without prognostic aerosol.
Paul Kim: the reanalysis is used to calculate trajectories and these are linked to observations made at the station.
Paul Zieger: What about sources of Aitken mode particles? Maybe some processes are missing?
Paul Kim: probably! I will be looking at source processes as well as sink processes, only one example shown today but multivariate analysis needed to get the best idea of what's going on.
Nick Schutgens: could you use AEROCOM simulations to pick interesting observation sites? (OSSE stye study?)
Paul Kim: definitely, I am interested in doing this.
Athanasios/Thanos Tsikerdekis: Hi, great presentation! How did you determined that precipitation during transport has little effect? I was a bit lost at that point. Thanks!
Paul Kim: Hi Anthanasios, thanks! Precipitation during transport had a noticeable impact when looking at observations linked to reanalysis. However, in the model, this impact was not as obvious. The concentration of

100-200nm particles (and more generally the average size distribution) is unaffected by the amount of experienced precipitation during transport. I hope that makes things a bit clearer!

Augustin Mortier (1441 - 1448)
Steve Smith: One issue with current inventories used by models is that they do not include primary mineral particulate emissions from combustion (mostly coal ash) - and I'm not sure if the models have parameterizations for these emissions. I'm wondering if this could contribute to the issues with coarse mode and PM10? (This is basically a missing emission source, particularly for coarser aerosols.)
Kostas: I don't think any model has such an aerosol included. How different are they from other aerosols? As passive tracers they should be easy to introduce, but for direct/indirect effects more work will be needed.
Augustin Mortier: I have no experience with the parametrization of these particles in the models, so I cannot provide a clear answer here.. However, I can say that over North Africa and Asia, where we have relatively larger coarse mode contributions, we actually have quite good performances regarding the coarse mode AOD. the bad performances are observed in Europe, North-America and South-America
Qirui Zhong: Could you please explain the strong postive trends of PM2.5 and PM10 over Arctic?
Augustin Mortier: This question was also risen by one of the reviewer. We have two hypothesis: a change in the air mass circulation pattern, or the increase of open sea, which might contribute to a higher production of sea salt aerosols.
Larisa Sogacheva: Based on the satellite data, AOD trend over China is not linear during the last 20 years (Sogacheva et al, ACP 2018, Part 2) with clear AOD maximum around 2011 and following decrease. Same for SO2 and NOx (van der A., ACP, 2017)
Augustin Mortier: Thanks for the references. This study focused on linear trends only, so we could not detect any break point in the derived trends. In an earlier version of this work, we included the possibility of having break point in the time series, and also noticed an increase followed by a decrease in this region. Associated to the observed trends, we conducted a representativeness study, using subset model data. This representativeness study revealed that the linear significant decrease observed in Asia (with Aeronet) is not representative of the actual trends in the region and in the study period, due to the partial observation coverage in space and time.
Larisa Sogacheva: by "actual trends" do you mean trends calculated from modelled AOD ?
Augustin Mortier: yes, in my sentence, 'actual' was a shortcut for 'model data using all of the gridboxes and timestamps available'. I agree that this might not be the truth, but it shows at least that the gaps in the observation dataset (in space and time) conducts to different trends.
Larisa Sogacheva: yes, both satellites and AERONET are available at clear sky only. But monthy data show the truth quite well.
Augustin Mortier: I agree that the monthly data can give a realistic overview of the AOD. By gaps in the observations, I meant that the number of stations is not so high in Asia, and in addition this number changed quite a lot during the study period (2000-2014) starting with very few stations. This change in number of stations might implies artificial trends, especially if the new stations are located in different backgrounds. We have to consider this effect with our 'regional time series' approach, which combine the measurements, and not the trends.
Lucia Deaconu: You could increase the number of ground based obs in China by using the CARSNET network (on request only I'm afraid). However, I'm not sure if it covers entirely your trend analysis period ..
Thanks! Will have a look!
Svetlana Tsyro (regarding coarse PM): Anthrpogenic primary coarse PM are tricky. In EMEP runs for Europe, we use country reported PM25 and PM10 emissions, which should be including inorganic dust from diff. industries and production processes, and also tyre/road wear, but... all those are so uncertain. We typically underestimate PM10, as there are other sources, unaccounted for, suche as road dust, agricultural dust, resuspension...not to mention primary biogenic PM.
Svetlana Tsyro (Arctic trends): Think it's been seen by others too. A citation from AMAP report, in prep.: In term of trends in 34 years of aerosol chemistry (Sharma et al., 2019), all aerosol components have declined especially eBC (absorption) and sulfate (scattering) at Alert (and other locations as shown above) except for NO3 and sea-salt (NaCl) where there are increasing trends in both.

Gunnar Myhre (1448 - 1453)
Kostas: How come you are not using the GISS models? Is there something missing?
I could not find it on the AeroCom server. Let me know where it is, very happy to include it.
Tero Mielonen: Did you have all the historical volcanic eruptions included?
Only the GEOS model had all volcanic eruptions included. They will redo the simulations without these emissions.
Thanks!
Duncan: Some of the models have present-day anthropogenic AODs of >0.05 what are the corresponding total AODs? They must be very large!
Will check, thanks!
Alcide Zhao: what are the differences between this and the CMIP6 historical simulations?
Will have a look.
Yves Balkanski: Any plan to get a timeserie of SSA from Aeronet and compare with the trend you show?

I showed only the anthropogenic, uncertain if this can be seen in the Aeronet data since 1995-2000. We'll check over some industrial regions.
Twan van Noije: We can provide data from EC-Earth3-AerChem CMIP6 historical simulations. These are coupled runs with double radiation call. Let me know if you're interested.
Great, very happy to include it!
Paul Ginoux: I am not sure if you are going to calculate RF or I have to provide it for GFDL-AM4?
If you have double radiation calls, that is very useful to include!
Paul Ginoux: I will check if we have saved it for that CMIP6 runs
Thanks!

Xiaohua Pan (1453 - 1459)
Ramiro: Any plan to analyse years beyond 2008? The wildfires from late 1990's seems to be very different from 2000's in Central Asia.
Xiaohua: Ramiro, thank for pointing it. In this experiment BBEIH, we chose 2008 because we intended to compare the field campaign ARCTAS in 2008.
Ranjit Solanki: what is the reason for the higher AOD values?
Xiaohua: Ranjit, can you specify your question? Higher AOD in which dataset?
Qirui Zhong: For the difference between FEER and GFED, have you validated the model against observation to see which inventory is better?
Xiaohua: This is the next step. I will do more validations to answer this good question.
Andy Sayer: I am a little suspicious of some of the MODIS retrievals right by the northern hemisphere "no data" line. I would check to make sure these are not being contributed by a very small number of days. I worry it could be cloud or snow contamination and not real aerosols. This is often the case where you see a hotspot right by lots of "no data" grid cells. Sadly there is no easy way to count the number of contributing days from the MODIS monthly products. I tend to either make my own composites from daily level 3 data, or else manually look at true colour and level 2 data on Worldview. Takes a while but can provide evidence either way that you can't get from level 3 data.

- Xiaohua: Andy, thank you for your insight. I would be very interest to know your result of check. I simply took the monthly mean data from Giovanni.
- Andy: Xiaohua, I can take a look some time. Could you confirm the exact data set (not sure if it is Dark Target or Dark Target + Deep Blue merge?) and version (should be collection 6.1 ideally) and time period (was it Aprils from 2008-2016?)
- Xiaohua: Andrew, here is the file name containing the information you ask (MODIS C6 and other), g4.subsetted.MOD08_M3_6_AOD_550_Dark_Target_Deep_Blue_Combined_Mean_Mean.20080401.180W_90S_180E_90N.nc
- Andy: got it, thanks! Ok that is Collection 6 data so it is out of date. :(I don't tend to point people at Giovanni because it's not under control of algorithm teams and tends to update on a delayed cycle. Maybe they have collection 6.1 now though, I will check. 6.1 had some improvements to snow masks and smoke masks in Deep Blue, which could make a difference. (you are using the DB/DT combined product which is probably the best version). Might be easiest if I write a file for you to read from instead if you just need an aggregate map like you showed. Will check in with you in the weeks after the meeting. :)
- Xiaohua: Sounds great, Andy! I would contact you after the meeting. Thanks!
- Andy: you're welcome :)

Xiaohua: aerosol observations like surface concentrations over Russia, Arctic, or Canada in 2008 would be appreciated to validate our model outputs. If anyone knows or has these kinds of data, please contact me or Ralph or Mian. Thanks.

- Ralph: plume heights for Siberian fires become available from MISR
- betsy: do you want surface aerosol data? if that would be helpful betsy.andrews@noaa.gov
- Xiaohua: @Besty, thank you! I will contact you.
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Maria Sand (1459 - 1503)
Kostas: The GISS model is GISS-E2.1-OMA, not E2.
Ross Herbert: Is the model diversity due to model differences in local emissions or transport? i.e., the peak in diversity seen at ~ 30N
Paul Ginoux: Very interesting your finding about NaCl. I am looking forward at your recommendations.
Athanasios/Thanos Tsikerdekis: Hi, great presentation! ECHAM-HAM and ECHAM-SALSA had a huge difference in AAOD, do we know why? (Thank you Harri!)

- Harri Kokkola: The aging of BC and dust is treated differently in HAM and SALSA. In SALSA, aging is much slower causing long lifetime for BC and dust.

Duncan Watson-Parris (1503 - 1513)

Mattia Righi: are model requested to sample the data along flight trajectories or do you this "offline"? Which (minimum) temporal resolution is required?

- Duncan: Thanks for the question! If you can do it online that's great, but otherwise I can provide tools for sampling offline from, ideally, 3 hourly output (at worst daily).

Ramiro: It would be great to summarize the simplified requirements for this experiment.

- The simplified requirements refer the time period. I used to require the full hindcast but now only 2017 is required. I'd still like as many of the requested diagnostics as possible, with a focus on the aerosol microphysical properties. Thanks!

Bjorn: Great presentations. On the PPE, can the constraints be done on absorption? Would be very useful!

- Thanks! We're using AAOD and BC MMR to constrain the BC RF and semi-direct effects, is that what you mean?

Nick Schutgens: can you constrain these three parameters while a host of others are kept constant? Mathematically it's possible, but how realistic would it be? I'm thinking e.g. of sulphate emissions that will have impact on BC aging etc.

- Good question! I think this is OK to first order, but it would be a good question to explore using a full PPE - we should talk about this :-)

Schuster: Nice talk. Are you really varying BC IRI from 0 to 0.8? That's way too large of a range, in my opinion.

- Thanks! Good catch. We're varying BC IRI from 0.2 to 0.8 to roughly match the spread from the Bond et al. We use the IRI=0. experiment to split the instantaneous BC radiative effect from the semi-direct effect (following Ghan et al. 2013).

Ahh -- I see. BB06 recommends IRIs of 0.63-0.79, though.

- Yes, it's probably a bit too broad, but it's better to have a broad set of training data and then constrain with the obs than find we can't match the obs with any of the values and then have to perform more runs. It's a tricky balance.

I dunno. IRI = 0.2 is no longer BC. We can chat at one of the mixers, though.

- Sure, sounds good!

Hongbin Yu (1513 - 1523)

Yves Balkanski: Hongbin, we have a model version that treats the large particles having 4 modes the largest mode being at 22 um. It would be interesting to integrate it in the comparison.
We can talk with Ramiro (Checa-Garcia) in providing you with the files.

- Hi Yves and Ramiro, that will be great.

Ramiro: In deposition did you consider the aggreation of both wet and dry?

- from satellites, we can only get total deposition. The method is only applied to oceans (not over source regions).

Its also up for discussion in session 2 later today

Toshi: Hi, Hongbin. How is the dust deposition flux estimated from satellite retrievals? Please tell me a reference, for example.

- Hi Toshi, it is Yu et al. (2019), JGR-Atmosp. "Estimates of ....."

Dongchul Kim (1524 - 1532)

Yves Balkanski; Hi Dongchul, it was not clear to me on which dust variable your Relative Source Contribution was based upon. Is it concentration, DOD, or? And why. THanks
Kostas: We should do the simulations soon and participate in this experiment!
Mian Chin: I believe it is fraction of DOD.
Dongchul: I presented column loading results, but I have other variables. DOD is in general similar to LOAD.
Paul Ginoux: Interesting experiment. I am going to participate with GFD-AM4_nSST and fSST. Masks are on AeroCom web site?
Kostas: Yes, they should be there.
Dongchul: Yes. Instruction in webpage worked well for other models so far. Let me know if you have questions.

Wenying Su (1533 - 1540)

Kostas: GISS-ModelE-MATRIX should also have all the data you need, please let me know if not.

- Wenying: I will check and let you know. Thanks for bringing that to my attention.

Yves Balkanski: HI Wenying, I would be interested to exchange our respective presentation to discuss whether we could work on these fluxes over the Sahel. THanks. (yves.balkanski@lsce.ipsl.fr)

- Wenying: Yes, that would be great, my email Wenying.Su-1@nasa.gov

Twan van Noije: I missed which simulations you are including in your analysis. We can provide fluxes from EC-Earth3-AerChem CMIP6 simulations. Let me know which ones you are interested in (e.g. AMIP, hist).

- Wenying: I don't think I have seen the EC-Earth3 output yet. I will check. Do you have all variables that I listed in your model outputs?
- Twan van Noije: I am afraid I missed that list. What do you need besides TOA fluxes? We have all variables requested by CMIP6/AerChemMIP.

- The variables are total column AOD, TOA up/downwelling SW flux, and surface up/downwelling SW flux.
- Twan van Noije: we have all of those at monthly frequency. Is that okay?
- yes
- Which simulations? CMIP6 historical or AMIP? Or nudged AeroCom CTRL?
- I used AeroCom Phase III CTRL run for 2010 for this talk. My talk on Thursday will focus on historical run for trend analysis.
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Michael: How do you get to the kernel?
Kai Zhang: A similar question regarding the kernel - what are the assumptions made for the kernel calculations, since the radiation schemes are different in various models?

- Wenying: The radiative kernels of AOD and surface albedo are calculated based on MERRA-2. We also used the kernels calculated based on CERES and MATCH aerosol transport models, the results are similar.

Nick Schutgens: can you interpret your analysis in the light of Bjorn Stevens earlier conclusion that models overestimate radiative fluxes due to aerosol?
Wenying: yes, I think that is the case.

Maria Burgos (1542 - 1547)
Peter Colarco: I am curious, do all of these models use pre-computed lookup tables for their optical properties (GEOS does)? Are there some internal mixtures that complicate this analysis?

- IFS-AER certainly does (and uses external mixing, so mixing state is not a factor)

Nick Schutgens: seasalt hygroscopicity shows hysteresis and the curve for decreasing RH actually goes lower than 40% before going to zero., so maybe TM5 is actually incorrect (sorry, Twan!)?
Paul: Hi Nick, TM5 actually does it correctly when it comes to NaCl
Nick: you mean it models both branches of the wet growth curve? But for that it would need to know the history of the seals.
Zak: I think an interesting question with hysteresis is to what extent it is or isn't important to capture both branches when averaged over a large grid cell.
Nick: agreed, Zak, that was what my question was leading up to.
The interesting thing is that inorganic sea salt has actually a lower hygroscopic growth than NaCl. Most models have it "right" due to the wrong reason. The models think it is NaCl but take a too low value (kappa=1.2 compared to kappa_NaCl=1.5). Concerning the hysteresis: yeah, probably not important in real life of large grids. We can discuss this tomorrow in the breakout ...

Discussion of status...
I think the slides and the discussion in board.net provide a very nice overview -
Maybe the oral discussion can be picked up in plenary sessions 2+3?

- this sounds like a sensible plan :)

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EU:3:45–5:15pm/NY:9:45–11:15am/CA:6:45–8:15am/JP:10:45pm–0:15am/CN:9:45–11:15pm
STARTING 15 minutes later on the full hour
Plenary Session 2 - Aerosol and component life cycle diversity [90 min]
focus on: remaining issue, recommendations for modelling, evaluation issues, proposed AeroCom activities 5 talks 5 minutes + 12 minutes discussion
Moderator: Mian Chin // Rapporteur: Pete Colarco
- Michael Schulz aerosol life cycle
- Hongbin Yu Dust lifetime and size
- Maria Sand BC and absorption
- Huisheng Bian Nitrate
- Kostas Tsigaridis Organics

Michael Schulz aerosol life cycle
Kostas: the fact that organics have the lowest inter-model diversity, less than sulfate, is shocking to me.
Tero: EC-Earth is using TM5 so that explains the similarity with TM5

Zak: Indeed, it appears to be the case that (at least some of) the short lifetimes in the IFS are "too short", based on the low bias of the model compared to its analysis. This is somewhat improved in more recent versions, but the gap has not been closed completely.
Nick: I guess you could ook where most of the underestimation happens; near sources or in outflow. In the first case, emissions are too low, in the second case, lifetimes are too short.
Zak: inded Nick - in different cases we can see both of these as issues to address
Yves: We have recommendations to give for the dust size distributions of dust based upon measurements compiled in the literature (Kok et al. 2018, Ryder et al., 2018; Di Biagio et al 2020; Adebiyi and Kok 2020; upcoming paper from Samuel Albani).
Ralph: Model diversity is not the same as model uncertainty, as we know. Should there be more emphasis on model-measurement inter-comparison, and some specific focus on measurements that are not yet available, or not available with sufficient coverage or accuracy? Without measurements, model inter-comparison does NOT provide uncertainty. +1
Duncan: I agree.
Paul Ginoux: For large particles the deposition scheme will provide the main bias. I think Jasper Kok is tetsting that for dust but sea-salt will be even more important. We need to test the different schemes.
What we need to do Paul is to input the same size distribution in several models which is what Jasper is doing (Yves)
Zak: for the balance of very high emission and deposition rates typical with dust and sea-salt, the numerics of their coupling to boundary layer mixing and any operator-splitting involved is also crucial
Ross Herbert: it would be useful to see the table you presented normlaised by the impact on RF - so some species may have a large variability (between models) but not too much impact on the variability of RF. find out who the troublemakers are...

Hongbin Yu Dust
Michael: One question might be how comparable Phase I,II, III results are - model ensembles have changed considerably.
How do you compute diversity? SD?
diversity = standard deviation/mean value
Yves: A logical experiment to promote is to include the size distrbution for dust descirbed here (see refs) into all models: (Kok et al. 2018, Ryder et al., 2018; Di Biagio et al 2020; Adebiyi and Kok 2020; upcoming paper from Samuel Albani).
For a modal distribution that boils down to 4 modes MMD: 1.0, 2.5, 7.0 and 22.0 um and sigma 1.8, 2.0, 1.9, 2.0 (see Di Biagio et al 2020)

- good suggestion.

Kostas: Maybe we should discuss on unified size-resolved diagnostics for all models? I mean beyond PM1/2.5/10 and AODf/c. Emissions, removal, forcing...

- betsy: yes!! this could be useful for things besides dust (like hygroscopicity). something like the Mann et al size distribution analysis would be great except for the full size range. also for understanding some of the underestimates of AODf/AODc/SAE reported in multiple papers (e.g. gliss2020 etc)
- most models don't provide size information.
  - yes - but they can (at least some of them) if it is requested.
  - we requested the size info. But so far we only have got from 8 models (~50%)
- I'm hoping to dig in to this with the aircraft experiment where we have lots of in-situ size distribution data. There I just request the mode radii and compute size cut-offs myself to simplify the diagnostics.
  - for inlet-based in situ observations, it is important to keep in mind that such methods have cut-off size, ie., undersample or not sample at all coarse particles.

Sophie Vandenbussche: dust observation from Thermal Infrared are "dust only", not sensitive to other aerosols -> AOD, mean altitude and full profiles (now resolution) are available, in addition there are retrievals of mean dust aerosol size - should not be considered "proxies" I think
Thomas: But there is an additional uncertainty due to the covnersion from 10 micron retrieval to 550 nm

- Yes, the size (to a less degree, shape) determines the conversion. Models need to get the size right.

Sophie: True, but the vertical distribution would remain similar, and the aerosol size does not depend on that. If we consider only coarse mode of course. Because indeed TIR is insensitive to small particles

- additional thought here: why are models "limited" to UV AODs? can't they also compute TIR AODs to then compare with the TIR pure dust products?

Ramiro: Beyond the dust size we have to remember the shape assumption that can produce also bias in the scattering/absorption optical properties.

- yes. another thing is dust mineralogy and its variations.

Andy Sayer: when you use satellites to evaluate the model dust, are you using monthly data? If so going to daily is probably best due to very different sampling of data sets. There are huge gaps in the satellite data in the

tropics due to clouds, swath width, etc (plus measurement time as Sophie rightly mentions just below). Monthly composites don't average it all out. Your evaluation may be biased as a result. Also, use of means is probably not good because AOD distributions are skewed and AOD retrieval biases are AOD-dependent. So I would not expect consistency between satellite products. Essentially, I think the way the satellite data are used to evaluate models, particularly in the case of dust, could be influencing our interpretations and steering us wrong. +1

- For dust deposition and lifetime estimates, we can only do on monthly/seasonal time scale. Certainly it is helpful to use hourly or daily products for satellite-model comparisons. Such comparisons can be done fore some episodes, like the dust intrusion into Caribbean Basin in June 2020.
    - Andy Sayer: maybe it is time to discuss as a group what would be needed to produce and archive daily output (at least for e.g. 1995-2020) for some aerosol parameters? I know it is expensive but we can't advance these methods if we don't make the step sometime. This is not specific to the dust stuff (would also need to deal with e.g. emissions being monthly and not real daily for some species) but a broader AeroCom/AeroSat issue. And yes maybe this summer's dust storm could be a case for looking at consistency between output from daily vs. monthly collocation for dust.
- Adam Povey: Indeed, the measurements will likely filter out the thickest plumes which will have some manner of effect on the size distribution. And the retrievals don't necessarily account for very large particles themselves.

Sophie: when comparing to satellites, also keep in mind the measurement time: close to sources it might have a huge impact!
Duncan: We could perform these sensitivtiy experiments as PPE?
Nick: shape of dust also impacts its transport, do we need to look into this? (https://eos.org/articles/have-we-got-dust-all-wrong&utm_campaign=ealert)

- Andy Sayer: Nick, thanks for sharing that, I saw it in EOS recently too and was thinking it'd be good to discuss this week. It would be interesting to quantify how much satellite retrievals change if there is a preferential orientation. It complicates the radiative transfer somewhat (single scattering properties). I am not sure who/what tools would be best to look at this. Everything I have assumes randomly oriented particles. Note this would also affect the AERONET inversions used for size distributions etc.
    - A few years back, Sasha Marshak's group looked into CALIOP depolarization ratio and inferred the evolution of dust shape along the trans-Atlantic transport (sorting). Non-spherical particles stay higher in the atmosphere than spherical particles do.
- Claire Ryder: I think that may come out of the work in the link (Vasillis' group), Andy. I am also looking again at our aircraft measurements with Jasper's group to see if we can infer dust orientation in a useful way to give more data on this.
    - Andy Sayer: Claire, sounds good! From the EOS feature it sounded like this is ongoing measurements so maybe we can get input from them at a future AeroCom meeting. Will check there and Vasilis' webpage later in case one of the links had tools for oriented particles already.
- Ron Miller: Vertical orientation of dust particles is a provocative idea. However, it seems like this would reduce aerodynamic drag and increase the rate of gravitational settling and removal. Our modeling challenge is the opposite: our models have insufficient coarse particles compared to measurements suggesting that they are removed too fast.
- Hi Ron, Huang and Kok (2020) suggest that accounting for triaxial structure of dust particles reduces the gravitational settling velocity by 15% and hence increases the gravitational settling lifetime by 20%. Accounting for this effect would keep more coarse particles in the atmosphere.
- yes, that is what I proposed.

Sophie: a question: how do you calculate those parameters (as lifetime, deposition) from satellite?

- It is published in JGR (Yu et al., 2019). We calculated dust mass fluxes (meridional and zonal, i.e., DOD is convolved with reanalysis winds) and used "mass-balance" approach to get the dust deposition. This method can only applied to oceans (without dust sources) and over monthly/seasonal time scale.

Adam: I've had a student have try to calculate deposition from satellite using CALIOP. He concluded it was better to basically convolve AOD fields with modelled wind fields, which is what most of the ocean colour people do when they want to estimate iron inputs. (so, in short, ask CAMS).
Sophie: I've done a similar thing to evaluate dust sources with IASI. But this is far from being quantitative so can't really explain what was done here ;)

- I don't know how you can estimate dust deposition in source regions. Emission and removal processes determine the dust mass in the atmosphere. It is formidable to separate them from satellite measurements. Our method can only be applied to oceans (where you don't have dust sources).
- Adam: Ah, handwaving. The most rewarding of sciences :)

Thanos: Hi Hongbin, great presentation. Just a brief comment. You mentioned that AOD is not a strong constraint for dust in data assimilation. The inclusion of other observations related to particles size and absorption (e.g. AE and SSA) would certainty help with that.

- Thanks, Thanos. Yes, additional observations could give a stronger constraint. But we don't have such observations on large scale. I believe that IF a model predicts fractions of individual components right, the assimilation of AOD could impose strong constraint on dust optical depth. But that is big IF.
- Thanos: For AE I think quite a lot of satellite provide AOD in two wavelengths. Indeed we don't have absorption observations from satellites (large scale) from multiple satellites. But some satellites can give you SSA (e.g. POLDER, OMI). The uncertainty of these absorption observations may be large, but it can certainly constrain dust a bit more than just AOD. So until we are certain that we can predict a large fraction of individual components right in the models, maybe the inclusions of other observations in data assimilation process is our best bet.

Maria Sand BC and absorption
Yves: Models should underestimate AAOD in dust regions since they do not account for very large particles that are much more absorbing than small ones. Do you infer that from your study in dusty regions?
Michael: So one problem here is really the optical calculation and how to diagnoste component contributions (mixing of BC,OA what to attribute to what?)

- Bjørn: That's certainly part of it, but there will also be transport and deposition differences.

Kostas: internal mixture in some models is an issue and a reason to exclude models from such studies. We should try to find a way to overcome this limitation, but not sure if there is a simple answer to that.

- Schuster: I don't understand why we would have to exclude models with internal mixtures. We can compute the effect of BC by computing AAOD with all absorbers and then compute AAOD without the internally mixed BC. Then resulting difference is an effective BC AAOD.
- Sure, but there are strong non-linearities in this approach.
- How?
- Size also changes when you do that.
- Yes, but is still captures the effect that BC is having on the atmosphere, which is what we are after.
- Agreed, but it will greatly vary by model, because of different underlying size assumptions. For example, a mass-based model might have 2-3 sizes which don't change, and aerosol microphysics models might end up having BC all the way to the coarse mode. Still, I get your "effective BC" argument. I am not saying it is not useful, but I do believe it might be hard to interpret across models.
- Of course. But size is part of it. As Tami Bond likes to say -- BC is not the only thing coming out of the tailpipe, and it does not necessarily make sense to try to isolate a single species when it is always co-emitted. So we pull the whole BC mixture to understand the effect of BC.
- I totally agree with that! Which is one of the reasons why I am not a big fan of deleting one species in sector studies, instead of deleting full sectors, which include co-emitted species. Anyway, I think we agree on that.

Jonas found also larger total BC MAC then total BC MEC for some models....
Ralph:Many models do not distinguish Black Carbon from Brown Carbon, despite very different physical and chemical properties, differences that can often be identified observationally. In the current model inter-comparisons, is this taken into account?

- Bjørn: Brown carbon is what is listed as 'organic' in Marias presentation. The separation is of course very model dependent, but we've used what each model has chosen to report

betsy: is it possible to look at a narrower wavelength range for AAE? there are a lot of surface absorption observations for the ~mid440-mid660 nm span

- Bjørn: We're only constrained by what the models calculate, so this is possible if it's asked for. For this particualr experiment, though, we asked for three specific wavelengths so as not to make the output excessively heavy.
- Greg: AAOD is super small, too. I am not sure that any curvature effects in the AAE are significant or measurable. I know that Tom Eck looked at AAOD curvature a few years ago, but when we are talking about a parameter that is less than an order of magnitude below AOD, I feel like we are dancing in the noise.
- Tom Eck: Greg, I agree with that in general the AAOD are too small and we may be overcome by noise, but at high AOD levels I think there should be sufficient signal particularly since SSA uncertainty is lower at high AOD. I saw spectral AAOD variation (spectral variation in AAE) at sites in China where the AOD were the highest in the entire network.
- Yes, high AOD helps. But I did a perturbation analysis of AAOD uncertainty. If dAOD = 0.01 and dSSA = 0.03, then dAAOD > 0.01 when AOD(440) = 0.4. When AOD(440) = 1, dAAOD = 0.02. I didn't calculate dAAE, but I suspect that it will be difficult to accurately quantify the curvature.Having said that, I know that you are very precise, so I am on the fence.

- Hi Greg and Tom, I prefer to look at western Africa where we have high AOD and AAOD by biomass in NH winter and dust in summer also in order to understand AAOD spectral dependencies associated with DU and BC. In addition the 440/670 also the complementary 670/1000 ratio is intresting ... and yes also FMF helps to indentify teh likely absorption type.
- I did look at the AAE as a function of FMF for the Ilorin site in West Africa during the biomass burning season (Nov-Mar) in my 2010 JGR paper (Fig 17). This site also has very high AOD and therefore uncertainty in SSA of ~0.01. I did not see spectral variation in AAE at Ilorin (440-675 nm versus 675-870 nm AAEs). The AAE varied from 1.4 at FMF of ~0.75 to 2.1 at FMT of ~0.2 with minimal wavelength dependence for the fine mode and dust dominated cases (or even mixed cases). This was done with V2 data so it may differ with newer V3 retrievals.
- interesting!
- yes for BC the AAE is pretty much close to 1.0 independent of solar wavelength, but not for dust ... this is important so hat dust absorption is not wrongly assigned to BC.
- The AAE = 1 only holds in the small particle limit. Open fractals can get away with this assumption, if they are opened up enough. Collapsed aggregates (aged) have AAE < 1. See my 2016 ACP paper, part 2 for a bunch of details. The AAE for dust can be anything, because size matters. Also, internal mixing changes the BC AAE.
-

Nick: how do you calculate species AE for ECHAM-HAM? It uses internal mixing...
Alf Kirkevåg: The method for calculating MAC for internally mixed aerosols matters quite a lot. For BC we could get almost anything between 3 and 20 m2(g globally averaged (in NorESM1.2), depending on the assumption. Related to the assumptions about volume weighting of AOD or ABS/EXT for internal mixtures.

- Bjørn: Alf, how do you then choose what you use in the end, for your particualr model? Observational constraints?
- Alf: We try to start with what we think are the correct basics (refractive index, size, mass density, observationally constrained), then end up with whatever comes out... No "tuning" towards what we think is the true end result. But the weighting assumption is perhaps out-dated. Why not calculate optics for each component by separate calls with all except that component, and then subtracting to see its effect on MAC of MEC. More expensive (and not linear anymore) but I think more correct. This is discussed in our 2018 GMD paper (for CAM5.3-Oslo(NorESM1.2).

Michael: Has anybody implemented BC and brown carbon mass budgets in a consistent way?
Michael: Should we test the BC and mixing assumptions in several models? Specify input to optics calculation?
Alf: perhaps easier to do sensitivity tests w.r.t. the amount of internal vs. externally mixed mass no structural changes needed)?
Philip: not so sure about this. Personally, I think it is probably second order as compared to the uncertainty in BC refractive indices to start with. Internal mixing of BC in microphysical models is relatively quick (much faster than lifetime).
Alf Kirkevåg: If you read my comment above, you can see that we have found large sensitivity, not physically based, but method-based. The method of assuming that speciated extinction (and absorption) for internal mixtures are proportional to the volume fraction (for each aerosol size), which e.g. leads to sulfate and sea-salt "borrowing ABS from BC when internally mixed.

Paul Ginoux: GFDL AM4 can do internal or external mixing of sulfate and BC.

Michael: Kostas has a point, can we remove enhancement of BC absorption specifically as an experiment?
Michael: But size is also changed when BC is mixed...
Greg: How much will size change, though? BC is only a few percent of the particle volume.

Susanne: MATRIX does run with and without mixing.

Huisheng Bian Nitrate
Kostas: from memory, the Henry constant of ammonia for dissolution only is about 0.2, not 100. I guess the two groups of models simply assume a different *effective* Henry due to the subsequent reaction with water and other species, which is pH-dependent.
Huisheng: JPL publication gave Henry law constant of pure water to be around 60 (M / atm). I conducted a unit conversion for any model whose unit was not in M/atm for multimodal comparison. If we further consider NH3 and H2O reaction for NH4, which is pH dependent, the effective Henry law constant is much larger than the value without pH correction.
Michael: So what is the **major** reason for the larger diversity in nitrate load, life time in AP3 models? NH3, HNO3, Dust&SS uptake, organic nitrate?

- Kostas: I would also add, stratospheric contribution of HNO3? I mean, HNO3 that crossed the tropopause moving down. NH3 wet removal will dictate how much you have higher up, that will meet stratospheric-formed HNO3.

- MS: Maybe we should also on the PD/PI differences on this
- Huisheng: I think the drive reason is the chemical formation of NO3 on sea salt and dust. This process results in not only a hugh diversity of NO3 production amount, but also a very different NO3 size distribution. Another important potential reason is the huge diversity of NO3 precursors. NO3 is a secondary aerosol. Its precursor HNO3 has a huge diversity already, up to a factor of 9 among models participated in the AeroCom III nitrate study.

Kostas: GISS-ModelE-OMA has coarse mode nitrate.
Huisheng: It seems we missed this information in the paper.
MS: I recommend a new questionary to modellers on their nitrate life cycle - models have evolved on Nitrate aeosol treatment probably.
Huisheng: see comments below following the discussion between you and Susanne.
Susanne: GISS-ModelE-OMA does include dust contributions to nitrate aerosol partitioning.
Huisheng: Yes, somehow we missed this information in the paper.
MS: Do we need more diagnostics for understanding the role of coarse nitrate / synergy with more size diagnostics in general
Susanne: to MS: we need clearer communication on the parameterizations, diagnostics will not help too much as the sensitivities in the parameterizations are gigantic for nitrate formation.
MS Agreed, so maybe we should formulate a good questionary here?
Huisheng: Generally, AeroCom models treat NO3 dry/wet depositions and dynamical transport similarly like other aerosols. The difference is that NO3 size is ranging from fine mode to coarse mode, which impacts on its dry/wet deposition.The key for NO3 simulation (i.e. the major reason for NO3 diversity in its load, life time) is Chemistry, Chemistry, Chemistry. The NO3 particle size distribution of a model stems from whether and how the model treatment NO3 formation on sea salt and dust particles. Susanne is right, the AeroCom models also use very different parameterizations for nitrate chemical mechanisms. Without having a detail diagnose of the chemistry mechanisms and their parameterizations, the normal budget analysis we applied to other major aerosol components would NOT give us a meaningful understanding.

Kostas Tsigaridis Organics
Paul Ginoux: Are you on the beach in Crete? We should have our next AeroCom over there!

- Philip: I agree it is about time to go to Crete!
- Crete 2022! I guess there is no travel 2021
- You guys are too young you missed a fabulous conference in Crete about 15 years ago. Phenomenal Crete!
- Philip: I wish I was too young but the conference was indeed very good!
- Kostas: I wouldn't mind having my work pay my next trip to Crete for a conference!

Nick: how much is known about SOA formation during/from biomass burning? It seems most studies consider consider biogenic/anthropogenic sources only.

- Very little. I should had added that on my slide, thanks!
- Tero: I've participated in one study: The role of semi-volatile organic compounds in the mesoscale evolution of biomass burning aerosol: a modeling case study of the 2010 mega-fire event in Russia; https://acp.copernicus.org/articles/15/13269/2015/
- Nick: Thanks, Tero, I'm aware of this paper. There is a single paper on SOA formation from BB!!! We know very little :)
- Tero: Yes, that is true! And they have big impact like we show in that paper, especially further away from the fires.

Michael: Before adding more complexitiies, do we understand why the OA burden varies so much today in models? BB, SOA, ++?

- A little yes, we are not completely in the dark. We should probably relax our model constraints a bit, and see what happens with the evaluation.
- What do you mean by constraints?
- Wet removal (solubility fraction), volatility, these are the primary things that come in mind. The range of volatilities as a function of temperature from the literature can give me essentially any amount of SOA I want in the model. Then the hygrophobic to hygroscopic conversion of POA is also a made-up number.

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EU:5:30–7pm/NY:11:30am–1pm/CA:8:30–10am/JP:00:30am–02am/CN:11:30pm–1am
15 minutes later probably...
Plenary Session 3 - Aerosol optical properties [90 min]
focus on: remaining issue, recommendations for modelling, evaluation issues, proposed AeroCom activities 4 talks 5 minutes + 17 minutes discussion
Moderator: Bjørn Samset // Rapporteur: Betsy Andrews
- **Mian Chin** Vertical profiles

- **Peter Colarco** biomass burning, ageing and brown carbon
- **Jonas Gliss** Mass extinction coefficient / AE / fine-mode AOD / coarse-mode AOD
- **Maria Burgos** Hygroscopicity plus size

Mian Chin (5:45E-6:07)
Michael: The better profile agreement is consistent with that BC life time is smaller and diversity is smaller now in AP3
Yves: I believe the outputs from the INCA model are online. If you could include them Mian it would be great. We have nitrate in the model. thanks.
Mian CHin to Yves: I will check
Andy Sayer: Mian, your A/P labels seem opposite from the ocean names on that slide (Atom flight tracks) i.e. A1-A3 were in Pacific, P1-P3 in Atlantic. Not sure if that was a typo or I was misunderstanding something?
Mian Chin to Andy: You are right - I made a typo (or I renamed the oceans)

- Andy Sayer: thanks for confirming :)

Michael : Is "normal" CO really a good tracer here to understand aerosol transport?
Mian Chin to Michael: If they use the same CO emissions and have similar OH
Steve Smith: From a climate/radiative forcing standpoint, understanding the reasons for the SO4 diversity would be a high priority.
Mian Chin to Steve: I don't quite understand why sulfate has higher priority than other species?
Steve Smith: Because SO4 is the largest contributor to anthropogenic aerosol forcing since preindustrial (direct + large contributor to aerosol indirect)
Michael: Which vertical distribution component is important for forcing estimates?
Mian Chin to Michael: Have not looked into that yet...
Philip: I agree that dedicated sensitivity studies would be good. The AeroCom Aircraft experiment, including ATOM and many more campaigns, will provide the strongest constraint yet on vertical distribution - modellers should contribute! We could then organise a mini PPE around this following e.g. the work by Zak in doi:10.5194/acp-16-2221-2016.

- Zak: This paper currently in ACPD is also relevant, doing something similar in NorESM2: Frey et al. (2019), doi:10.5194/acp-2019-846
- Mian Chin to Philip and Zak: Good idea about mini PPE. Let me know you plan.
- Zak: The approach I took in the 2016 paper was based around on/off tests of processes, rather than parametric sensitivity tests; however some of these could probably be reimplemented in a parametric way, and hopefully one that is transferable between models (even if the parameter is simply a scaling on the raw tendency from a particular process).

Yves: It could be that your 10 orders of magnitude is much less than that just because you are not comparing aerosols over the same size range. We need first to agree on the size distribution that is injected into the atmosphere for dust and seasalt, secondly to account for cutoff of measurements (in fact you were just saying it as I was writing) :-)
Kostas: for higher up, the multi-order of magnitude difference might not matter if concentrations are near-zero either way.

- Philip: True to some degree but it does provide an important constraint on key processes, in particular convective scavenging.

Yves: We should design experiments looking at vertical profiles as we get from source regions to remote regions to see how the range evolves with the time we give for processes to take place.
Michael: Wouldnt it be easier / more efficient to implement different parameterisations / formulations in one model, then implementing one process in many models? CESM/NorESM - CAM are open source (github based) models, so one could construct a transparent process.

- Nick: agreed. Sensitivity studies in a single model would at least provide ideas on what controls vertical distribution the most.
- Mian Chin: Yes, we had a bunch of sensitivity studies on that.

Twan van Noije: Please let me know which data from TM5 and EC-Earth3-AerChem is missing for this analysis. We can also provide CO and Radon.
Mian Chin to Twan: Thanks and I will check
Susanne: Yes to Nitrate being incredibly uncertain. But in your vertical profiles you need to consider how very small the concentrations are for NO3. It would be important to look at that closer to the sources.

- Mian Chin to Susanne: Still, even though they are small, they could be important for IN

betsy: i *think* i had this question with your talk but also applies to peters... for the vertical profiles are confident are the model simulations of vertical profiles of RH? since that can have effect on vertical extinction due to hygroscopcity
Mian Chin to Betsy: I mainly compared dry aerosol mass concentrations. For ec550aer profiles, RH matters, but the diversity of ec550ear is much smaller than aerosol species dry mass.
thanks- i was thinking of the ec550 profiles.

Peter Colarco (6:07-6:27

Zak: just to note, CAMS forecasts also include the radiative effects of prognostic aerosol back onto the meteorology (since June 2018), as does the CAMS Reanalysis. It's good to have multiple forecast models and reanalyses including this (which has been common in the climate context for somewhat longer!) We see several degrees of surface cooling around strong smoke events, compared to a control. Whether this is an improvement or not against temperature observations depends on the underlying temperature bias of the model in that region.

- Peter Colarco: Thanks for the update/reminder Zak!

Ralph: Pete -- We have exactly the near-source injection height and particle type maps for smoke plumes from MISR. It is not everywhere daily, and is at ~10:30 AM ± 1 hour, but we can distinguish black and brown carbon. Is there a way to use such data as constraints on your model?

- Kostas: YES! Any constraint on BrC emissions is more than welcome.
- Ralph: Kostas, We separate BC, BrC, dust, and non-light-absorbing aerosol optical analogs at 1.1 km horizontal resolution with MISR. I will send you some examples.
- Sounds perfect, thanks!

Betsy: seems like a combination of surface insitu and column could be super helpful to constrain the vertical profiles when aircraft measurements aren't available

Yves: Absorption has also a role in determining how much you increase the low level clouds. Have you looked if you realistically represent absoption from these aerosols? Yves

Sampa Das: Correct, we are looking into that in our ongoing work.

Susanne: Pete, why would MERRA get the BB height wrong, if the aerosols are assimilated?

- Sampa Das: The aerosol assimilation is based on AOD from MODIS/MISR/AERONET, so while column load is kind of constrained, we do not do do assimilation in the vertical.
- Pete Colarco: Sampa actually looked at the AOD from the forecasting system, and you could see the aerosol DA torquing the predicted AOD all over the place because we put smoke in near the surface but in the real world it was at high altitudes and blown by different winds. It was interesting to see that!
- Susanne: Ah, get it. Has CALIPSO been used for MERRA in some cases?
- Sampa Das: I don't think we include CALIOP data yet.
- Pete Colarco: we do not
- Susanne: Are CALIPSO data to infrequently observed to be useful for assimilation?
- Pete Colarco: I think it can have impact. It's just in a state of still to be developed. I also think it is not prioritized since not available in NRT. Would be useful in reanalysis

Nick: What did you mean by 'accounting for the radiative lofting of aerosol'? Did this require extra model development? It is not a natural consequence of a certain heating profile and its consequences on dynamics?

- Sampa Das: Since free-running simulations might deviate in time from the expected transport, we adjust the meteorological fields every 3 to 6 hours (winds, temp, pressure). But, for the PyroCb case, we did not adjust the temperature in the stratosphere, such as to be able to feedback the impact of aerosol heating on the stratospheric temperature
- Nick: so it is more an adjustment of the assimilation than the model?
- Sampa: correct, we modified the way we do the regular replay/ meteorological assimilation for the stratosphere
- Nick: but this suggests that the original meteorological analysis was rather wrong (not as good as usual anyway). Any idea what caused this?
- Sampa: so the sensors providing the temperature data for assimilation are not able to provide the update at the horizontal and temporal time scale that we would require for accurately simulating the transport of such PyroCb smoke plumes.
- Peter Colarco: And I think it is important that the possible aerosol contamination of the meteorological observations themselves is not really accounted when these data are assimilated, or maybe often drive to discarding data where it is clearly impacted.

Michael D: For the SE Atlantic, what might the role of large-scale aerosol-circulation interactions (reducing subsidence over the ocean) and the model representation of the southern African Easterly Jet be in causing the low plume height bias? Also, may the high base bias be in part a CALIOP artifact (signal saturating and not seeing all the way down)?

- Rich Ferrare: Michael. yes, you are correct regarding CALIOP artifact. There are alternatives ways of using CALIOP data that reduce/remove such artifacts. Tune in to my presentation on Wednesday.
- Peter Colarco: Agreed difficulties with that, and aware of some of the improvements with the dataset, but CALIOP can see the plume top altitude quite well and the model clearly undershoots that.
- Greg: CALIPSO has extra problems in the South Atlantic these days, the so-called Southern Atlantic Anomaly. Largely associated with the age of the instrument and interaction with the Van Allen radiation belts in that region.

Jonas Gliss (6:27-6:42)

Greg: The sum of absorption AAOD does not equal the total AAOD when the aerosols are internally mixed. Think of core-shell, for example.
Kostas: I will also discuss a little bit on clear/all-sky AOD on Thursday. I would like to see even speciated clear-sky from models, not just total.
Twan van Noije: how would you attribute extinction to NH4 since refractive index is based on ammonium nitrate (at least in our model)?

- Kostas: In GISS ModelE, NH4 is part of SO4/NO3, so sulfate AOD includes some NH4 mass.

Mian Chin: Yer 2020 is a COVID year, which would not be the best reference year

- Jonas question was whether one should set up a new CTRL experiment, which could be still the meteo year 2010

Maria Burgos (6:42-7:00)
Paul Ginoux: Do you have the reference for inorganics sea-salt hygroscopic growth?
Paul Z: The sea salt work: https://www.nature.com/articles/ncomms15883

- Paul G: Thanks.
- maria b: also in this morning's presentation I have inserted all the links to the references I have used, as well as to the published paper. In case you need to look for something else.

Zak: the growth line for sea-salt in IFS-AER doesn't look quite right – our lookup table has no change in radius or density until 40%, rather than a linear increase all the way from 0%. This hasn't changed in recent model versions. See Table A2 of Bozzo et al. (2020) – doi:10.5194/gmd-13-1007-2020. The values originate from Teng et al. (1997).

- Maria B: To calculate it I used scattering values at RH=0 and 40. Is there no way that scattering at 40% shows an increase with respect to RH=0?
- Zak: There is growth from 0% to 40%, but if you look at that table, it all occurs after 30%, not linearly all the way from 0%.
- Maria b: but in our results it looks linearly just cos I have two points, 0, and 40% RH. Does this make sense to you? If I study scattering at RH=25, 30, 35...% we would see what you mean or?
- Yes, that makes perfect sense, I just hadn't realised those curves were backed out from the limited RH sampling of the submitted datasets rather than representing the actual formulation in the model. It's possible then that ours isn't the only model which has a nonlinear growth in this region but you simply don't see it from the requested data? :-)
- Exactly!
- Yes, I know... I should have explained it in a more detailed manner.
- No problem - that makes perfect sense now - just needs a bit of thought how to interpret the results.
- Thanks! :)
- However, there's an anomaly I don't understand myself in the extinction lookup table – although the physical properties (size and density) don't change at lower RH, the mass extinction coefficient appears to decrease from 0 to 30% before then increasing as expected when the particle grows. I'll have to consult those more familiar with the Mie code that generated this table.

Mian Chin: If GEOS GOCART has too high hygroscopic growth, then why the GOCART model H2O fraction AOD is lower than others?
mu
betsy: partly could be because we are looking at specific RH values (high RH=85%) but not ambient. so if gocart ambient RH is lower than other models than perhaps that is why water fraction is lower than other models (goes back to my question about how do the model RH values compare amongst models (and reality)?)
Nick: are you able to say anything on dust wet growth? Same for BC? Which is right (hydrophobic or hydrophilic)?

- Maria b: Most of the models assume it to be non-hygroscopic. We have compared it to a site Niger, which actually did not show hygroscopic growth, so for us made sense that they assumed no hygroscpic groth.

Paul Ginoux: Models cap max RH growth. Do you have recommendation?

- Paul Z: Our observations were usually not above 90% RH, so this is our limit. Concerning the sea salt, I was really happy to see how well the curvature of f(RH) for sea salt was captured and how well it compared to lab and field observations
- Maria b: for models we also only had values up to RH=85%, so we could not study large RH range

Yves: It is hasardous to say that dust does not grow. Many lab measurements show the possibility of growth once heterogeneous chem. has taken place on the surface of dust. +1

- Nick: but that chemistry will be due to condensation of e.g. so4, not?
- Not only so4, all acids stick to dust quite efficiently (see HCl and HNO3!!!)
    - Paul G: If there are still some Ca2+ or Mg2+ available for HNO3 after H2SO4 has been titrated!
- Kostas: no, this is heterogeneous uptake of SO2 and HNO3.

- Nick: sure. But the wet growth may then (possibly) be modeled by those species. The dust itself is still hdyrophobic
- but these species are stuck on the dust surface...
- Nick: yes, I understand that. But this does not change the kappa of the dust. Only the kappa of the internally mixed particle (which is primarily dust). Does that make sense?
- Well the mixed particle definition is between 2 aerosol components, so gas+particle is it mixed?
- Greg:What about illite? Some studies indicate water uptake for illite.
- Susanne: When I studied this with our model, GISS, the water uptake effect on dust was rather small compared to the overall effect of dust, due to its dominance by mass.
- Yes Susanne you are right (as often!)
- Nick: Susanne: are you saying that there is another effect beyond kappa mixing that may have a more profound effect on wet growth of mixed dust particles? Because I interpret what you just said as: "dust is mostly hydrophobic" (which I would expect)
- Susanne to NIck (hi!!!) I looked at it in terms of effect on radiative forcing. In terms of inorganic aerosol composition, the aerosol mass including water is not insignificant, and it does impact the lifetime effect of dust mass a lot. But not its optical properties that much, but there will be another level of significance to this when it comes to ice cloud interactions, then those details can matter a lot, potentially.
- Nick to Susanne: Hi!!! And thanks for the explanation. Let's catch up on one of the social thingies.
- Yves: Good talking to both of you and I would have
- said hi first!
- Hei Nick and Yves lets go for dinner! (hahaha - crying emoji)
- everything becomes virtual these days , good night to both of you.
-
- Greg: Any thoughts on g(RH)? Wet radius / dry radius? This could be another constraint for the models.
    - Paul Z: @Greg: f(RH) captures the entire size distribution, while g(RH) is mainly measured for fine mode particles using HTDMA
    - Greg: Well, f(RH) is usually fine mode as well. But we don't need to abandon f(RH) -- g(RH) would be an additional constraint.
    - Paul Z: Agree. For us the main missing part was info on the size distribution.
    - Greg: Right. That's why I thought it might be helpful.
    -

Ralph: Seems there is a fundamental need for *systematic* aircraft measurements of hygroscopicity (along with other particle microphysical properties). This will be discussed a bit more in later sessions this week.

- Paul Z: There is a lot of data already around, which is not fully exploited. One thing I see is that controlled lab experiments (e.g. with respect to aging) could be added to the analysis. One thing that Maria mentioned: It is important that the models first should try to reproduce the observationally-based relationships of f(RH) with organic or inorganic mass fraction first. In addition, pure component f(RH) from models for organics, sea salt, etc could be compared to lab- and field data.

Concluding observation, based on Petes talk:

heehee!
:D
Sampa: Haha..nice observation!

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