AeroCom-Aerosat Board for keeping discussions, comments, question and answers
DAY 3 , October 14 2020
Please put your full name in front of a comment at least once in a session.
- if you fill in your name at the top right, people can see it by hovering over your text
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 minute taker tool
==========

Plenary Session 6 - Covid impact on aerosol loads, air quality and forcing [90 min] *one slide by moderator with main issues + key questions / 5 min presentations of recent work* Moderator: Kostas Tsigaridis
Rapporteur: Hickman

- Johannes Quaas: Assessing aviation-induced cirrus from satellite during COVID-19 (5 min)
MichaelS: very nice... Emissivity... what exactly is that? Is that measured by MODIS? - it is (sort of) like optical depth, but measuring the cloud effect in the terrestrial
Ralph: I'm wondering how much the loss of cirrus was offset by the reduction in aircraft fuel burned. - do you mean what is the microphysical effect? Don't know, we hope to dig into this once Calipso/Cloudsat (the Sourdeval et al. ACP 2018 ice crystal number retrieval on the basis of DARDAR) data are processed
Andrew: I wonder if the sensitivity would be the same in each quintile? The quintiles are by absolute, not relative change, correct? Does that matter?
Ralph: I was thinking of the greenhouse gas emission reduction (both short and long-term), but the presumably reduced microphysical effect is certainly another factor, working in the opposite direction.
Nicolas: Very nice study Johannes. Do you have plans to work with DLR on comparing to their models? That could be a nice constraint.
Andrew: We are doing contrail studies with the NCAR contrail model as well. The radiative effect seems broadly linear with the reduction in aviation traffic.

- Nicolas Bellouin: Impact of Chinese Covid-19 lockdown on aerosol and radiative fluxes over East Asia Seas (5 min)
Michael D: What are the AOD anomalies defined in relation to?

- Nicolas: They are defined with respect to the long term average 2003-2019.
- Thanks! Is the analysis sensitive to trends? There are big increases from 2003-2011 and decreases from 2013-2019 over that region.
  - Nicolas: There is probably an sensitivity to the period used to calculate trends, given the large changes in AOD observed over China over the past 20 years. Something to look at but the lockdown signal should stay, I think. Other signals (like Beijing Olympics) are weaker.
- Andy Sayer: I would also be curious what the uncertainty in the AOD baseline is, due to the long-term trend. The uncertainties on trends from satellite (and also AERONET) can be quite large because the distribution is skewed and sampling is limited. I have seen various studies into trends in China, often putting in a breakpoint around 2010, but many of the analyses have problems (e.g. the trend line is discontinuous or breakpoint is hand-waved). So my guess is that subtracting the trend from the natural variation might make a precise quantification different. Having said that, the drops Shobha showed in China from VIIRS were pretty large so maybe it is significant enough that it will stick out. :)
  - Nicolas: I agree Andy, we have to remain cautious. But according to MODIS the AOD anomaly over the Northern Yellow Sea is -0.15 -- largest monthly anomaly for March since MODIS was launched. So from the point of view of signal-to-noise ratio, this is a good opportunity.
  - Andy: wow -0.15 is pretty big - yes - looks like this is unlikely to be coincidental!
- Wow, that does seem quite large. It would be good to get to the bottom of the differences in results so far, where my paper with Rob in GRL (https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2020GL088913) and a nice new work out of NASA GISS (https://acp.copernicus.org/preprints/acp-2020-567/) are not able to distinguish an AOD difference from the long-term trends whereas the results shown today are more striking and clear.
  - Nicolas: We focus on the month of March, where anomalies are largest. February was anomalously hot and humid in China, and that affected hygroscopic growth in a direction opposite to the emission perturbation. So perhaps the choice of time period affects the results? We also focus on the Marginal Seas -- anomalies over land may be less statistically significant because of larger variability. (Not sure.)
    - I took a quick look at our results for March 2020 in more detail. We do see a coherent AOD decrease (albeit not stat. sig.) over the East China Sea in March 2020. I'll send a followup email in case you're interested. The points re: meteorology and higher variability over land are well-taken

Zak: is this all-sky AOD compared to clear-sky DRE, or are both clear-sky? I'm guessing the latter since all-sky AOD is easy to model but hard to observe!

- Nicolas: Yes, all is clear-sky. I have never been really sure of what an all-sky AOD would mean.

Gerrit de Leeuw: you reduced aerosol emissions in yourmodel simulations, did you also reduce trace gas emissions? in particular NO2? there are several publications on enhanced AOD due to secondary formaition

- Nicolas: We reduced primary emissions of BC and OC, and precursor emissions of SO2 and NH3. But not other emissions, which may feedback on the aerosol. It is possible that anomalies are smaller than they could have been because of secondary formation, but on the regional scale the signal seems dominated by industrial aerosols.

Toshi: How long did you assume emissions would have decreased in the simulation (a few months or through the year 2020)?

- Nicolas: We perturbed emissions from February to April 2020 -- we ran the simulations in June, so were limited by the nudging data available then!

Michael: The slope TOA flux vs AOD over land and ocean - interesting - can it be used as a constraint for AeroCom models?

- Nicolas: I think it can. I tried in fact, using the "old" AeroCom Hindcast experiments but not many models participated then, and the data is of poor quality. If you have suggestion for a nice pair of control/perturbation experiments, that would be great! Probably no need to run simulations just for that.
- Michael: lets try indeed - your values looked very reasonable !

Philip: Nicolas, Norman – the positive RFari off the cost of China seem reasonable, however, I was surprised to also see relatively large positive RFari off the east coast of the USA. This seems a bit puzzling. Any thoughts?

- Nicolas: That feature was also found by Fabien Paulot in his 2018 paper: https://acp.copernicus.org/articles/18/13265/2018/ According to their model, that is explained by the decrease in SO2 emissions in the US. (Their section 3.2.1.)

- Shobha Kondragunta: China and Taiwan: A Tale of Two COVID-19 Lockdown Measures and Air Quality (5 min)
Michael D: AOD has been decreasing long-term over China since 2011-2013... I think we should expect AOD in China in 2020 to be below climatology without accounting for trends even for no COVID-19 impacts.

- Andy Sayer: similar comment from me about Nicholas' talk about. Though the VIIRS pdf shift here was pretty convincing so it seems likely we can say there was some drop!

Michael, true. same with NOx emissions too. But these reductions are huge (about 0.5). So that is not trend. It is due to shutdown
Mian Chin: AOD over Taiwan is higher than that in China?
Mian, don't think so. For China the domain is bigger so average may look like similar to Taiwan. But China has higher AODs
Michael: Probably NO2 is correlated with other pollution. But maybe still be a good proxy. Although BC and SO4 are more long lived then NO2....
Michael, that is the whole point. NO2 short lived and aerosols long-lived. So we can isolate near source aerosols using NO2 as a filter
Toshi: Good talk. Do you have any information on a change in tropospheric O3 concentration before and after the pandemic in Taiwan?
Mian @Toshi: Shobha uses VIIRS AOD and also showed NO2 form TROPOMI, I think. For tropospheric O3 amount that needs some work to separate stratosphere from troposphere. I think there are products available or in progress.
Mian, thanks for responding. Maybe Toshi is thinking of surface ozone values. Toshi, no we did not look at ozone from ground monitors either in Taiwan or in China
Toshi @ Mian and Kondragunta: Thank you for your response. A change in O3 concentration is interesting as shown in Xiaohong's presentation in which O3 concentration may increases due to NOx emission reduction because of the titration effect (e.g., https://doi.org/10.1016/j.scitotenv.2020.139542).
Lucia: Do you plan looking at AOD correlation with SO2/SO4 as well?
Lucia, we plan to. But can be challenging
Gerrit: AOD over China decreased 2011-2017, but we are not sure what happened thereafter. For NO2 we see the decreased halted in recent years, we are checking that for AOD too
Gerrit, thanks. Will look forward to seeing your results
The 2011 -2017 AOD trends over China were published in Sogacheva et al. ACP, 2018
Larisa Sogcheva: satellite AOD in 2018-2019 has been continuing to decrease (not published)
Larisa, is the trend bigger than the signal we are seeing in China, especially?
Larisa : may be figure 8 in ACP 2018, Part 2 is an answer to your question?

- Xiaohong Liu: Impacts of COVID-19 on Aerosol Direct and Indirect Radiative Forcing (5 min)
Mian@Xiaohong: I may have missed it - where did you get the emission datasets for COVID period? Forster et al 2020
Mian: Do you have data to support the precip and temperature increase/decrease simulated by model?
Andrew: are these changes significant given the variability?

- Ragnhild Skeie: Changes in aerosol composition and radiative forcing due to COVID-19 in OsloCTM3 (5 min)
Thomas: why was there a sulfate burden reduction at the Andes?

- Andy Sayer: I think there is a lot of mining activity there. Perhaps that shut down too? I don't recall hearing about it in the news, though I don't know whether I would have. https://en.wikipedia.org/wiki/Andes#Mining
- Christoph: might be also due to volcanoes in the Andes
- Ragnhild: Natural emissions are kept constant. I havn't digged deep into the anthropogenic emissions. Emission per country and sector are aviailible here if interested: https://github.com/Priestley-Centre/COVID19_emissions

- Augustin Mortier: COVID-19: Impact on AOD and European Air Quality (5 min)
Mian Chin: Dust was only seen in March over S Europe? Not in April?
Augustin Mortier: I guess it was mostly in March, this year. But I will check day by day what's happening in this region!
Mian Chin: Large fires in N America
Yves; Could we use your technique to point to regions that are rather pristine (i.e. regions that do not see any deviations from their climatology?)
Augustin: Yes, I guess we could filter the regions associated to a low/non significant deviation!
Andy Sayer: I wonder if we could use bootstrapping or something to probe this further and estimate some uncertainties on these drops. It is interesting to me that a lot of sites were at the low end of the box whiskers plots but not super below. It is true that most of the maps looked blue, but spatial covariation could mean that things like weather systems affect many sites at once (i.e. these sites are not independent, especially in dense areas). The -10% for the whole world was quite interesting too. I guess the upper limit for a drop we might expect to see would be the overall anthropogenic fraction (I don't know what the rough % is for different parts of the world) - so seeing the regional variation on your last time series plot might also reflect regional differences in anthropogenic vs natural aerosols.

Augustin: The bootstraping is a good idea. We could probably use model data and subset of thee data to investigate the effect of the spatial and time gaps in the observations. I agree that the drops are expected in the different regions depending on the relative contributions of anthropogenic and natural particles. We will definitely dig a little bit more into that!
betsy: nice - we tried looking for a signal in our (very sparse) surface insitu measurements but didn't see anything striking relative to the previous 3 years. our sites are mostly pretty remote so less immediate impact from anthropogenic sources. we also didn't control for weather but anecdotally were hearing from site techs that there were some anomalies.
Augustin: thanks :) It's funny, I would have expected a more clear signal at the ground level.. I don't know if the Aeronet stations are more remote..?
Tom Eck: These are AERONET total aerosol optical depth comparisons (correct?), of 2020 versus 'climatology'. However there are long term trends in AOD at several of the sites shown, including MD_Science_Center and sites in China. Due to decreasing AOD in time over the last 10-15 years the reductions in 2020 are not all due to COVID lockdown emission reductions We looked at AERONET data in the group at Goddard comparing to the previous 5 year means and did not see the dramatic decreases in AOD that were shown in your presentation today. Also the reductions shown in May though August are not likely due to COVID as restrictions in economic activity had been greatly reduced by then.The meteorology has been very unusual in some of the sites you analyzed such as in Maryland , USA there was much greater frequency and amount of precipitation thereby washing out aerosol and resulting in lower AOD. +1
I agree with your comments. Our AOD deviation might be affected by potential trends. In the second part of my results (not the ones available on the we interface), I limited the years for constructing the climatology to the years after 2010 in order to reduce the potential bias induced by a trend.
Regarding the meteorology, I also agree that using only AOD measurements, we cannot distinguish a reduction of anthropogenic emissions from unusual meteorological contributions.
Tom Eck: There is still a significant trend in AOD from 2010 to 2019, so that does not fully reduce the effect of downward AOD trends at several sites.
True.. which means Paul G's idea (next comment) of detrending could be nice!
Paul G: Isn't it important to first detrend before looking at 2020 anomaly?
yes, that is our plan to deseasonalize the data but we don't have AOD data for the whole time period processed with the same algorithm. So we are reprocessing right now to create the 10-year VIIRS consistent record

- Paul G: I can share with you the daily value MODIS Deep Blue (0.1x0.1) Terra & Aqua AOD with derived fAOD and cAOD at the AERONET sites
- Tom Eck: Paul, we at AERONET have been analyzing the fine and coarse AOD from SDA for hundred of sites in the context of COVID lockdowns in addition to MODIS data retrievals of AOD (Jinglong Zhang) for the globe plus data assimilation analyses by Xian Peng at NRL.
    - Paul G: Are these the O'Neil values (that I always used to validate the fAOD nd cAOD from MODIS DB)? Are these results published?
    - Tom Eck: Yes these are SDA fine and coarse AOD from O'Neill's algorithm, they are a product on the AERONET web page. SDA retrievals have been published many times what are you asking regarding publication? Our COVID analysis is still in progress, so not published.
    - Paul G @TOM: When detrending the long-time series of MODIS/AERONET AOD would you or not included 2020?

Paul G.: Augustin very nice work
Thanks a lot :) The detrending strategy is a good idea!

-

- Svetlana Tsyro: Impacts of COVID-19 lockdown on European air quality (5 min
Mian Chin: Why obs showed PM10 increases but model shows decreases? Dust?
As I explained there were different reasons in different cities: anthropogenic and natural (dust, fires) from long-range transport, and local road dust (but also increased biomass burning in Paris, N. Italy etc - not shown here). Some of those sources are not reproduced in the model (road dust, increase in wood burning).
What are "indigeneous sources"?
Svetlana: in those slides they are city-reagions plus national ("Local" on the graphs)
Pierre Nabat : in the decrease in PM10, could you identify which aerosol species explain most of the decrease ? (using notably EMEP model) sulfates, nitrates, BC ?
Svetlana: we have not looked at this as we focused on explaining why we could NOT see Covid effect in most cases. But of course, it's possible - and interesting, thanks!
Roxana: a small comment in you talk about lockdown, but there were no lockdowns in Germany or Sweden, only restrictions. I guess your dates refer to travel restrictions but also these mean something different from country to country, maybe another term would be better for clarification.
Svetlana: You are right. We just started using this 'lockdown' term loosely while working on this study, including in it all types of restrictions/measures, and stuck to it. We should have found a more generic word (or clarify what we meant). Do you have any good suggestions? Regarding the 'lockdown' dates, the BCS has not provided any. So we adopted "non-essential movement banned" from Politico (which by the says for Germany: Partial lockdown).

Discussion 60 (min): What have we learnt so far from the Covid-19 natural experiment?
Best practices to analyze the various data of such an exceptional situation against the long-term background
Andrew: One major issue is sampling uncertainty in emissions.
Mian .... did anyone separate sector contributions to AOD reductions?
Mian, yes Kazu and Kevin. In China (their model runs) transportation sector was dominant. They saw in the model reductions of up to 12 ug/m3 when they included NOx reductions. So it is nitrate and secondary organic aerosols that decreased due to decreased emissions compared to Business as Usual
Mian: Also separate fine with coarse mode AOD from AERONET
Paul G@Mian: I did separate fine and coarse AOD over land using MODIS DB & VIIRS. Detrend then look at the 2020 anomaly. Will compare with AERONET from Augustin.

Michael D: One point re: emissions: My impression is that the industrial sector emissions changes in particular may be overestimated in the Le Quere et al analysis and thus subsequent analyses. They estimated ~40% decreases based on preliminary coal data which is not really consistent with the relatively muted change in thermal power generation in China (based on nationwide government data). Because the China lockdowns occurred earliest, I think they might have an over-sized impact on the initial estimates that has been carried through.
Gunnar: This paper includes GHGs https://www.nature.com/articles/s41558-020-0883-0
Greg: NASA quit flying field missions during the early lockdown, so that is a problem too.
Andrew: But NASA and NOAA did do some flights I think at the end of lockdown and during this summer.
Yes. Missed the early transition but they are flying now.
Andrew: Let's hope they don't catch another lockdown....but we'll see what winter brings!
Mian Chin: Most models presented here uses Forster et al 2020 emissions, which is based on mobile data from Apple and Google. But how reliable are they? Any other emission datasets in preparation?
The Forster et al emssions are mobile data that is baselined against earlier emissions from Le Quere et al 2020, which is sector by sector, but preliminary. So definintely more work could be done on emissions. But it's a thankless task, and not many groups do it. One other option is to try to get uncertainty estimates and vary emissions. It does seem as if some of the aerosols were baselined to mobility data and that;s not really correct.
Gerrit: for NO2 over China: Ding et al, GRL, just published (top-down using TROPOMI
Mian @Gerrit: Thanks - I know there are a couple of efforts on NO2 emission based on satellite data, which is probably the most straightforward dataset to use. But other species are more challenging.
Mian, Kazu and Kevin (JPL) already published in ACP. Used TROPOMI NO2 and SO2 data to modify model emissions for COVID scenario
Zhining Tao: To Xiaohong, your simulation reveals the increase in surface ozone during Feb-Apr. due to COVID-19 (mostly through emissions change). How about stratospheric ozone intrusion? Is 2020 special in this regard?
Flo Malavelle: Rebounding on Yves argument. With the icelandic volcano eruption in 2014-15 we roughly estimated that it released 10 times the amount of annual SO2 emissions released by the combined 28 (back then) european countries. That was the order of magnitude required to get a clear signal on cloud effective radius at a regional scale. One could work out natural variability in specific regions to get a feeling of how much reduction in emissions due to covid would be required to have a detectable effect on ACI.
That being said, it might be hard to untangle an effect on ACI due to covid at the regional scale. At the city scale however, there is potentially something to tease out. In Air Quality modelling we've seen clear reduction in some pollutants (eg NOx) that ties well with traffic but the picture on PM25 was much more complicated. This indicates that the main source of aerosol emissions maybe not the one affected by restrictions (in UK at least). Big cities with large population but limited surrounding industries might be interesting candidates for looking at things like city tracks.

15 minutes break
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
Plenary Session 7 - indirect effects and observational constrains [90min] *one slide by moderator with main issues + key questions / five 10 min presentations* Moderator: Johannes Quaas
Rapporteur: Edward Gryspeerdt
1 slide by moderator with main issues + key questions

- Paquita Zuidema: Oracles overview [10min]
Why is the impact of humidification on SSA small?
Greg: Smoke is generally less hygrosopic than pollution, so maybe that has something to do with it. Also, in situ folks generally assume that absorption does not change wrt RH.
Paquita: yes, we could not evaluate absorption changes with RH. We did have two nephelometers, one providing a scattering measurement at the ambient RH, and the other at a dry (~20%) RH. Their ratio was less than 1.2 for 90% of the time in the free troposphere. This translates into a change in the SSA of less than 0.01 - so not zero, but nevertheless small. That's what the original statement is based on.
Michael: What are the plans for further comparison to other (AeroCom) models?
Paquita: Sarah Doherty is finishing her manuscript examining the vertical structure from basically the same models and comparing them to the obs. Her study extends to more years and includes a comparison of the clouds - everything that is needed for a DARE calculation. This is still not an aerocom-type assessment. We don't have any other further plans at the moment unfortunately.

- Edward Gryspeerdt: Indirect effect - Identifying observational constraints (10min]
Michael: CAM5-3-CLUBB also has a bent relationship? a bit opposed to CAM5-3

- Ed: Yes - I am not sure it is really clear yet what causes this relationship. There are a variety of possibilities, entrainment, precipitation feedbacks etc.
- Michael: It was a major shift in the convection parameterisations, so its an interesting change.
    - Ed: Agreed - it is a useful datapoint in identifying what is going on. The 'unpredictable' models are potentailly the most interesting
        - We could explore if Andrew and colleagues could rerun this - otherwise I think we can also run the NorESM-CAM6 w and wo CLUBB, need to check.
            - Ed: More models are always interesting, so would be nice if possible

Mian Chin: SPRINTARS shows very different Nd LWP relationship compared to other models?

- Ed: Yes - one possibility is that diagnostic Nd models are better able to re-produce the negative relationship at high Nd (if the wet-scavenging effect drives it), as they would be more sensitive to reductions in aerosol from wet-scavenging

Michael: Could you formulate an (updated) experiment proposal, and we add to the wiki?

- Duncan: These are indeed very valuable, but I think we should add the diagnostics to the core set instead, it's not really an 'experiment'...

- Ed: I agree - it wouldn't have to be something run separately. It would be great to add into other simulations/experiments where possible. Given that there is not much output required, it would be great to make it a more common set of output (or something like the control, where it is updated semi-regularly)
- Dan: We tried to include a lot of the key diagnostics from the indirect experiment in our current traj experiment. Worth checking if we cover your needs there.
    - Ed; Do you have matching PD-PI simulations? That is mostly where we end up with things that are missing
    - Dan: No PI I'm afraid (except for UKESM1 which we plan to extend to PI)
        - Ed: UKESM is about the only more up-to-date one I already have (thanks to you and Flo :-))
        - No problem Ed!

Duncan: What was the ECHAM-HAM AAC model out of interest?

- Ed: I am not 100% sure, I think it was Johannes Mülmenstädt's 'best estimate' model
- Cool, thanks

Kai: nice talk. For "ice-free boxes", do you mean sea-ice free, or cloud-ice free?

- Ed: Cloud-ice free, in an attempt to do something similar to a satellite observation. This reduces the statistics quite a lot though, so the 'all-data' results are *probably* more useful

Nicolas: Very interesting talk, Ed, thank you. But given the challenges of modelling clouds (and that's before even mentioning aerosol-cloud interactions!) in GCMs, should you not look at other models? Large domain LES simulations are appearing, and applying your methods on them could be most insightful.

- Ed: Absolutely, although I think the GCM results are still useful. The question is not so much 'are the models correct?', but 'could we ever measure this?' (given meteorological covariations, feedbacks etc. For this, I think GCMs are really useful, as they

Bastiaan van Diedenhoven: Since precip can be assumed to scale with LWP and inversely with Nd, does the LWP-Nd variation in models correlate with precipitation variations in the models?

- Ed: That is an interesting point. From what I have seen, it doesn't seem like there is a huge variation between precipitating and non-precipitating locations within the GCM. There might be some ways to better identify potential wet-scavenging driven cases though.

Lazaros: Can you separate in the GCMs the liquid clouds coming from the large-scale (stratiform scheme) and from shallow convection scheme? Should you?

- Ed: That would be interesting - I am not sure how we could do that with the current output we have though. Perhaps something similar to your regimes would be a way forward (assuming we could show it works in one model at least!). Currently there is output for cf-cod-ctp type regimes from most of the models, but only at a grid-mean level, rather than the full histograms.
- Johannes: most GCMs do not diagnose convective clouds, only stratiform/large-scale ones (in terms of optical properties)
    - Ed: Would it matter for the detrained cloud? Perhaps that is not a real distinction for the model though

- Velle Toll: constraint on cloud water response to aerosols (10min]

Michael D: For the On-Off behavior, I wonder how much of the differences are due to limited detectability of small perturbations versus there actually being no perturbations under some conditions

- Velle: Good point Michael. I agree that other cases are most likely perturbed aswell. However, these strongest perturbations could matter the most in terms of radiative forcing in many regions, depending on how often they occur.

Nick Schutgens: why is the importance of open to close change not well known?

- Velle: Closing of open cells has been observed in ship tracks and is documented in multiple studies. There we can compare the track and less polluted area about 10to20km further away. But it is much more challenging to estimate how large-scale industrial emissions affect this as we do not know the unperturbed case.

Rob Wood: what crierion are you using to detect the tracks? Are you using r_eff, some combination of nearIR and visible channels, or some other method? Does this choice matter? The reason I ask is that 2.1 micron is also sensitive to LWP, so selecting based on this may be also selecting for LWP

- Velle: Good point. We have used NIR. In principle the Segrin et al method. I have tested reff and Nd, results remain similar.
    - Ed: Is there a selection effect, in that you identify them in NIR images? I don't have a good idea on how much that matters, but you could get a good idea with the industrial/fixed location sources
    - Velle: I first use MODIS RGB 3-6-7 available from NASA GIBS to select the cases. And then use 2.1 microm reflectance to classify pixels as polluted and unpolluted. As these perturbations we are looking at are very strong, the automated method does not throw away too many cases below defined perturbation threshold (2sigma).

- Otto Hasekamp: Retrieval of Cloud Condensation Nuclei to Quantify Radiative Forcing due to ACI (10 min)

Sophie Vandenbussche: [maybe a bit our of scope] what do you mean precisely with hydrophobic mineral dust not suited as CCN? I mean is this a common "type" of dust or pretty rare? Is there any way to distinguish it form measurements?

- Our explanation is that particle sphericity is a good indication for the capability of an aerosol to take up water (and hence act as CCN).
- Thank you. Must the particles be spherical or not to be good CCN?
- Yes that is what we fund.

find

Dan: r_lim will vary significantly with environmental conditions (e.g. number/updraft limited regimes), as will the associated susceptibility. How do you account for this? (e.g. under certain conditions particles smaller than the Dusek limit you referred to can act as CCN). See recent paper for sensitivty of smallest activated dry aerosol radius on environmental parameters using a cloud parcel model: https://www.nature.com/articles/s41467-019-12982-0

- By choosing a rather large value for r_lim we expect that we only include CCN particles which gives the 'correct' relation with Nd. The assumption is that it is better to exclude non-CCN (and missing some CCN) that the other way around.
- Dan: I think that assumption will be quite dependant on the shape of the aerosol size distribution, and under certain environments might not hold - i.e. you miss a lot of CCN. You might be able to use the results in the above paper I cited to identify which regions the assumption is valid. I agree, that for most regions it is a better trade off to select a higher r_lim value, but for certain regions it might not be optimum.

betsy: has this technique been applied to the size distributions retrieved from aeronet? could then get values over land. (assuming the aeronet size distributions are comparable to the satellite size distributions youare using.)

- We compared our Nccn to AERONET, see backup slides. Yes, AERONET can do this over land.

Hamish Gordon: is the aerosol forcing over land from models surprisingly strong? Naively I would have expected the effect of the higher land albedo to outweigh aerosol changes.
There is really large variation among the models for the ratio RF_global / RF_ocean. This ratio ranges from ~1.1 to >2

- Thanks...I think I maybe misinterpreted the ratio - does 1.1 mean 10% of the RF is from the land, or does 1.1 mean the RF over land is 10% stronger than the RF over ocean?
- 1.1 means 10% comes from land
- makes sense now, thanks!

- Marta Luffarelli: Aerosol retrieval in presence of clouds (10 min)

Sophie Vandenbussche: I'm interested in the full reference for Larisa's work on Cloud Post Processing Scheme :)

- https://amt.copernicus.org/articles/10/491/2017/amt-10-491-2017.html -- we use it in product genearation
- -> thanks!! I'll read that. Do you think it would be applicable also in infrared (IASI)?
- It is based on 10 km retrieved AOD grid with spatial constraints, So yes, but the thresholds will depend on your resolution
- Thanks again. I am printing the paper and will read ;) You'll know if I manage to make it work for me!

Andy Sayer: how does the CISAR joint (aerosol plus cloud) approach work - does the algorithm try both aerosol and cloud models and see what fits best (e.g. Optimal Estimation cost function)? or is it some sort of adaptive mask? What fraction of pixels are ambiguous?+1

- If I rememeber correctly, way over 10% were in the twilight zone in our cloud/aerosol cci experiment (not retreived either aerosols or clouds) (Andy: thanks Pekka!)
- Marta Luffarelli: CISAR defines a solution space through the selection of 6 classes, 3 for aerosols and 3 for clouds. It is probably a bit long to answer by chat but I'd gladly discuss it somewhere else. All classes are used in the same run and the through the Optimal Estimation method we find the "best" OT associated to each class.
- Adam: Is that all six at once or two separate runs, each with three?
- All at once
  - Huh. I'd be curious to dig around the information content on that. (Would never have the time, but a man can dream.)
  - I'll be happy to notify you as soon as we publish a paper on this :)
- Maybe you have a reference to provide? ;)
- https://doi.org/10.5194/amt-11-6589-201 here are the basic concepts of the CISAR algorithms are described, but without including clouds. A new paper should hopefully be published in the next few months :)
- I'm very interested to read when that happens :)

Bastiaan van DIedenhoven: What are the inputs to CISAR (e.g. wavelength bands needed)?+1

- CISAR is applicable to any sensors, it is not based on any particular bands, However it does exploit the spectral response to build the prior information and clearly some bands are more "interesting" than others for building a good prior information on clouds. At the moment CISAR needs the acquisition time and geometry, the spectral response function, and model parameters, i.e. TCWV, TCO3, surface pressure.

Sophie: would your approach (joint retrieval) be applicable to other instruments? (In my case IASI)

- CISAR is theoretically applicable to any instrument operating in the visible/ nir spectrum. so far it has been applied to SEVIRI, MVIRI, SLSTR, PROBA-V.
- OK, so not for IASI then unfortunately. Thanks

Anin: Is the CISAR applicable only to MODIS observations?. Is it possible to port it to multi-angle polarimeters?.

- Andy Sayer: Anin - these data shown were I think SLSTR, which is a dual-view instrument. I know CISAR has been applied to SEVIRI (geostationary) too.
  - Anin: @Andy Thanks for the clarification. This technique seems very interesting considering the fact normal MAP aerosol retrieval algorithms take a lot of time to run retrievals for a whole scene.
  - Anin: @Marta What is the processing time for a single pixel retrievals using the CISAR?.
  - It is about 2s per pixel per accumulation period (16 days).

Duncan: Perhaps it's a silly idea, but is there any way of comparing optical depth (at a narrow wavelength) regardless of whether it was a cloud or aerosol (or something in between...!) between the satellite and models?

- Adam Povey: Yes, but not many algorithms produce aerosol and cloud in a manner that's radiatively consistent at TOA, so it can be difficult to tell the modellers what they should be outputting.+1
- Duncan: OK, thanks.

- @Duncan, indeed one of the main goal of CISAR is consistency, as surface reflectance, cloud and aerosol single scattering properties are retrieved with the same radiative assumptions.

Lazaros Oreopoulos: How can one distinguish between 3D artifacts in aerosol retrievals near clouds from genuine differences in the properties of aerosols near and far away from clouds?

- @Lazaros, very interesting question. I have to admit I need to investigate more on this.
- Anin: A collocated observation of HSRL2 plus a multiangle polarimeters will be useful to study these 3D effects. ACEPOL 2017 campaign had 4 polarimeters + 2 Lidars onboard on ER-2 aircraft and I am sure that you can find some cases where this 3D effect is evident.

Lazaros: For example, aerosols near clouds may be subject to more swelling because of increased humidity. This can be investigated if some info on aerosol size (e.g., AE) is available (devoid of 3D effects!). See here: https://modis.gsfc.nasa.gov/sci_team/meetings/201810/presentations/plenary/varnai.pdf

Discussion (40 min): Ways forward to better constrain aerosol-cloud effects with observations
Andrew: Yes, there are several things underway. Like with volcanoes as Michael mentioned. We intend to develop a database that could be used for GCMs.

Plenary-Introduction: 1 slide introductions from breakouts 4-6 of non plenary talks (ca 15 minutes altogether)
Philip: @Ben: nice stuff on self raising aerosol. Effectively this is the same as the well documented thermally direct circulation induced by absorbing aerosols? This is likely to only work in the tropics... c.f. https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2019GL083479
Michael @Ben: What is the mechanismn? are air parcels lifted on average?
Philip: @Michael: I assume it is a large scale thermally direct circulation induced by the absorption...

AND REMEMBER, tomorrow we start with a photo
Join a short group photo session on zoom, switch on the camera !!
Thursday before plenary EU:1:30pm/NY:7:30am/CA:4:30am/JP:8:30pm/CN:7:30pm
https://eu01web.zoom.us/j/68970213682?pwd=czRtVnRRVHJQZSsvSTlSL294WEhvUT09
Display an item indicating where you are?
For example, a clock showing the current time, a small national flag, or logo of our organizations...

15 minutes break
EU:5:30–7pm/NY:11:30am–1pm/CA:8:30–10am/JP:00:30am–02am/CN:11:30pm–1am
Start of breakout sessions delayed +30minutes

Breakout 4 Moderator Kostas Tsigaridis Rapporteur Jonathan Hickman,
Suborbital observations & biomass burning & COVID
ATTENTION use the new webex link distributed this morning for breakout session 4
https://nasaenterprise.webex.com/nasaenterprise/j.php?MTID=me721ebac70f533c586b1d866775d3c93

Link to Kristina Pistone paper regarding in situ and remote sensing retrievals of smoke properties during ORACLES is https://acp.copernicus.org/articles/19/9181/2019/
Note (Sampa Das): Just to add, based on Paquita's talk this morning and based on Yohei's ORACLES 2016 paper (https://acp.copernicus.org/articles/20/11491/2020/acp-20-11491-2020.pdf), " The FT SSA ranges widely across the models, with mean model values ranging between 0.80 and 0.92; in situ values are approximately 0.86."

Tom Eck
betsy: can that wierd BB size distribution be tracked across the US with other aeronet sites?
Tom Eck: Yes, we have seen this aromalous AOD spectra in Colorado and UP Michigan AERONET sites.

- does it shift at all - like i guess i'm wondering if you can say something about aging by looking at changes across the country
- Yes it does shift slightly with the most extreme case being at GSFC. Much aging occurred over the Pacific so there are even some extreme SD to the east of LA after aging for 3 days or so in an elevated layer (~5km) over the Pacific Ocean
- LA= los angeles (or louisiana)?
- Sorry, Los Angeles, CA :)

Tom, UMD did a flight to measure absorption of this aged smoke plume on September 16th if you want to follow up with Russ Dickerson and Xinrong Ren
Tom Eck: Hi Shobha, Brent talked to Russ Dickerson and found out that they could not fly up to the 5 km altitude where the smoke was located on Sep 15.
Tom, yes. They flew on the 16th and sampled smoke between 2 km and 4 km.
Tom Eck: On the Sep 16 the smoke type was somewhat different, not as large particle size as on the 15th, and we are beginning to investigate that.
Rich F: The NASA ACTIVATE project has both airborne remote sensing and in situ measurements of smoke observed this past September offf the east coast.
Tom Eck: Thanks Rich. I heard from Brent that found out that the aircraft sampling was not high enough to get to the 5 km layer from this CA smoke event.
Rich F: that's true on one day. However, there was another day when the smoke was lower (just above teh marine BL), when teh Falcon flew within the smoke with the in situ sensors.
I can send you some preliminary images showing this.
Tom Eck: OK great, thanks again.

We are also planning to do combined (lidar+polarimeter) retrievals of the smoke from teh HSRL2+RSP measurements.
Did you have HSRL measurements on Sep 15?
Yes. I just sent you some preliminary images.
Anin: Hi Tom, I have a question regarding the HYSPLIT back trajectory you have shown in your slides. Looking at the worldview and MODIS data, we can trace that , a quiet a lot smoke coming from the fires near to Denver, CO. However, HYSPLIT is not showing that in the back trajectory towards the GSFC station.
Tom Eck: Hi Anin, we have looked at GOES time loops in addition to the HYSPLIT back trajectories, and for the Sep 15 date at GSFC it seems that the smoke is coming from the west coast fires. The West coast plume also passed over Colorado on Sep 14 and we see the extreme size distributions at AERONET sites near to Boulder, CO as well.

- Anin: Yes, looking at the ABI images will give a clear picture on the trajectory of the plume. I wish we had some HARP observations during the same time. But the orbit was bad for that day, that it was capturing data close to terminator for the North American region.
- in case it's of interest, here's the last 30 days of continous surface spectral scattering and absorption measurements just north of boulder CO. https://www.esrl.noaa.gov/gmd/aero/net/getplot.php?key=overview&sta=bos&type=avg you can see the broad multi-day peak which was more long range/aged smoke in mid september and the sharp peaks more recently which were from more local (CO-based) fires.

Rich Ferrare
Lucia Deaconu: It's great to see a validation of the SODA product with HSRL observations. I was wondering how were you retrieving the lidar ratio? It is for the operational product? I think I missed the explanation.

- We used the constrained retrieval using teh opaque water cloud to derive the AOD then use this to derive teh lidar ratio. It is actually the output of teh SODA retrievals. Damien Josset has been working with us on this.

Nice. Is there a paper on this yet?

- No, unfortunately not yet. Hopefully soon.

Qian Tan, is CALIPSO Cloud product you showed available to the public?

Ben Johnson
Claire: Really interesting results Ben. Did you use a realistic SSA for BC? I also wonder if your results could extend to similar problems/effects we see in the Saharan dust plume? Also, can you rule out circulation changes due to BC absorption radiative effects, which might impact the plume transport and height?
Hi Claire, thanks for your comments.

1) The SSA for the absorbing aerosol layers varies interactively as I'm using the GLOMAP-mode scheme. In HadGEM3-GA7.1 the BC absorption is quite strong, maybe 30% too strong compared to AAOD observations from AERONET. This happended after we upgraded to CMIP6 which puts out a lot more BC emissions. Nevertheless, I think the conclusions would be the same if we tone down our BC RI slightly, just slightly smaller magnitude.

2) Yes, I'm sure that the absorption in dust could have the same physical effect, although CLASSIC dust isn't that absorbing so I think it'd only make a difference in thick plumes with plenty of coarse (absorbing) particles.

3) The lofting in the model occurs through the impact of the absorption on the large-scale vertical velocity, rather than on local effects. Sorry I was rushing and probably didn't make that clear.

- Thanks Ben!

Yves: Ben, is it important in your study to have the correct SSA for BC (same question than Claire) and also how much care do you have to put in having the correct particle size distribution? Have you done sensitivity runs on these two?
Hi Yves. See comment above on the SSA. The size distributions in HadGEM3 (GLOMAP-mode) have been validated with aircraft observations and come out pretty good for biomass burning haze (CLARIFY, AMMA, etc).
Lucia: Hi Ben. Nice study! I was wondering if you plan looking at humidity advected as well..that might not be an actual thing but in a study I made before we were unable to separate the humidity from the biomass burning aerosols so one explanation we had was that they were advected and transported together...You think that could be the case?

- Yes, that it an interesting point. I have thought about the BC absorption helping to lift up CO and other trace gases, but you are right it will likely aid the vertical transport of water vapor too. Typically the large-scale ascent that is enhanced by BC heating drives increases in deep convection in those regions that are convectively unstable already. Monsoon precip is increased for example due to the elevated heat pump.
- Interesting! Thanks! It would be nice to be able to separate meteorology from aerosols - or humidity from BC
- Yep, its very hard to disentangle the BC direct impact on ascent from the meteorology because as soon as deep convection is present, the BC heating can lead to strong feedbacks (more BC heating, more precip, more latent heat release, more circulation change...) I've noticed that such latent heat releases can be 5 times more than the original BC radiative heating input.
- That's why doing simplified model simulations could help understand the processes there.
- Hmm, yes that's something to think about. Even a back of the envelope calculation is enough to show what vertical velocity change you expect from a certain heating rate. That calculation only works in the tropics mind, where you can assume a balance between radiative cooling / heating and adiabatic heating / cooling from subsidence / ascent.

Sampa Das: Hi Ben, Nice talk! So, just to confirm you did a free-running simulation or a nudged meteorology run?
Yes, it was free-running.

Ron, what sort of simple model simulations did you have in mind?

Anin Puthukkudy

Yves: Hi Anin, in your retrieval, you have to assume a PSD. For GRASP, do you have the same PSD for all the geographical locations or do you vary the PSD with the distance to the source regions?
Anin: Hi Yves, In GRASP there are multiple options for retrieving the PSD. For the smoke case, we are assuming that the PSD is represented using 5 log-normal modes. However, the concentration of each mode is a free parameter in the retrieval. Basically it means that we are retrieving the PSD with some a priori constraints. For the case of AERONET comparison we have tried a more simplified approach where, PSD, complex refractive index, and shape distribution are fixed for the five aerosol components and we are retrieving only the weight for the aerosol components. This simplified approach reduce the number of retrieved parameters from 15 to 6 for the case of AirHARP. All the retrievals I have shown are single pixel retrievals so there is no constrain on the aerosol properties over the spatial dimension. For the case of size of the particles, both the HSRL2 AE and AirHARP AE shows that the AE is increasing when we move away from the smoke source.
Reed: In the five mode configuration are the 15 parameters 1 concentration, 1 RRI and 1 IRI times 5 modes? If so, is the RI spectrally flat? Do you think that impacts retrieval performance?

- Anin: 15 parameters includes: 5 for the PSD, 4 for the RRI, 4 for IRI (since we are using 4 bands, spectrally smooth), 1 for SF and 1 for the aerosol layer height. So RI is not spectrally flat. I didn't mention the surface parameters since it stays same for both cases. I'm not retrieving RI for each size modes.
- Reed: Okay, I see now. RI is constant across size bins but not wavelength.
- Anin: Yes, exactly. I will try to do a multimode retrieval for RI in the future for high AOD cases.
-

Breakout 5 Moderator Johannes Quaas Rapporteur Ed Gryspeerdt
Aerosol and clouds, indirect effect

Natalia Chubarova

Suvarna Fadnavis

Haruka Hotta

Mattia Righi

Breakout 6 (Moderator: Andrew Sayer; Rapporteur: Thanos Tsikerdekis) (https://nasaenterprise.webex.com/nasaenterprise/j.php?MTID=m66b5b6a82b5ebd0ed1265fc1811155e7)
- Douglas Alyson: Understanding changes in warm cloudiness since pre-industrial times using machine learning of satellite observations
Adam Povey: <drags out hobby horse> To what extent is the poor performance of linear trends due to AOD being log-normally distributed? Or the trends having a change in behaviour in the middle of the timeseries?
Alyson Douglas: I believe it is hard to say overall. In order to quantify how a linear approximation artifically introduces error because ACI are not linear means you would have to also exactly quantify how the environment modulates every interaction. And then you would also have to take into account how high aerosol concentrations regions are more likely to experience changes in the local environment due to direct aerosol effects as well. So there are many reasons why linear regressions may have poor performances, one of which is the log-normal distribution.
Duncan: Nice talk! Why does the error increase when using the higher resolution CF?
Also: How did you do your train/val/test split?
Alyson Douglas: At a 12 km cloud fraction, you really only have 12 possible states of cloud fraction (1-12 pixels being cloudy) so exactly predicting between 1 or 2 pixels being cloudy leads to a higher error at a 12 km scale than a 96 km scale. We did 80-20 test, within the 80 is a validation split of 20%.
But was that randomly, across different grid-cells, or did you split it across time (e.g. different years?)

- Tsikerdekis Athanasios: Aerosol emission estimation using future satellite observation capabilities under the framework of Observing System Simulation Experiments (OSSEs)
Tero Mielonen: What are the spatial and temporal resolutions of the simulations?

- Thanos: Hi Tero, as I mentioned is 1.8degrees (T63) and every 3hours.
- Tero: Thanks! By the way, I also did some simulations with different meteorology (ERA-Interim and CERA-20C) and noticed big differences in AOD levels. I think the main reason was dust and sea salt emissions which depend on wind speed so small changes near the wind speed emissions affected the emissions quite a lot.
- Thanos: That is very interesting! Sea salt and dust emissions under difference meteorology of course will be different. The differences in my case at least were far away from the source regions and can be attributed to the different transport path for ERA-5 and ERA-interim.
- Tero: Yes, the transport routes can also change. I was surprised on the size of the effect. I had assumed that the different ERA data sets wouldn't have big differences. Apparently, I was wrong :) If you are interested, I can share some results .
- Thanos: Definitely interested! Indeed I thought that ERA-5 and ERA-interim would be similar. But for an averaged period July-September the transport path over the gulf of Guinea is quite different, hence the different AOD.
- Tero: Great! I'll put some figures together and I'll send them to you in a couple of days. I'm using ECHAM-SALSA so our models are quite similar.
- Thanos: Thank you!

- Wandji William: How close to sunshine-duration based AOD retrievals is the ECHAM aerosol load over Europe?
Thomas: SD-AOD is a relevant dataset for early period satellite and model evaluation, but needs a comparison to AERONET
Gunnar: A link to the validation of the SD-AOD would be useful.

- William: Please, find here the link for the paper https://amt.copernicus.org/articles/13/3061/2020/amt-13-3061-2020.html

Thomas Popp also made the suggestion to try using SD-AOD as a way to evaluate satellite obs before AERONET was widespread (e.g. AVHRR, ATSR2, early GEO)

- Xie Bing: The contributions of short-lived climate pollutants to global climate change since the pre-industrial era
Interesting to see more work on aerosols and the Asian monsoon

- Yang Yang: Impacts of domestic emissions and regional transport on aerosol concentration, radiative forcing and climate during 1980–2018
Tero: Why there's no change in European BC in the Arctic (slide 6).
Andy wondered if we could use model "tagging" capabilities to trace aerosol types from different source regions (cf. African smoke separately from South American smoke) in order to draw better "boxes" for regional assumptions in satellite retrieval optical models.
Nick noted it is difficult to evaluate tagging, though radioactive tracers are one option.

Plenary presentations of this break-out session are planned for tomorrow (Thursday Session 8). The link for these presentations: ftp://ftp-projects.zmaw.de/aerocom/meetings/ny2020/pdf_summaries/AC_session08_talk.pdf
- Garrigues, Sebastien - AOD monitoring within the CAMS data assimilation
- Kramer Ryan: Observed aerosol forcing trends over the A-Train satellite era
- Myhre Gunnar: Update on AeroCom Historical experiment
- Schutgens Nick: An AEROCOM/AEROSAT study: evaluation of global models with satellite AAOD and SSA
- Wenying Su: Regional trend comparison between satellite observations and model simulations: 200207-201712
- Yang Yang: Modeled 1980-2018 trends over E.Asia

General discussion focused on AOD, AAOD, forcing, and uncertainty confronting models with obs (some topics for tomorrow's plenary)

Nick pointed out that a lot of uncertainty sources don't occur in isolation but are structured and link with each other.
Nick noted we don't necessarily have sufficient satellite AAOD retrievals to get as meaningful a diversity as we do for total AOD (cf. Schutgens et al., in press, https://acp.copernicus.org/preprints/acp-2019-1193/)
However that paper established that diversity is a fair proxy for uncertainty in satellite AOD. So next step would be to pick a region to focus on and figure out why diversity is high there. (The paper also notes a few AERONET sites where all retrievals seem to do poorly, which could be places to target.) Potential topic for a future AeroSat experiment?
Wenying noted with CERES we can combine AOD and flux to estimate forcing efficiency, which is not AAOD, but is something related for model evaluation
Larisa Sogacheva : and inter-comparison of satellite monthly AOD over different regions here: Sogacheva et al., 2020, ACP https://acp.copernicus.org/articles/20/2031/2020/

Other suggested topics for discussion in breakout 6 (and plenary 8):

- Best practices to integrate information from satellites and modeling
- What are conditions of high / low consistency within satellite data / modeling and between both?
- What should be development priorities to improve on the above over the next year or so?
- Best way to compare different products and resolve differences for data assimilation

--