INSTRUCTIONS:

- Do not delete content in the Etherpad!
- Start comments with your first and last name, example:
  - Jennie Thomas: Question for Antoine, what is the main impact of your figure on slide 6?
- We have prefilled the talks that are occurring in each session, please note questions for the interactive discussion during the talk.
- You can respond to questions during the interactive discussion and also pose extra questions during the discussion time.
- This "pad" is a collaborative document that can be edited by anyone who has the URL. ***DO NOT DELETE CONTENT.***

********** 3rd CATCH Open Science Workshop 9-13 May 2022 **********

Day 3 - 11 May 2022: Polar halogen chemistry and interlinked processes
(Jennie Thomas & Thorsten Bartels-Rauch): What is the role of snow, including snow on sea ice, compared to sea salt aerosols in activating and sustaining halogen chemistry? What are the main outstanding questions regarding how halogens influence atmospheric aerosols and oxidation capacity?

Please use this etherpad "chat" to leave your general comments and specific questions to speakers. This way we'll keep a record of your contributions and discussion across all three session of today.

Session 1 - 0800-1000 UTC+2

Poster session

Introduction

Insights into multiphase iodine chemistry in cold environments
ROOSE, Antoine Roger

nice talk Antoine. Just a comment - we've also been remeasuring temp dependence of O3 + I- on seawater and also see only a weak T dependence (Lucy Carpenter) A. Roose: Thank you Lucy, Nice to see we got the same behavior. We will confirm through molecular modelling in both surface and bulk phase (not introduce today as it is only initiated). We are also performing depth profile analysis of iodine oxide using liquid jet XPS to increase the understanding of the chemistry. LUcy: sounds great. Yes, strong gradients of I- in solution definitely make the problem more complex.

Halogens in Ice Core Records
SPOLAOR, Andrea

Jennie @Andrea - I think the issue of no post-depositional processing of bromine on the ice sheets (Greenland, Antarctic) is not so clear. Let's chat about that during the discussion.

Andrea Spolaor: sure, for field experiment is not easy to resolve but I never determine a clear emission of Br, means a decrease in the Br in the surface snow

Joel Savarino - Giving the volability of halogens compounds I doubt there will be no snow emissions.
Markus @Andrea @Joel - our observations above sea ice show Br enrichment of snow and often depletion of aerosol in winter spring indicating active recycling Br chemistry, so both emission and deposition in the source region.

Andrea @Joel@Markus: recycling of Br could be present (iodine is recycle) but I never determine a significative depletion in the surface snow. Again this topics should be discuss in presence, difficult from here.

Shuting Zhai @Andrea @Jennie - I have a poster on the impact of snow chemistry of bromine on ice core preservation and would love to discuss with you both there! I'll be there during poster session before session 3 of the halogens.

Andrea Spolaor: I've seen the poster and could be nice to discuss toghether. Unfortunatly this evening (Italian time, should be session 3) I cannot attend but could be super nice organise a chat also in coming days.

Shuting Zhai @Andrea - I will also be there tomorrow before session 1 if that works for you!

Andrea Spolaor@ Shuting Zhai: this could work, tomorrow before session 1 will be fine for me

Markus @Andrea - Ice core interpretation of Br/Na is complicated by recycling near the source (snow on sea ice and SSA from blowing snow) and also during transport to the ice coring location. Some more detailled modelling will be needed.

Andrea Spolaor @Markus: it is complicate but from ice core we see the main process and we cannot disentagle the specific process. The emission is complex but to me is the transport the most difficult aspect. Next time the Catch should be in presence. Modelling is essential now

Lucy@Andrea: I'm not convinced that there is strong evidence for a direct biological source of I in polar regions (will address in my talk)

Andrea Spolaor: Iodine is rather complex to understand and our is one possible explanation to interpret the iodine signal

A. Roose : Could recycling of iodine oxyde in the snow by photolysis processes be involved in the iodine emission just after the night time?

Andrea Spolaor: could be but it is an argument difficult to discuss only typing

Anoop@Andrea: Considering the fast recycling that happens in the snowpack, the signature of which you see in the annual signal, do you see any correlations with meteorological parameters - or even UV radiation etc.? I mean on an inter-annual scale

Quick question from Thorsten Bartels-Rausch (PSI): The bromine enrichment follows temperature. Is there correlation with snow falling? Is it thus rahter (or partialy) humidity/snowfall than temperature?

Modeling snowpack emissions and recycling of Arctic bromine and chlorine
AHMED, Shaddy

Bill Simpson: Thanks for a nice talk. Is the increase in HOx occurring via halogens attacking VOC (leading to new radicals) or via the O3 + X-snow reaction which makes radical precurors?
Shaddy Ahmed: Thanks Bill. What we find is that the increase in HOx is occuring mainly from Cl + CH4 forming RO2 in the model (and then conversion to HO2 and OH). (It's important to note that this is only occuring very close to the surface and there is no significant change in HOx above 50 m).
Bill Simpson: Thanks so much for the reply; very clear.

Joel Savarino - To be sure I got the right message, are your snow emissions prescribed, prognosticated or constrained by atmospheric concentrations?
Shaddy Ahmed: Thank you Joel. We parameterize our snow emissions of Cl2 and Br2 based 2 proposed mechanisms: (1) as a function of solar radiation and surface ozone concentration and (2) based on deposition of HOX and XONO2 (X=Cl, Br).
Joel Savarino: ok but you tune or calibrate your parametrization against what observations?
Shaddy: @Joel We tuned the parameterizations based on the surface observations from the OASIS campaign (from the measurements showed in the presentation).

Shuting - Great talk! I wonder if you have calculated the emission fluxes of Br2 and Cl2 from your 1D model?
Shaddy Ahmed: Thank you Shuting - Yes we calculate surface emission fluxes for both Br2 and Cl2 in the model (see figure 8 in **https://doi.org/10.1029/2021JD036140**). We calculate surface fluxes that are close to those observed from the coastal snowpack for Br2 and Cl2. Our Cl2 surface flux is a little higher than previously observed but this may be due to the high Cl2 period we chose to study and would probably expect a smaller flux on other days during spring.
Shuting - Thank you! You just answered my follow-up question on why Cl2 fluxes are higher than Br2, which contradicts with most observation-based calculations.

Interfacial supercooling and the precipitation of hydrohalite in frozen_

NaCl solutions as seen by X-ray absorption spectroscopy
BARTELS-RAUSCH, Thorsten

A. Roose: How the salting effect impacts the Henry constant? Does it fit the behavior of your uptake coefficient? Thanks for the question, Antoine. The solubility of ozone is greatly reduced by an salting-out effect caused by the high br concentration (plus additional impact of the organics). Jacinta was able to estimate these salting effects based on literature data and with that parameterisation she could reproduce the uptake coefficient and its T trend; yes.

Impacts of Surface and Blowing Snow Sources of Arctic Bromine Activation
MARELLE, Louis

Comments for posters:
Peter Effertz

Bill Simpson: Interesting poster and thanks for sharing this. I'm very interested in what seems to be a recent decline in the ozone depletions in March. It almost looks like since about 2012 the ODEs have slowed down. Any thoughts on that? Any thoughts on a more complex analysis that simply a linear trendline?

Peter Effertz: Thank you! Yes, I have been interested in the drop off or slow down as well. I'm not sure yet if its being driven my chemical or dynamical changes. I do see a significant change in the winds in March since 2014. It is just a linear trendline right now, but I would like to do a more involved analysis with back-trajectories.

Nicolas Faure

Shuting Zhai

Discussion

Jennie @3D polar bromine modelers: Why are models of polar bromine so different? Is there will from the community for a model intercomparsion that includes comparison of multiple parameters - met, ozone, depo, bromine, aerosols, sea salt aerosols, etc? If you are in for doing this - please say it!

Louis: I am in of course.
I think the first step would be a relatively naive comparison of the different models - we need
(1) to agree on a time period to simulate when many measurements are available
(2) agree on some basic variables to save and compare and on their time resolutions and vertical levels (I would say - surface Ozone, surface BrO; Br2 and BrCl emission fluxes from the different processes; ozone and halogen deposition fluxes; BL height; 2-meter temperature; aerosol surface area and sea salt aerosol concentrations)
(3) agree on simulations to perform - I would say one base simulation and one with halogen emissions turned off
Markus F: A model intercomparison is a great way forward. I wonder how much of the difference is due to model resolution (e.g. WRF-CHem vs Geoschem or UKCA)? sea ice coverage and boundary layer will be represented differently at increasing resolution. Also time step plays a role, monthly vs daily means of BCs.
Louis @Markus - Yes I agree, and if possible we should be using similar resolutions, and for nudged/forced models the same input meteorology (for example WRF-Chem is forced with NCEP-FNL sea ice, SSTs, and boundary conditions in these runs but we could also use ERA5)
@Louis, like a CMIP experiment where all models are forced by the same inputs.
@Louis @Markus - using the same met is too ambitious in my opinion. But, at least the met should be compared.
@modelers (from Bill Simpson) we worked with Chris Holmes on GEOS-Chem modeling of these reactive halogen events. I think it would be good for you all to reach out to Chris to see if he would like to participate. His former undergraduate student (Kaitlyn Confer) is now working with Lyatt at UW and might be interested, as might be Lyatt and/or Shuting Zhai.

Andrea B.: I just wanted to make the point that chemical ionization mass spectrometry is a powerful tool for iodine species detection. Bromide CIMS especially: https://amt.copernicus.org/articles/14/4187/2021/

Session 2 - 1200-1400 UTC+2

Poster session

Introduction

Iodine chemistry over the Southern Ocean
MAHAJAN, Anoop

New particle formation over the high Arctic pack ice by enhanced iodine emissions
BACCARINI, Andrea

K.Sellegri: seasonnal variation with 2 peaks look interestingly similar to the double peaks diurnal variation of iodine

Andrea B: Thank you Karine, yes there are similarities but I think the underlying processes are very likely different and very different locations…

Iodine chemistry in the polar regions
CARPENTER, Lucy

Kerri Pratt: Beautiful talk! Halfacre et al 2019 (ACP, https://acp.copernicus.org/articles/19/4917/2019/acp-19-4917-2019.html) is another lab ice study showing I2 production.

Lucy: THanks Kerri Ill check that out!

Jennie: Thanks for the nice talk, its interesting that the peak SSA is in winter and that there is no iodine at the top of the snow in that season.

Lucy: Yes - that is not easily explainable, from the source side at least.

Kerri Pratt: Lucy - your snow depth profiles are really interesting. Our tundra snow I- also showed more at lower depths - since there's no sea ice underneath for those profiles, it might be an interesting comparison in

terms of absolute conc. to your exciting MOSAiC data?
Lucy: Definitely. Happy to send you our data.
Kerri: I don't have anyone working on iodine right now, but Angela Raso's dissertation has more snow [I-] from other locations in the Arctic (including High Arctic) and Antarctic. If you're digging up and pulling together old data, it would be exciting to collaborate - if you might be interested!
Lucy: Sounds good. If you can send to me the data then I'll let you know how we get on and set up a meeting at some point.
Kerri: Great!

Markus F - Very exciting to see an updated summary of MOSAiC iodine vertical snow profiles. It would be good to include any atmospheric observations as well as precip events to disentangle iodine sinks/sources. We have measurements of major ions (plus Br-) in both snow and from aerosol filters we should compare to. Re:snow photolysis I am not aware that the snow e-folding depth has been measured during MOSAiC, best to ask Amy MacFarlane.

Jennie: There is also this paper, which is of course for bromine, but I think gives some insight to polar night from Bill Simpson: https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2018GL079444
Lucy: thanks Jennie. Bill always worth reading!
Somehow I never get around to modeling this one with our 3D model - someone should do it!

Anoop: Would you still need a biological source to explain 10s of pptv of IO? From the talk I understand that the inorganic source is enough only for a couple of pptv at most.

Lucy: we would need Antartic snow samples to answer that question. Of course, in the Arctic I think we only need to explain low ppt levels of IO.

Anoop: Makes sense - but it sounds like an extra source rather than proving/disproving the biological source. The Friess numbers were similar to the Arctic number for snowpack iodine - although there are doubts about the methodology. We'd be happy to send you some samples if you want :)

Lucy: Of snow? yes please!

Anoop: Great - will connect with you regarding this. Also, as you know we have IO observations during MOSAiC!

Lucy: we should connect on that as well.
Roseline: @Lucy @Anoop, We also have HIO3 measurements from MOSAIC and Gruvebadet, Svalbard as well. We did measurements using NO3- CIMS. Please let me know if that interests you as well.

Anoop: Definitely. Will email you after the workshop.

Xin Y: is surface iodine enriched or depleted (comapring to that in sea water)?

Kei. H: Thanks for nice talk. Iodine/Na ratios in brine and frost flowers showed iondine enrichment on young and new seasonal sea-ice wtih wet surface. you can check the data in our work (https://doi.org/10.5194/acp-17-8577-2017).

Jennie: one other reference that I find useful, for bromine also from Bill's group:
https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017JD026906
Snow melt, meaning T>0 == stops bromine activation and fresh snowfall re-activates release

Snow pH is also quite important: as shown by lab and studies and Kerri's snow chamber work.
Kerri: I agree snow pH is critical! It must be acidic for Br2 production.

Kerri: Lucy - to your question about brine migration vs SSA deposition, Domine et al 2004, ACP is my favorite. Here's our paper (Peterson et al 2019, Elementa) that incorporates Bill's snow paper's data (Krnavek et al 2012, Atmos Environ): https://online.ucpress.edu/elementa/article/doi/10.1525/elementa.352/112483/Snowpack-measurements-suggest-role-for-multi-year

Kei. H.: Anybody monitor pH in snow on sea-ice during MOsAiC?

Andrea @Lucy: nice talk Lucy, this is the paper that could be interesting: data are available

https://www.sciencedirect.com/science/article/pii/S004896972035169X#f0020
Lucy@ Andrea - thanks, got it.

J. Kleffmann: Hi Lucy, if sensitivity is a problem with the detection of nitrite in snow, this can be done easily by the Griess-Saltzman recation and a liquid core waveguide. We reach 10^-10 mol/l by our LOPAP technique, which could be simplified for snow measurements...
Lucy: thanks Jorg, this would be interesting. Do you have any measurements already in snow/ice?
J. Kleffmann: me only made a few measurements in Barrow, but I have to look to the data. Here our LOPAP instrument was used like during calibration...
You can find the data in dx.doi.org/10.1021/es404002c | Environ. Sci. Technol. 2014, 48, 165-172

Atmospheric sea-salt and halogen cycles in the Antarctic
HARA, Keiichiro

Xin Y: Great work!
Kei. H: Thanks.

@Kei. - when will you make it for your planned field work? Sorry if I missed it.

Kei. H. We are planning next field work around Siorapaluk in Feb-Mar 2023.
Jennie: Great - really looking foward to seeing that!

Comments for posters:
Peter Effertz

Nicolas Faure

Shuting Zhai

Discussion

Lucy: Jennie I think a working group on sea ice biogeochemistry of iodine is a good idea and I'd be happy to contribute to the open questions.
Great - I'll bring this up with Nadja and Megan, who lead this group. Our next in peson meeting is at SOLAS in Cape Town, with remote attendance

possible.
Here is the SCOR WG info: https://scor-int.org/group/coupling-of-ocean-ice-atmosphere-processes-from-sea-ice-biogeochemistry-to-aerosols-and-clouds-cice2clouds/

Session 3 - 2100-2300 UTC+2

Poster session

Introduction

Artificial neural network simulations of BrO in the Arctic troposphere BLECHSCHMIDT, Anne

Jennie: What is the impact of training the neural network with satellite obs vs. ground based obs? Can we use both?

Anne (@Jennie): This is interesting and something that could be tried. Using ground-based data other parameters could be used as input or be tried to simulate (such as O3)

Jochen: Machine learning methods tend to perform poorly when they extrapolate outside of their training data. Are you running into this problem by training the model only with one year?

Anne (@Jochen): The idea was not to necessarily create the "best" performing ANN. We were actually interested in the question if an ANN is able to "forecast" BrO just by providing weather and sea ice data as input. Funnily, Ilias also tested an ANN that was trained by a random selection of satellite BrO data and even then the ANN was not able to reproduce the long-term BrO trend.

Jennie: There is a nice poster from the NOAA group (Peter/Irina) on long term ozone trends, worth having a look at. Maybe a limitation here.

Anne (@Jennie): Thank you for the hint, I will have a look!

Nadja: 2007 was a very unusual year for the Arctic wrt sea ice , would it be better to use a more "regular" year as the main training year? Also I was curious about including snow as an input parameter
@Nadja - Jennie here - great comment!

Anne (@Nadja): Ilias chose the year with a strong relation to meteorology also partly on purpose. The ANN is only able to learn the relation between input and BrO if there are a couple of bromine explosions with "exemplary" conditions included in the chosen year. Providing too many different conditions eventually also not leading to the production of bromine confuses the ANN, so that the agreement to satellite data gets smaller. He also reproduced the whole time series by using a random selection of BrO data from 22 years as training and by choosing an early and late year of the time series, but this did not lead to an improvement in the results.

Bill Simpson: This is a cool study. It looked to me like your model's performance worsened for years far from 2007 (the selected year). Is this the case? What might have evolved?

Anne (@Bill): Thank you. It is true that the ANN performs best for the year that it was trained with. This is because the meteorological and sea ice conditions can still be close to the chosen days that were used for training (he used 70% of 2007 data for training and the rest for validation and testing). Looking at the trend, it is correct that the ANN reproduced MAM averages only on the magnitude of the 2007 MAM mean and also the spatial agreement degrades from 2009 onwards. The interesting question is what we are actually missing as input to the ANN to be able to reproduce the trend. However, I think it would be worth a try to use the ANN for short term future predictions.

Bill Simpson: I thought the wind speed depenendence was interesting. We observationally see that in multi-year data sets, there is a similar wind speed dependence -- peaking at middle wind speeds, lower BrO at low winds and high winds.

Anne (@Bill): This is very interesting! What kind of multi-year data are you specifically referriing to - MAX-DOAS? Do you have an idea why this pattern is observed (and modelled)?

Peter Peterson(@Anne): The theory I put forth was increased ventilation of the snowpack and higher boundary layers to explain the peak at moderate wind speeds (https://acp.copernicus.org/articles/15/2119/2015/) This is one of the papers Bill referenced in his talk.

Anne (@Peter): thanks for the reference, I'll read it!

The influence of snow on Arctic halogen chemistry
PRATT, Kerri

JRoberts: How high does Br2 get in interstitial air?
@Jim - terrible question- Jennie here. With pumping of interstitial air, this is hard to say quantitatively. It is enhanced compared to what is observed in the atmosphere. @Kerri will say more.
Kerri: @Jim - It can be very high (see Custard et al 2017, https://pubs.acs.org/doi/abs/10.1021/acsearthspacechem.7b00014) - ppb level when tens of ppt above the snowpack. However, as Jennie says, this level in the interstitial air depends on the snowpack ventilation/wind pumping, which we're perturbing when sampling directly from this interstitial air. So, we have just thought about it qualitatively in terms of trends, rather than exact mole ratios in the snowpack because of the dependence on ventilation.
JRoberts: @Keri - Thanks!

Shaddy Ahmed: Very nice talk Kerri and great job getting such a wide range of halogen measurements (and adding I2 to the list!). I was wondering if you have considered measuring ClONO2 or BrONO2 species? If yes, what would you say are the main difficulties of measuring such species? (Would be interesting to get an idea of XONO2 concentrations in the Arctic.)
Kerri: Yes! We've definitely tried, but I don't think we have the right ion chemistry in our instrument. Perhaps @JimRoberts can comment on that. We discuss in Wang & Pratt (2017, JGR, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017JD027175) that XONO2 are important for reactive halogen cycling, even at background NOx levels.
JRoberts: The problem with ClONO2 is that is makes NO3- readily upon reaction with I-, so one sees very little if any I-ClNO3 clusters. Don't know about BrONO2, but could be the same.
Shaddy Ahmed: Thanks for the reply @Kerri and @Jim
Thanks Jim!

Horizontal and vertical distributions of reactive bromine in the Arctic SIMPSON, Bill

@Bill - Jennie here. I feel that there is a confusion about your 2007 paper regarding if it's bare sea-ice or snow on sea-ice causing bromine activation based on the trajectory analysis. I just think we need to clarify for the community that most of the first year sea ice is snow covered (also also multi year sea ice), so that analysis is consistent with snow on sea ice activation. In our modeling for 2012, we consider FYI and MYI the same, there isn't much MYI and it may be the same for bromine activation based on your and Kerri's work. Trajectries are also sensitive to all PBL species (such as aerosols). Bill: Thanks! Yes, I think there is some confusion here. We indicated that it was snow on the first year ice, not the ice itself. The paper talks about "FYI areas" to mean that these are areas where the underlying surface is sea ice, but there is snow on top.

Jennie: Also thanks for the comments on PBL dynamics and aerosols aloft. All good points. Bill: Thanks.

Peter: Is there still a MAX-DOAS at Utqiagvik? How long of a record do you have? Bill: The UAF MAX-DOAS was moved out of Utqiagvik, I think around June 2018. We might bring it back, or we could figure out ways to get another one there. There is interest with Heidelberg and we are discussing that.

A cyclone-induced bromine explosion event in Ny-Ålesund, Svalbard YANG, Xin

@Xin - Jennie here - this is very similar to the work from the Bremen group regarding bromine activation where air is lofted above the surface in storms. I am wondering if our different obs data sets (surface in our case) and satllite remote sensing obs of BrO in your case, produce different results regarding the mechanisms that dominate bromine activation.

Peter Peterson - I think the differences in observational data definately do contribute, as satellite measurements tend to be less sensitive to shallow layer type events than ground based measurements. We are planning

on working with the TROPOMI BrO data to compare with aircraft data from CHACHA so hopefully we can shed some more light on this. I definately think more long term intercomparison type studies are needed, especially as satellite observations keep improving. It is a bummer that the MAX-DOAS in Utqiagvik is no longer there, hopefully @Bill or someone can get a new instrument in place. I think those long term observations are really valuable.

Additional comment from Jennie - We have so far not compared our focused model case studies with sat obs due to frequent low level clouds near the surface. Shaddy's work shows that the lowest part of the atmospehre (50 meters or so) is different from the rest above. This is also known from other studies with vertical profiles or towers. How should we deal with this when comparing with BrO tropospheric columns? This is a question for all remote sensing teams. I would be quite happy to follow up with anyone who wants to work together on comparing model results with satellite remote sensing in the future.

Kerri Pratt @Xin - Moore et al. (2014, Nature, https://www.nature.com/articles/nature12924) showed that open water caused vertical mixing that caused O3 and Hg(0) to recover. Do you see this in your data? How do you disentangle the SSA source from open water vs the difference in vertical mixing?

Role of blowing snow as a source of sea salt aerosol and halogens in polar regions
JAEGLE, Lyatt

Nadja: From the last presentations it seems that condidering modeeling snow salinity in ocean-ice snow models might be a useful step. Are their any thoughts on where the salinity is coming from? i.e from below(via the ice) from sea-salt aerosols at snow formation or deposition after snow fall? other?
@Nadja - Jennie here: Lucy C.'s talk has some data on this. @Bill had also data on this. It is my opinion that the sources and controlling processes are not clear.
Lyatt: The source of salinity is likely a combination of wicking from the sea ice below and from deposition (+frost flowers). Our approach was to look at observed vertical profiles of salinity in snow over sea ice.
Peter Peterson: I think wicking is probably only a big contributor for

surface snow in shallow snowpacks, I think the wicking is limited to the lowest ~20 cm of the snowpack or so based on prior work by Florent Domine. Of course snowpacks are projected to get shallower in the future due to delayed sea ice formation, so it could become more relevant for surface snow

Kerri Pratt: @Lyatt In May et al (2016, JGR, https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2016JD025273) we examined several years of sea salt aerosol data at Utqiagvik, showing a dependence on wind speed and lead presence being necessary. Then in Kirpes et al 2019 (ACS Central Sci, https://pubs.acs.org/doi/10.1021/acscentsci.9b00541) we showed that leads produced the measured sea salt aerosol, based on the organic coatings observed that were indicative of bubble bursting.
Lyatt: Thanks for these Kerri. It would be great to understand whether the same source function for SSA applies for opean ocean emissions and emissions over open leads. Currently, that's what we are using (same source function).
Nadja: cool, thanks both. I will check the talk & paper. If there is a link with wind
speed and leads it is more likely related to deposition processes, hence more difficult to model in ocean-only models

Bill Simpson: Talking about Glen Liston, doesn't he have a blowing snow sublimation model? Can this be coupled into blowing snow aerosol particle formation processes?
Lyatt: Yes, Glen does have blowing snow in his SnowModel-LG, we haven't been using it yet, as we are calculating blowing snow online using MERRA-2 meteorology. We are planning on comparing the two blowing snow formation simulations. Bill@Lyatt: That sounds great. My understanding is that the RH and sublimation processes are hard to deal with, so having multiple views at this would be very informative. Great work!

Comments for posters:
Peter Effertz

Nicolas Faure

Shuting Zhai

Discussion