Improvement and calibration of clouds in models

LES, Observations and Parameterizations (II) Session

April 15, 2021, Météo-France, Toulouse, France

Feel free to ask your questions through this document during or after the different talks.
The objective is to be able to extend discussion as there is no real coffee break.
Please indicate your name with your question

- 1- Convective momentum transport through the subtropical shallow convection in LES simulations

Vishal Dixit, Louise Nuijens, Kevin Helfer

Does atmospheric moist convection transport horizontal momentum upwards to significantly impact surrounding flows? This has remained an enigma in spite of considerable efforts. We utilize unique multi-day large eddy simulations run over the tropical Atlantic under German HDCP2 project to evaluate the convective momentum transport (CMT) effects by shallow convection.For typical trade wind profiles during boreal winter, the convection acts as an effective “cumulus friction” to decelerate the north-easterlies. This effect is maximum near the cloud base while in the cloud layer, the effects are weak but are distributed over a relatively deeper layer. In the cloud layer, the zonal component of the momentum flux is counter-gradient and penetrates deeper than reported previously. The transport through convective updrafts and downdrafts captures the right sign of the counter-gradient momentum transport but underestimates it severely.

Question 1 <Najda Villefranque>: sorry if I missed it ; how do you identify your updrafts and downdrafts (I saw the criteria in the legend of your figure but is that for individual cells or coherent 3D structures?) -- Thanks :)

Vishal: Thanks Najda, we applied the criteria for individual cells. Looking at coherent structures is currently ongoing.

Question 2 <Frédéric Hourdin>: If I got it well, you say that mass fluxes are not enough to explain the momentum transport ? Is the downard compensating transport included in your computation ? Did you try to look at this in some classical 1D cases in LES : Ayotte, Armcu … My experience is that using mass flux parameterizations greatly improves the representation of momentum transport. You may have a look at Figure 2 3 4 and 11 in https://www.lmd.jussieu.fr/~hourdin/PUBLIS/Hourdinetal2002.pdf that shows a clear improvement of the vertical profiles of U and V (we did not evaluate the momentum flux itself). But may be the environment of those 1D reference cases is too simple.

Vishal: Yes, we accounted for the compensating downward transport. The downward transport is important in cloud tops (probably related to subsiding shells). I haven’t looked at 1D cases, thanks for sharing this reference. **Frédéric** : if you have relevant diagnostics for climate models, we would be interested to see if the implementation of mass flux transport of U and V by our “thermal plume model” in IPSL-CM5B and IPSL-CM6A improves those diagnostics. We have some evidence at least that it improves the representation of near surface winds significantly https://www.lmd.jussieu.fr/~hourdin/PUBLIS/Hourdin2015.pdf I would be interested to have a copy of your paper when available. **Vishal:** Thanks, I will look into your paper. Louise is collaborating with ECMWF to look at effect of CMT on surface winds. First version of our submitted paper is available **here** but I will share a published version with you soon. **Frédéric :** thanks.

Question 3: (Hannah Christensen) If I understand correctly, these circulations are missing in conventional parametrisations of CMT. Have you thought at all of how you could include this effect?

Vishal: Yes, most present day climate models do not include CMT due to shallow clouds and few which do tie it to mass transport (updrafts and downdrafts). I think one way of including the full transport is to adapt EDMF for shallow CMT or follow CLUBB like approach which Vince Larson advocates, except that the Gaussian distributions need to be based on large domain LES. But, it is probably too early to comment on this without testing :)

Question 4 <Roel Neggers>: Very nice talk, indeed momentum transport is often overlooked but is crucial to get right in parameterizations. I was wondering how periodic boundary conditions in an LES can harm the chain of processes that you described in the schematic figure. Do they break or affect the gravity waves when the domain is too small?
Vishal: Thanks Roel. Yes, I think the scale of cloud circulations are significantly larger to hamper the growth of adjacent cloud system. This could happen because gravity waves interact with the circulation surrounding clouds but mainly through hampering/ influencing adjacent cloud growth. Another point is that, if the domain is too small then the cloud systems are not in hydrostatic balance and there is a significant buoyancy residue (we confirmed this by sampling budget terms through a smaller domain ~25km). Conclusion one draws for parameterisation depend upon how much transport occurs through buoyancy residue vs through circulations. Badlan et al. (2017) also highlight this point for deep convection. **Roel:** Thanks Vishal, I will read that paper. Did you, or are you planning, to write a paper about this impact of domain size? **Vishal:** Partly these findings about the influence of domain size are included in our present paper, but a more extensive analysis can be done. At the moment, I am looking at CMT impacts on large-scale circulations, but hope to come back to this aspect in the recent future.

Question 5 <Maxime Colin>: Fascinating talk! Sorry if I missed it, but how did you originally define the counter-gradient flux layer? And why are you allowed to use hydrostatic balance for these convective clouds?
Vishal: Thanks Maxime! Great to virtually meet you. Counter-gradient / Up-gradient flux layer is defined such that the sign of momentum flux and mean wind shear is same in that layer. A couple of implications: 1. This layer consumes TKE, 2. Favours enhancement in the shear as opposed to downgradient diffusion that weakens shear.
We figured that when considered over big domain (100kmx200km) these systems are embedded in quasi-hydrostatic environment, with small buoyancy residue driving circulations.

2- Turbulent effects and dynamics of the cloud-environment interface in a LES of a growing cumulus congestus

Clément Strauss, Didier Ricard, Christine Lac

A Large-Eddy Simulation (LES) of a cumulus congestus has been performed using the Méso-NHmodel with a 5-m resolution in order to study the fine-scale dynamics and mixing on its edges. Toroïdal circulation eddies have a strong signature on the resolved turbulent fluxes.

A partition of the cloud and its environment is used to characterize the dynamics, buoyancy and turbulence near the cloud edges. At the vicinity of the cloud-environment interface, downdrafts caused by eddies coexist with a buoyancy inversion while the cloud interior is mostly rising with positive buoyancy. Turbulence on the edges is finer scale than inside the cloud.

An alternative simulation, where evaporative cooling effects are suppressed, indicates that those effects are mainly present near the edges and contribute to attenuate the convective circulation. Evaporative cooling has also an impact on the buoyancy inversion and on the path of the entrained air.

Question 1 <name>: Is the toroidal circulation really different from the concept of the subsiding shell or just a higher resolution picture of the same thing?

Question 2 <Fabian Senf>: May be I missed it. How do you describe the (mixed-phase?) microphysics of the towering Cu? … great work, BTW…

Clément Strauss: We used the ICE3 microphysics scheme from the Meso-NH model

Question 3: <Richard Forbes> Great talk. The evaporation at sub-grid cloud edges is an important term in prognostic cloud schemes. Can your results lead to an improved physical formulation/insight of such a parametrization?

Question 4 <Maxime Colin>: Do you know if the Kelvin-Helmholtz instability at cloud edges play any significant role?

Question 5 <Maxime Colin>: How did you diagnose or estimate the entrainment at the eddy bases from your images?

3-Improved Ice Aggregation Formulation in a Two-Moment Microphysics Scheme

Markus Karrer1, Axel Seifert2, Davide Ori1, Vera Schemann1, Stefan Kneifel1

Accurate simulation of ice aggregation is important for precise precipitation prediction, especially when sophisticated models are used, e.g. two-moment bulk microphysics schemes. We simulated 43 days with ICON-LEM and applied a radar forward operator to perform a statistical comparison between synthetic and observed multifrequency radar measurements. The analysis reveals an overestimation of the Doppler velocity and snow particle sizes at higher temperatures.

We implemented new particle property relations inferred from aggregation modeling and updated other microphysical parameters that affect aggregation rates (e.g. formulation of the aggregation kernel, sticking efficiency). This allowed us to evaluate the sensitivity of aggregation to these parameters. The particle properties and the aggregation kernel have the largest impact and their improvement in the scheme leads to smaller biases in the simulated Doppler velocity and snow particle size.

Question 1 <Etienne Vignon>: Great talk! If I understand well you assume that the increase in Doppler velocity and particle size is mainly due to aggregation. Do you have an idea if and how much riming plays a role in your case study?

Answer MK: From the simultaneous view of three different frequencies and mean Doppler velocity we know that moderate or heavy riming is rare (https://essd.copernicus.org/articles/11/845/2019/), we can not completely exclude slight riming, though.

Second question is: aggregation can be enhanced by turbulence (and this effect is often missing in Estick parameterizations). Have you looked at the Doppler spectral width in the "warm" layer? Thank you very much for your answers.

Answer MK: Good point! We have not looked into the details of the spectral width, but we will have a closer look at the whole Doppler spectrum, soon. From our experience spectrum width in ice clouds are often very small (below 0.1). Most often we find wind shear to induce some more turbulent layers. There are also well-tested methods to retrieve Eddy-Dissipation rate from mean Doppler velocity time series. The advantage would be that this method is less sensitive to microphysical effects which often also affect the spectrum width. But we should definitely put more effort in estimating the effect of turbulence on aggregation, so thanks for that suggestion!

Question 2 <Richard Forbes>: Your results look much improved for T warmer than -15, but colder than -20 the results looked further from the observations with the new formulation. Do you understand why?

Answer MK: Thanks for your question! We focussed indeed on the warmer temperatures, because there our measurements are most sensitive (we need a certain aggregate/particle size in order to get a detectable dual-wavelength ratio DWR for our used frequencies X,Ka,W). It might be that we could improve the sticking efficiency parameterization at colder temperatures. At these temperatures also the parameterization of cloud ice properties (mass-size, velocity-size) plays an important role. We didn't try to optimize the cloud ice properties, because we have low constraints on them. It might be nice to extend our observational analysis and setup (we might need a radar with higher frequency, e.g. G-Band) to learn more about the cold temperatures.

Question 3 <David Neubauer>: You will have to assume a relationship between size and area of the snow particles. How do different assumptions for this relationship affect your results?

Answer MK: Thanks for your question! We take mass-size, area-size and velocity size from the same 3D aggregate shapes (https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020MS002066) to have these parameterizations consistent. And decided to take these aggregate shapes, which matched the observed velocity-size relation the best. Therefore, we have not tested different area-size relations yet.

Question 4 <Steve Sherwood>: If I understand right, your main result arises from the asymptoting of the fall speed to a constant as R increases beyond a certain size, so that collisions cease and size is limited? Or is that an oversimplification? This would be a really interesting result. Nice talk.

Answer MK: Thanks! The formulation of the aggregation kernel (A-kernel vs D-kernel) and the asymptotic behaviour of the particle terminal velocity appears to be of similar importance**.** In the 2-moment scheme the variance of particle velocity is considered by assuming Gaussian velocity variance (at a fixed size). So the aggregation does not cease completely although the mean particle velocity approaches a fixed value.

Question 5 <Masahiro Sawada>: Great work! Did your modified microphysics improve the representation of rainfall intensity and/or rainfall amount at surface? I expect that the improved representation of particle velocity & size would improve the rainfall at the surface but it is not always true.
Answer MK: Thanks! Indeed, the modified microphysics impacts rainfall intensity and amount. In some cases more (when a sublimation layer is present) and in some cases less. However, looking at the precipitation at the observational point (no 2D field) of the whole campaign it seems that precipitation intensity is now slightly too weak. In simulations with a larger domain (whole Germany) and summer cases the simulations with new microphysics tend to underestimate moderate rain. Most likely there were compensating errors in the default model and we removed one of the error sources.

4-Process-oriented Evaluation of cloud parameterizations using a Cloud System Concept
Claudia J. Stubenrauch1, Marine Bonazzola1, Graeme L. Stephens2
Recently GEWEX initiated working groups on Process Evaluation Studies (PROES) to provide observational based metrics for a better understanding of physical processes. Within the framework of UTCC (Upper Tropospheric Clouds and Convection) PROES we are creating a synergetic database of UT cloud systems. Convective cores, cirrus anvils and surrounding thin cirrus are identified using cloud emissivity derived from IR Sounder observations. Lidar - radar observations of CALIPSO-CloudSat and TRMM provide information on the vertical structure and precipitation of these systems, essential to determine their heating rates. This cloud system concept allows to relate the anvil properties to processes shaping them.
We present the utility of this process-oriented observational metrics (relations between cloud system properties and proxies mimicking their life stage and convective depth) for the development of a more coherent bulk ice cloud scheme in the LMDZ climate model.

Question 1 Sophia Schäfer: What data and retrieval methods do you use for heating rates, and how exact are they?
<Claudia>: We use NASA CloudSat-lidar V4 FLXHR radiative heating rates which use CALIPSO-CloudSat profiles of different retrieved variables. Then we train the neural networks to

predict these HRs from AIRS cloud properties and ERA-Interim atmospheric properties. You can get more details in Stubenrauch et al. ACP, 2021 : doi:10.5194/acp-21-1015-2021

Question 2 <Kwinten Van Weverberg>: Is there any subgrid variability in ice cloud cover in LMDZ (i.e. cloud fraction scheme), or do you assume ice to cover the entire grid box?

<Claudia>: Phase depends on temperature, so I assume that each individual layer includes all ice if its temperature is according, for the CIRS simulator we use max overlap between contiguous cloud layers to get subgrid structure of Cb, Ci, thin Ci (so then one deep cloud can have ice on top and liquid lower)

Question 3 <Fabian Senf>: The cloud system concept is a really interesting approach! Does it involve the detection of convective cores and supported anvils via object-based methods?

<Claudia>: Thanks Fabian. It is actually quite simple in principle: in a first step we merge adjacent grid cells of similar cloud pressure (within 50 hPa) and in a second step we use emissivity to distinguish convective cores (>0.98), Ci (0.5-0.98) and thin Ci (0.1-0.5); however since we have frist gridded the data of 0.5 deg, there are a few technical issues; when we go to GCM resolution we also used in addition rainrate for identification; you can find more information in Protopapadaki et al. ACP 2017doi: 10.5194/acp-17-3845-2017

Fabian: Thanks for the answer! A follow-up question: Do you see potential for synergistic use of geostationary satellite data in your approach, e.g. for cloud system tracking?

<Claudia>: Actually, we want now to use Lagrangian transport study to see if we can follow these cloud systems; for this we combine AIRS and IASI data to reconstruct cloud systems at UTC (the ones we have constructed so far were at local times 1:30 AM, PM and 9;30 AM, PM. We will check with geostationary data how the results compare.

Question 4 <Maxime Colin>: Thank you Claudia, that was very informative. How good is the estimate of system age by the convective core fraction measure?

<Claudia>: Thanks Maxime. Convective core fraction has been used already by Machado & Rossow 1993 as a proxy using ISCCP data, but they used TB to build cloud systems; as TB depends on both height and opt depth, one can only follow very cold systems; however they found that the coldest systems have le longest life times. Even using pcld and emissivity of cld (>0.98), the convective cores include part of the thick anvil; but nevertheless these proxies still give interesting insight.

Question 4 <Masaki Satoh>: How complicated your ice microphysics scheme?

<Claudia>: The current LMDZ uses a bulk ice scheme with vm = f(IWC) & De=f(T); as we looked for an example to use the cloud system approach for evaluation of a new scheme, we only looked at improving this bulk ice scheme; there are other colleagues at LMD developing a more complicated microphysical scheme, which takes time; but already making this bulk ice scheme more coherent first by coherent presentation of vm & De (making sure that they are well related) and by making them both dependent to IWC & T, in agreement with many different field campaign observations, shows improvement in the UT clouds.

- 5- Improving convection in LES through detailed land-surface modelling

Bart J.H. van Stratum, Chiel C. van Heerwaarden, Jordi Vilà Guerau de Arellano

LES is skilful in forecasting convection because of its explicit representation of turbulence and convection. However, this skill strongly depends on a correct partitioning of the available energy over the surface heat and moisture fluxes. Our goal is to improve the representation of the land-surface in LES, through a detailed representation of the (plant) physiological processes, and spatial properties of the surface. First, we setup a new testbed for performing LES of realistic weather, forcing LES with the dynamic tendencies obtained from the dynamic core of HARMONIE-AROME. Validation of a multi-week LES experiment shows that this setup manages to capture the wide variety of weather phenomena observed in reality. Second, we are developing a new land-surface model, including a high-resolution database of the surface properties of the Netherlands. We demonstrate the land-surface model in offline mode (forced by ERA5), running in high resolution over a central part of the Netherlands.

Question 1 <Roel Neggers>: Nice talk Bart, great to see the testbed being continued! Maybe this would be a somewhat ambitious goal, but are you planning to simulate 70 years of weather at Cabauw with microHH? Would be awesome..

Thanks Roel! 70 years would indeed be challenging, for some of the work that we are planning, we are aiming for ~1 year of LES. For the validation for now, we will probably run one month for each site.

Question 2 <Fleur Couvreux>: The 1-month simulation you show i s a 30x1-day LES simulation or 1x30-day simulation? What about surface information ?

At the moment it consists of 30x1 days, we previously (at KNMI) also worked with 1x14 days, but the differences were quite small. We include a nudging term where the mean state of LES is nudged towards the mean state of ERA5, to prevent LES from drifting away. This is probably the reason why there are only small differences between the two approaches. The surface is at the moment initialised from ERA5, which works well without spinup, because the land-surface models in MicroHH and ERA5 are very similar.

Question 3 <Ashwin Mohanan>: Two questions 1) Have you investigated the effect of boundary conditions and sub-grid scale models in the log-layer? Or to put it this way: do you get a good match of the LES log-layer with MOST while using MicroHH? 2) Is LS2D meant to aggregate a set of standard test cases for validation of LES studies or initial conditions alone? What I mean is will it serve as only **input (intial conditions, forcings etc)** for a study, or will it also have some **reference dataset (statistics)** to compare against?

1) With the method that we use, there is a bit of manual tuning for the chosen resolution required. We use the rather simple wall damping of Mason and Thomson (JFM, 1992), which corrects the mixing length and hence the eddy viscosity close to the wall. Better, grid independent, alternatives are very welcome that do not involve the costs of a dynamic SGS model.
    a) One possible well-known SGS model would be Vreman (POF, 2004) which does not require wall-damping. We are looking into boundary conditions and it is a work-in-progress :)

2) Our main goal with LS2D was to create a tool to easily generate real-life LES cases, both for single day case studies, or longer experiments to look at atmospheric processes from a more statistical perspective. It's primary task is to generate only the LES input, the availability of reference data (e.g. observations) varies wildly from site to site, so that is difficult to automate.

-6-Application of Remote Sensing to Study Dynamical and Morphological Characteristics of Meso-Scale Convective Systems to Develop 'Thunderstorms Numerical Prediction Model (TNPM)'Over Tropics'

Virendra Kumar Goswami

The large scale kinematic and thermodynamic behavior, evolutionary features & 3D structure of selected mesoscale Convective Systems, e.g. intense Cloud Clusters, and Severe Thunderstorms, NHCZ & SHCZ, SSTs would be investigated by using Aircraft, Doppler Weather Radar, conventional, and Satellite data fitted with Lightning sensors & CubeSats carrying high-frequency passive microwave sensors, over the domain.

The values of characteristics, e.g. lifetime, distribution, trajectories, size, &3Dstructure, i.e., the vertical extent of these systems would be computed in order to develop a 'Thunderstorms Numerical Prediction Model' for Asian, Tropical, Mid-latitude.

Based on Goswami's 'Cluster Coalescence Theory' & 'Giant Cluster Theory'; regional/sub-regional tropical Cyclone Forecasting Model (TCFM)' would be developed using satellite imageries & computation of deep convective mass transport inside the thunderstorm-cell through Cloud Tracer Analysis.

Question 1 <name>:

Question 2 <name>:

-7- Evaluation and improvement of cloud and precipitation processes of NICAM with ULTIMATE

Masaki Satoh1, Woosub Roh1, Shuhei Matsugishi1, Yasutaka Ikuta2, Naomi Kuba1, Hajime Okamoto3

We propose a collaboration study between numerical models and ground remote-sensing observation data over the metropolitan area of Tokyo. The initiative is called ULTIMATE (ULTra-sIte for Measuring Atmosphere of Tokyo metropolitan Environment), in which using an intensive observation data in the Tokyo area together with satellite observations, cloud microphysics schemes of numerical models are evaluated and improved.

We will consider the use of observation data planned for ground validation of the EarhCARE satellite and the dual-polarization Doppler weather radar, which is now in operation at the Japan Meteorological Agency. We particularly focus on the evaluation and improvement of the Non-hydrostatic Icosahedral Atmospheric Model (NICAM), which can be used seamlessly on both global and regional domains, allowing us to quickly test the improved scheme on a global scale, compare it with satellite observations, and estimate climate sensitivity.

Question 1 <Fleur Couvreux>: Can you conclude to a significant improvement from a 2nd moment microphysic scheme? Also with your nice data set over Tokyo, can you say there is an impact of the city on occurrence of deep convection?

A1 <Masaki Satoh> Current 2nd moment scheme of NICAM is best. We have modified the single moment scheme by comparing the 2nd moment scheme. Now it is comparable in terms of super cooled water.

We have not detected the city impact on deep convection, because just a short period of the analysis and comparison. To show the impact, more statistical analysis is needed.
Fleur=> thanks for your answer

Question 2 <Chiel van Heerwaarden>: My question is a little bit out of scope, but are the (improvement to the) schemes you presented also usable at LES-type resolution (~25 m)? We are using the NSW6 scheme for RCE-like simulations, but have some issues with some of the processes.

A2 <Masaki Satoh> I think the schemes in NICAM can be used for LES. An example of the use in LES can be found in Sato et al.(2015,PEPS, https://doi.org/10.1186/s40645-015-0053-6). What kind of issues?

Thank you for the paper link. In Tomita's 2008 paper there is the Bergeron process that has the timestep of the model in there (Eq. 72). This does not play out well in our LES, and it also makes the term go to very large values at small time steps.

A3 <Masaki Satoh> Please note that dt1 in (72) is given by (73), and not a model time step. This term is not affected by the model time step.

A.6 Bergeron process

According to Hsie et al. (1980), the Bergeron process is formulated as

$$P_{SFW} = N_{I50}(a_1 m_{I50}^{a_2} + \pi E_{IW} \rho q_c R_{I50}^2 U_{I50}), \quad (71)$$

$$P_{SFI} = q_i / \Delta t_1, \quad (72)$$

where

$$\Delta t_1 = \frac{1}{a_1(1-a_2)}\left[m_{I50}^{1-a_2} - m_{I40}^{1-a_2}\right]. \quad (73)$$

<Chiel van Heerwaarden> Sorry, I did not put the next eq, but in (74) there is a dt in the numerator of the equation, which means that N150 has a limit of zero if dt goes to zero.

<Masaki Satoh> In general, the B-F process is when the time step of the climate model is long.
It deals with evaporation of cloud water and sublimation growth of cloud ice.
(e.g. Khvorostyanov and Sassen, 1998, JAS,
https://doi.org/10.1175/1520-0469(1998)055<1808:CCSUEM>2.0.CO;2)
When the time step is short, as in LES, double counting will occur. It should not be used.
It seems that the correct behavior is that it will not work when dt is short.

- 8- Aerosol-cloud-turbulence interactions in well-coupled Arctic boundary layers over open water -

Jan Chylik, Roel Neggers

Recent field campaigns in the Arctic have successfully collected state-of-the-art datasets on low level mixed-phase clouds, however key aspects still prove hard to measure. We present complementary high-resolution simulations to serve as a virtual laboratory, being properly constrained by relevant measurements. The focus lies on gaining insight into interactions between aerosol, hydrometeors and turbulence.
A composite LES case is constructed based on dropsonde profiles collected by collocated airborne measurements in a cloudy boundary layer over open water in the Fram Strait as observed during the ACLOUD campaign on 18 June 2017.
The results highlight the impact of ice hydrometeors on the thermodynamic state and turbulence of the Arctic boundary layer. Furthermore, we find that the variation in aerosol concentrations also modifies the structure of the turbulence. Opportunities created for the evaluation of parameterizations for Earth System Models will be highlighted.

Question 1 <Fleur Couvreux>: There is a very strong impact of the CCN on the vertical profile of buoyancy and structure of the clouds. I may have missed some information but one issue with the 2-moment microphysics is to be able to strongly constrain the CCN. What is the CCN uncertainty we have from observations?

A1 <Jan Chylik>: That are good questions. Firstly, the CCN uncertainty: there are high-quality observations of aerosols of various sizes, however it is not yet clear how good are said aerosols in acting as condensation nuclei at given supersaturation. Secondly, there is high spatial variability in aerosols.

Regarding the 2-moment microphysics scheme, we have tried both an approach with constant CCN parameters, as well as an approach where CCN concentration is treated as prognostic variable.

Question 2 <Maxime Colin>: Sorry for the basic question, but how important is moist convection in polar regions? Do convective clouds in polar regions produce substantial surface rain in observations?

-9-Realities of developing and improving parameterizations related to clouds in global climate models

Hideaki Kawai, Seiji Yukimoto, Tsuyoshi Koshiro, Naga Oshima, Taichu Tanaka, Hiromasa Yoshimura, Ryoji Nagasawa

The aim of the presentation is to introduce realities of developing and improving schemes related to clouds in global climate models. The representations of clouds in climate model MRI-ESM2 used in CMIP6 simulations are significantly improved from the previous version MRI-CGCM3 used in CMIP5 simulations. The score of the spatial pattern of radiative fluxes at the top of the atmosphere for MRI-ESM2 is better than any of the 48 CMIP5 models. We will show comprehensively various modifications related to clouds, which contribute to the improved cloud representation, and their main impacts. The modifications cover various schemes and processes including the cloud scheme, turbulence scheme, cloud microphysics processes, the interaction between cloud and convection schemes, resolution issues, cloud radiation processes, the aerosol properties, and numerics. We would like to emphasize that the improvement of performance in climate models is ordinarily contributed by many minor modifications.

Question 1 Fabian Jakub: You show a multitude of different reasons why the model improved. Are you working on disentangling the various contributions and how they relate to each other? (Your talk answered my question :) Thanks!)

A 1 <Hideaki Kawai>: Thank you!

Question 2 <Frédéric Hourdin>: Seems that the "Distress and dilemmas in developing and tuning models" (*it is partly a private joke, using the title of a very good presentation by Hideaki at the conference on model tuning in 2014*) is far behind you. How much would you attribute to parametrizations improvements and to model tuning ? **Good job !** With LMDZ, we had bad surprises when deactivating mass flux scheme in the stratocumulus clouds, based on a threshold on the inversion strength.

A 2 <Hideaki Kawai>: Thank you, Frederic! Actually, we need tuning at the final stage of model update to match 20C temperature increase etc. But at least, we cannot improve model performance just by tuning. We need improvements in schemes, as you know. In fact, we did not tune cloud related parts a lot in MRI-ESM2 (section 3.11 in Kawai et al. 2019 GMD https://gmd.copernicus.org/articles/12/2875/2019/gmd-12-2875-2019.pdf). As for deactivation of shallow convection scheme, I guess the effect depends on the schemes a lot. (It's possible that the behaviour of shallow convection in our model could be a little strange. That can be one of the reason of the good effect of suppression of shallow convection in our model.) Let's discuss it in the future. =)

Frédéric : no doubt we need improvements in schemes. Quite convincing. So you suggest you did not have to tune your parameterizations to get the latitudinal distribution of CRE right for instance ?

Hideaki : Basically, we didn't. For example, using ECTEI contributed to good latitudinal cloud distribution to some extent. ECTEI is larger (more low clouds) over mid- and high- latitudes (compared to tropics) than in the case of EIS. It is very helpful for more low cloud cover over the Southern Ocean.

Question 3: <Martin Köhler> Nice! Where did the improvement in the Southern Oceans came from? Sorry, I didn't understand the 2 important changes**. Thanks!**

A3 <Hideaki Kawai>: Thanks, Martin! (New stratocumulus scheme & Suppression of shallow convection for Low cloud cover.)

Question 4 < Fleur Couvreux> : Thanks for your presentation. The change in the Stratocumulus scheme is it just the switch from using the CTEI towards the ECTEI and or were there other changes in the stratocumulus scheme?

A4 <Hideaki Kawai>: Thanks! Actually in our old version of stratocumulus scheme, we gave stratocumulus cloud cover directly (diagnostically, as an exception of Tiedtke cloud prognostic scheme) using inversion strength, which is similar to Slingo scheme (Kawai & Inoue 2006). In our new scheme (Kawai et al. 2019 GMD), we only control the cloud top turbulent mixing (Tiedtke prognostic cloud scheme forms stratocumulus clouds naturally). Simulations using EIS and ECTEI give similar results basically. But when we use ECTEI, it forms more low clouds over the Southern Ocean because ECTEI corresponds to more low clouds over low SST oceans.

-10-Sensitivity of ice formation processes in the ice modes scheme

Tim Lüttmer, Peter Spichtinger

Common microphysics bulk schemes only consider a single ice class which includes sources from multiple formation mechanisms. We developed and implemented a two-moment microphysics scheme in the atmosphere model ICON that distinguishes between different ice modes of origin including homogeneous nucleation, deposition freezing, immersion freezing, homogeneous freezing of water droplets and secondary ice production from rime splintering, frozen droplet shattering and collisional break-up, respectively.

Model assumptions, e.g. choice of nucleation parameterizations and representation of ice nucleating particles, affect crucially the time evolution of clouds in the simulations. Using our newly developed bulk scheme we can determine the contribution of the various ice formation mechanisms to the total ice content and evaluate their radiative effect, respectively. We performed sensitivity studies for idealized as well as for synoptic cases in ICON in convection resolving resolution.

Question 1 <Fabian Senf>: Tim, nice talk! Do the different ice modes transfer differently to graupel, snow & hail? And do you assume different shapes for different modes?

Tim: All ice modes interact with other cloud particle classes in the same way. We also assume the same shape for each mode, because ice crystal geometry will more strongly depend on the temperature and supersaturation regime than on the ice formation process.

Question 2 <Jan Chylik>: Thank you for an interesting talk. The ice classes you present are based on different sources of ice production, however have you also considered different geometry of ice crystals due to different temperature ranges?

Tim: In general the two moment bulk scheme in ICON does not consider a change of ice geometry during the simulation. We considered tying ice shape to the ice mode, but it is hard to find a rigid and general relationship between ice formation process and shape. And such a relationship is likely to complex to be resolved in a bulk scheme.

Question3 <Kwinten Van Weverberg>: Interesting talk. Do the different initiation processes of ice formation happen in a particular order within a time step and how to decide how the processes compeed with one another for water vapour?

Tim: Yes, the scheme uses a consecutive order of microphysical processes. Nucleation and freezing of particles first, deposition nucleation next and collision processes at the end. For deposition growth we use the semi-analytic approach from Morrison et al (2005), where we estimate the relaxtion time scale of each ice particle class and solve the depositional growth for all classes at the same time.

-11- A new diagnostic cloud scheme for heterogeneous moist parameterisations
Martin Köhler
In the ICON model parameterisations of microphysics, turbulence and convection provide a heterogeneous set of information acting on moisture and cloud that is then fed to radiation through a filter called the cloud cover parameterisation.
We are presenting a concept for a new cloud cover parameterisation that is based on a total water PDF with at least three moments - mean, variance and skewness - such as provided by a double-Gaussian or beta function. These three moments are then determined as follows: the mean uses grid mean water quantities from the dynamical core, the variance comes from the TKE turbulence scheme and the skewness is based on statistics from the convection scheme.
First SCM model results evaluated against LES will be presented as well as impacts on radiative fluxes in a set of global ICON simulations.

Question 1 <Kwinten Van Weverberg>: Interesting talk! Do you take temperature variability into account as well in the cloud scheme, or do you assume the humidity variability to dominate (I think the Tompkins scheme makes that assumption as well)?

Martin: I am not sure yet if I should include temperature variability and if it's important and we can gain information.
Thanks for your nice talk as well. I might try your ideas of the entrainment zone. Yet, I think your parameterisation works best with Adrian Lock's 7-type BL scheme.

Question 2 <Fabian Senf>: Martin, nice overview! In the beginning of your talk, you showed a rather long list of differently used cloud cover definitions: Do you think that a single & fully consistent CC definition can be achieved sometime in future?

Martin: To run microphysics as 1/0 at very low resolutions (160km for ICON-seamless) is giving you biases because the seen mixing ratios are biased low - because of low-resolution averaging. On the other hand the cloud cover needs to be very robust for the test of running all parameterisation with a unified cloud cover. It will be easier to unify cloud cover for turbulence and radiation.

Question 3 <Roel Neggers>: Nice talk Martin! Good to see that these super large domain simulations are used to inform cloud parameterization this way. Do you think the fits of the functions depend on the cloud regime or region, for example land versus ocean? Philipp Griewank recently looked at scale dependence in these functional fits for cloud fraction distributions for ICON simulations for Germany, maybe that could be of interest: https://doi.org/10.1029/2018MS001421

Martin: Thanks for the reference. The parameterisation of course has to work uniformly globally. But there are also if-statements like "strcu on/off" in Hideaki and IFS. So I would like to look on the one hand at DYAMOND (2km globally) and also 300m EUREC4a.

Question 4: <Richard Forbes> Thanks Martin, great to see this development! You made the decision to develop a diagnostic rather than a prognostic scheme. So do you think the memory in a prognostic scheme is secondary to the other issues?

Martin: The question of memory is most important for deep convective anvils. Convection is in quasi-equilibrium and the cloud scheme should do the same. So memory would be given by the continuous convection activity. But with a convection parameterisation that switches on/off I have a problem. Therefore I will test some memory.

Comment 5: <David Neubauer> Interesting that turbulence and convection should be considered in the cloud cover scheme. Steffen Münch has recently included these as source terms for cloud cover in a new prognostic cloud cover scheme in ECHAM-HAM: https://doi.org/10.1029/2019MS001824\

Martin: Thanks for the reference.

Question 6: **<Danahé Paquin-Ricard>** I'm not sure what you mean about the microphysics scheme at low resolution; is it "run" at low resolution or only the diagnostic cloud scheme will provide "large scale clouds"? if so, at which resolution, the microphysics scheme is "activated"? So, the cloud diagnostic will "add" a cloud source?

Martin: Sorry, for confusing. The ICON runs from LES (~100m) to global coupled climate resolutions (~100km). The microphysics runs at the respective resolution. The error of using the grid-mean water variables for microphysics gets worse for low resolution because of the power-law total water behavior.
The cloud microphysics and the cloud cover scheme are both running. The responsibility of the cloud scheme is the information about the sub-grid-scale information. It is based on sources from turbulence and convection. This sub-grid info needs to be passed to microphysics, which it is not at the current moment.

Thanks, I understand now!

Question 7 <Maxime Colin>: Was the TKE scheme the only scheme used to represent boundary layer processes? There was no specific boundary layer scheme active, right?

Martin: We use the TKE scheme to represent the BL as well as any turbulent transports throughout the atmosphere. Of course, below the lowest level we have a surface layer description. We don't have any specific diagnostic about BL or cloud top such as the BL schemes in the UM or IFS.

-12- Improvement of a representation of mixed-phase clouds, and its impact on a global cloud-system-resolving simulation

Akira T. Noda1, Tatsuya Seiki1, Woosub Roh2, Masaki Satoh2,1, Tomoki Ohno1

Many GCMs have suffered from an underestimation of low-level mixed-phase clouds in mid-to-high latitudes, leading to an underestimation of solar albedo. Reducing this bias is thus important for improved projection of a future climate. Roh et al. (2020, JAS) and Seiki and Roh (2020, JAS) recently revealed major sources of the underestimated mixed-phase clouds in a one-moment bulk cloud microphysics scheme: overestimations of the Bergeron-Findeisen and riming processes and a growth rate from cloud water to rain. This presentation applies their scheme to a global atmospheric model, NICAM, to investigate its impacts on clouds over the global domain. A 14-km mesh NICAM experiment shows improved reflection of solar incident not only higher latitudes but lower latitudes due to a reduction of rain formation, showing that revised warm microphysics processes also play an important role to improve a global radiative energy budget. Impacts on cloud feedback processes will be also discussed.

Question 1: <Richard Forbes> Thank you Akira. What was the change you made to the single-moment scheme - was it just the suppression of collision growth or more than this? And was the collision growth you are referring to riming, i.e. - ice and/or snow with cloud liquid drops? I wouldn’t have thought this was too active right at cloud top?

Akira: Thank you for the question. Seiki and Roh (2020), the new one-moment scheme shown in my talk, changed a conversion process from cloud water to rain from the Berry (1968) scheme to the Khairoutdinov and Kogan (2000) scheme,which makes the longevity of cloud water much longer.

Also, they changed the diagnosis of number concentration of cloud ice, Ni, from Hong et al. (2004) scheme to the Phillips (2007) scheme. They found that the former overestimates Ni, leading to an excessive riming, and then acts to reduce supercooled liquid unrealistically.

They also introduced a fall of cloud ice, which helps separate partitioning of cloud ice under cloud water to suppress riming (cloud water to cloud ice), and maintains supercooled water.

According to their paper, vapor depositions to snow and graupel are turned off when their mixing ratios are small.

You can find more details in their paper:

https://journals.ametsoc.org/view/journals/atsc/77/11/JAS-D-19-0266.1.xml

-13-Cloud microphysical parameterization and Orography influence on extreme rainfall event of Kerala (2018)

Sandeep Pattnaik

The goal is evaluate the ability of these sophisticated CMPs in replicating the event. In addition, efforts are made to provide a new evidence of support from remote synoptic scale events facilitated by orography causing this extreme rainfall event over Kerala (2018). It is found that the choice of CMP has considerable impact on the rainfall forecast characteristics and associated processes. Horizontal moisture flux convergence (MFC) was the major (minor) driver of convection for higher (weaker) threshold of rainfall with WDM6 predicting the most consistent peaks of MFC in comparison with the TRMM rain rate peaks.Hydrometeor analysis suggests that the Milbrandt and Thompson Aerosol Aware schemes (WDM6 and WSM6) were unable (able) to capture the cloud ice realistically which led to large (less) error in the rainfall prediction.

Several results are shown with the updated set of parameterizations. Most striking is the improvement on the most important deficiency of the current Harmonie-Arome model, namely the underestimation of low stratus clouds and the overestimation of the cloud base height. Other improvements concern the triggering of rain and the representation of the stable boundary layer. As illustrated, most improvements can be related to a better preservation of atmospheric inversions.

Question 1 <Maxime Colin>: It is interesting that you use increase in RH as a surrogate for climate change, but a little bit counter-intuitive. From Romps 2014, I expect that RH will remain roughly constant with warming. Can you comment on this?

Question 2 <name>:

14- A cold pool perturbation scheme to improve convective initiation in convection-permitting models

Mirjam Hirt, George C. Craig

Cold pools are essential for organizing and initiating convection. In a recent investigation, we identified several sensitivities of cold pool driven convective initiation to model resolution within hectometer simulations. In particular, a causal graph analysis has revealed that the dominant impact of model resolution on convective initiation is a direct consequence of weak vertical velocities at the gust fronts, rather than being related to changes in the buoyancy or other properties of the cold pools.

To address this deficiency, we develop a parameterization for convection-permitting models to improve the representation of cold pool gust fronts. We enhance vertical wind tendencies within these gust fronts towards a target vertical velocity based on similarity theory. This parameterization strengthens gust front circulations and thereby enhances cold pool driven convective initiation. Consequently, precipitation is amplified and becomes more organized in the afternoon and evening.

Question 1 <Frédéric Hourdin>: When you say that cold pools are not well represented in too coarse LES, you mean when running at 600 rather than 150 m resolution ?

yes exactly, in our study we use resolutions from 150 to 600 m grid sizes.

Question 2 <Fleur Couvreux>: Nice talk. For what resolution do you promote the use of such scheme? I guess it assumes that the buoyancy of the cold pool should be well resolved in order to get the right w0. Thanks for your answer

Maybe I can add, that the scheme should adapt to different resolutions as well. We have not tested it yet, though.

Question 3 <Maxime Colin>: Sorry if I missed that, but what is the target w you are using for the nudging?

15- Recent developments in prognostic physics in ARPEGE-Climate: Turbulence in the presence of convection and discretization of convective vertical velocity in PCMT

Jean-François Guérémy

This talk presents 2 recent developments in prognostic physics included during 2018 in ARPEGE-Climat and implemented in 2019 in the coupled operational seasonal forecasting system of Météo-France. The first and most important one concerns the consideration of convective PCMT transport flows in turbulence; this is done by including thermal and dynamic convective productions, in addition to their pre-existing turbulent contributions, in the temporal evolution equation of the turbulence. The second development deals with a better discretization of the convective vertical velocity equation in PCMT. Results will be illustrated using 1D simulations on case studies and 3D global simulations in mean climate and seasonal forecast mode.

Question 1 <name>:
Question 2 <name>:

16- Persistence behaviour of heat and momentum fluxes in convective surface layer turbulence

Subharthi Chowdhuri1, Thara Prabhakaran1, Tirtha Banerjee2

The characterization of heat and momentum fluxes is of paramount importance for a plethora of applications, ranging from engineering to Earth sciences. Nevertheless, how the turbulent structures associated with velocity and temperature fluctuations interact to produce the emergent flux signatures, is not evident till date. We investigate this by studying the switching patterns of intermittently occurring turbulent fluctuations from one state to another, a phenomenon called persistence. We discover that the persistence patterns for heat and momentum fluxes are widely different. Moreover, we uncover power-law scaling and length scales of turbulent motions that cause this behavior. Furthermore, by separating the phases and amplitudes of flux events, we explain the origin and differences between heat and momentum transport in convective turbulence. Our findings provide new understanding on the connection between flow organization and flux transport, two cornerstones of turbulence research.

Subharthi The interested readers can see these two papers in **Physics of Fluids** for further discussion on the topic of persistence and its usage in turbulent flows.

https://aip.scitation.org/doi/full/10.1063/5.0013911
https://aip.scitation.org/doi/full/10.1063/5.0027168

For any questions, please feel free to contact me at subharthi.cat@tropmet.res.in or subharthi1987@gmail.com

Question 2 <name>:

INVITED TALK:

Using LES and observations to inform the representation of convective organization and memory in next-generation Earth System Models

Roel Neggers

The genesis and maintenance of spatial patterns in cumulus cloud populations have become intensely studied in recent years. This effort is motivated by the important role that organization plays in the coupling between convective clouds and the general circulation, in climate sensitivity, as well as in the grey zone problem in convective parameterization. Guided by this research, new conceptual modeling frameworks for capturing both organization and memory effects have been proposed, many of which take population dynamics into account. The nature of these new approaches creates unique data requirements for their calibration, training and evaluation. Where most previous model evaluation initiatives have focused on reproducing bulk statistical moments such as the mean and (co)variances, new schemes are required to also reproduce metrics reflecting spatial organization and convective memory. In this presentation we will briefly review the consequences of this development on how Large-Eddy Simulations (LES) and observations can be used for this purpose. As a practical example we will consider BiOMi (Binomials on Microgrids, https://doi.org/10.1029/2020MS002229), a newly formulated model to describe populations of interacting convective thermals as distributed over a two-dimensional Eulerian grid. Key elements include i) a fully discrete formulation based on a spatially-aware Bernoulli process, ii) object age-dependence for representing life-cycle effects, and iii) a prognostic number budget allowing for object interactions and movement. These features introduce convective memory and organization, but also optimize the computational efficiency of the framework. The BiOMi thermal population model is coupled to an EDMF parameterization and implemented in a primitive circulation model. First experiments for subtropical marine Trade wind conditions as observed during the RICO and EUREC4A field campaigns are discussed, including an evaluation against associated LES and observational datasets.

Question 1 <Martin Köhler>: Nice ideas! How does the interaction of objects work? Is it similar to the plume interacting kernel in ArakawaSchubert74? Are they interacting through the mean state?

Roel: Thanks Martin. The objects in the application I discussed represent sub-cloud scale thermals, so the interactions we include directly reflect their behavior. We know from obs and LES that thermals like to cluster and together form cumulus clouds, so that is the process we tried to capture. I think plumes in AS74 more represent classes of plumes of similar size, so that would be kind of different (but related). Thanks for bringing this up, it is good to interpret our model in the context of their groundbreaking work! When BiOMi is coupled to our multi-plume EDMF the comparison to the AS74 framework is more straightforward, as these are both spectral schemes. In that system the objects do interact through the mean state, through competition for available potential energy.

Question 2 <Fabian Senf>: Roel, nice talk! I did not really get how you describe and constrain interaction between convective cells? Proximity to neighboring cells? Oh, the same question as Martin had … One more: How do you constrain lifetime of cells?

Roel: Thanks Fabian! I had to stay a bit on the surface concerning the model description, given the time frame of the presentation, but the interaction between objects is fully described in the paper: https://doi.org/10.1029/2020MS002229. In our simple application to convective thermals both rules of interaction work through the probability field p with which objects are born on the microgrid. We take the objects present on the grid and perturb p in a cone around them, so that births are favored close to existing clusters. The 2nd rule scales this p field such that the average birth rate obeys the overall constraint as applicable to a large area. These two rules are ultra simple, and are just a first exploration of what this could give us in terms of clustering. The real science of calibrating the associated constants of proportionality against obs and LES only begins here...

Question 3 <Frédéric Hourdin>: Is your model propagating information on the populations from one horizontal grid cell of the large scale model to the neighbours ?
Roel: Not yet, but this could easily be done. That way cluster growth is in principle unlimited. However, exchange of data between grid cells of the large scale model is of course expensive. But maybe there are ways to deal with this.. it is certainly something I want to investigate further.

Question 4 <Fabian Jakub>: Which processes do you think make sense to take into account in the microgrid, e.g. surface exchange, radiation effects etc.
(* answered on the conclusion slide, Thanks.)
Roel: So far we only looked at cluster area distributions, in order to feed to our $ED(MF)^n$ convection scheme to replace the existing closure. This simply was our first goal. But in principle much more is possible. For example, there have been quite a few lattice model studies that have included the integrated water vapor field, given the important role it plays in cloud formation, precipitation and radiation. I particularly like the recent study by Ahmed and Neelin (2019) https://doi.org/10.1175/JAS-D-18-0368.1. Also coupling the object behavior to surface heterogeneity maps is possible, for example to capture intense triggering over certain areas such as elevated heat sources.. or capture the effect of cloud shading (3D of course!) on cumulus triggering.

Question 5 <Mirjana Sakradzija>: Nice talk, Roel! How is the model constrained? No bulk closure is needed, but I guess some parameters would still need some kind of a closure. How is the model coupled to the model dynamics? Thanks!
Roel: Thanks Mirjana! Good question. It is still very early days in the development, in fact I am already quite happy that the system seems to work in the first place. So there is a lot of work ahead to scientifically constrain the population model. To train BiOMi I am taking the first steps in using machine learning, based on many spatial fields of cloud populations. In this we can build on the intensive work that is going on in classification of cloud patterns, which can then act as labels in supervised learning. Concerning your question about closure, indeed $ED(MF)^n$ still has closures, that problem does not go away. But the level at which we do closure has moved compared to a bulk scheme, from say cloud base mass flux closure to the initialization of and mixing by size classes of plumes. So far, the BiOMi population model only delivers the (area-)weights to go into the mass flux formulation for each plume. This weight then influences the contribution of that bin to the total flux.

Question 6 <Hannah Christensen>: Do any of your probabilistic rules depend on the large scale state as opposed to the state on the microgrid? Do you need to introduce additional spatial correlations in the stochasticity of the scheme (thinking of the large-scale averaging used to condition the Plant-Craig approach)
Roel: In the application I discussed the object birth rate is dependent on the surface buoyancy flux, so that’s one way in which information of the large-scale state comes in. The work done by the EDMF plumes coupled to the population model also depends on the large scale state of course, in various ways: for example, their vertical extent depends on stability, proximity to saturation and

cloud formation, etc. Any feedback on the surface buoyancy flux then flows back into the object population model.

Question 7 <Zhihong Tan>: Great talk! If I understand correctly, do the BiOMi objects represent the boundary layer thermals that grow into individual cumuli? What does the parameterized interaction between these objects represent: do they represent the boundary layer processes, or also the interactions in the cumulus layer (especially for the deep convective regime)? Thanks!

Roel: Thanks Zhihong, indeed the convergence of thermals below cumulus clouds is what we had in mind, first focusing on the surface-driven convective boundary layer. So yes, the interactions do represent boundary layer processes. Any processes above, such as cumulus outflow or downdraft effects, are not considered yet in our simple model. I know Steef Boeing in his 2016 paper (https://doi.org/10.1515/mcwf-2016-0003) added a layer to introduce precipitation effects, and was successful in creating cold pool like structures this way. So I guess that could work.

Question 8 < Fleur Couvreux>: How do you specify birth rate and information on ages of your element of your population?

Good question, thanks Fleur. For these things we used the results of a few recent studies that looked into thermal behavior using LES or CRM. For example, Hernandez-Deckers and Sherwood (2016, http://doi.org/10.1175/JAS-D-15-0385.1) present some information about the typical lifetime of thermals in a convective layer. That study was a real inspiration for this work. For the birth rate we for now simply followed previous studies on how the number of coherent objects depends on surface instability; but I think we need to do dedicated LES studies to investigate this more deeply, because this very much depends on how the objects are defined. Counting and tracking newly emerged objects would be needed for this. This is something that could also be studied in a laboratory, I guess..

Question 9 <Maxime Colin>: Fascinating presentation! You had a simple equation to explain what the bulk approach means. How would you write one (or several) equation(s) to similarly capture the essence of the decentralised parameterizations?
Roel: Thanks Maxime, I am glad you enjoyed the talk! The essence of decentralized approaches is that there are multiple independent modules that can compete or collaborate. So I guess each module can/should have its own equation; maybe the same equation with an added dimension, such as size-space in case of a spectral approach. There will be terms that represent interaction between the equations; that would go towards a predator-prey ecosystem (see also your question below)

Question 10 <Maxime Colin>: It looks like our study on a predator-prey model of convection is related to your presentation framework to some extent. How did the stability issues appear (if any) in your population model? More generally, is there any structural assumption that explains why you said that the decentralised models are more subject to subtle responses to weak perturbations?
Roel: There is competition going on on the microgrid; this works through the rules of interaction. In effect big clusters can suppress smaller ones in their vicinity. This leads to clusters disappearing and merging, which introduces time fluctuations; you can see this in the evolution of the largest cluster size on the grid. It seems that, when coupling the microgrid to our EDMF framework, the system can deal with these fluctuations, which was a pleasant surprise. I still have to dig into why this is, and understand it better. Of course, these fluctuations become more pronounced when

selecting a smaller domain, which is purely due to subsampling the binomial pdf in the grey zone. So that is actually variability we want to keep. Philipp actually tested BiOMi also as a pure classic predator-prey model, including stochastic elements. You can see some results in the paper (https://doi.org/10.1029/2020MS002229).
Concerning the response to perturbations by (de)centralized approaches: because of the added degrees of freedom a decentralized approach has in principle more ways to adjust. However, that this will happen smoothly is of course not guaranteed; it very much depends on the nature of the system, and for example how the interactions function. I think one use of decentralized approaches is not just purely operational application in a weather or climate model, but also just to get insight into how these responses to (climate) perturbations might work.

Thank you all for your interest and the good questions asked!

Discussion:
Deadlock in parameterization: Will ML and high-resolution modelling solve this issue ?
What about Super-parameterization experiment : why not in global climate yet : cost, cyclic conditions, technical issue, interaction with other processes (chemistry, dust,..), scale separation...
Even at global km-resolution still need for parameterization (turbulence, microphysics, cold pools,…

Development of new scheme: Need time to be as good as current parameterization
But now we have :
- new tools to fasten the process
- multi cases with LES available
- computer power
- big data, ML (! Not staying to single column version, use it to learn about exchange between grids ? Memory?)

High-Tune:RenDeRer tutorial

Training material:
https://www.lmd.jussieu.fr/~nvillefranque/pub/formations/htrdr-2021.pdf