Geosci. Model Dev. Discuss.,
doi:10.5194/gmd-2016-88-RC2, 2016

Geoscientific Model Development Discussions

Interactive comment on "The Radiative Forcing Model Intercomparison Project (RFMIP): Experimental Protocol for CMIP6" *by* Robert Pincus et al.

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Received and published: 15 June 2016

Pincus, Forster and Stevens describe the concepts of the Radiative Forcing Model Intercomparison Project (RFMIP), intended as a contribution to the 6th Coupled Model Intercomparison Project (CMIP6). In my opinion, RFMIP is a key contribution to CMIP6 and an outstanding advance over the previous CMIP phases. It is of utmost value that for the historical period, but also for the future scenarios as well as for the idealised simulations (4xCO2) the effective radiative forcing (ERF) and its spatiotemporal distribution is quantified. This will help enormously to advance the interpretation of simulated responses, and of detecting and attributing climate change signals.

RFMIP proposes to do so in a series of three distinct sub-projects. The first one con-

sists of conducting simulations with prescribed, pre-industrial distributions of sea surface temperatures (SST) and sea ice cover (SIC) that allow to diagnose the ERF for different combinations of forcing agents at different times. The second is split into two and intends to compare model-simulated radiative transfer results to reference computations, on the one hand for greenhouse gases (CO2?), where pre-selected thermodynamic profiles are used, and on the other hand for aerosols, where global snapshots from the contributing general circulation models (GCMs) are used. Finally, a time-varying climatology of the anthropogenic perturbation to aerosol radiative properties and cloud droplet number concentrations is used to consistently impose historical aerosol ERFs to all participating GCMs.

The concept in general is excellent, and will help the science a lot.

My main remark is that at several instances, more details would be useful to exactly define the RFMIP in order to very precisely describe the set-up to model centres.

I have a few more overarching suggestions, and several minor ones.

General remarks
- The paper only occasionally indicates which model diagnostics (output) is to be produced and analysed. One could only assume that the different contributions of top-of-atmosphere (also surface?) radiation flux densities (solar and terrestrial, all-sky and clear-sky) are to be determined. What about cloud quantities?
- When it comes to the aerosol ERF it has been shown that it is useful to on-line diagnose components of the forcing. This in particular involves a triple-call to the radiation, allowing to diagnose the radiative forcing due to aerosol-radiation- and due to aerosol-cloud interactions (Ghan, Atmos Chem Phys 2013; doi:10.5194/acp-13-9971-2013). Would it not be useful to request such a diagnostics?
- For the transient ERF, more specifications are necessary how to compute it. Are multiple ensemble members necessary? Or are time slices computed? Or else is a noisy signal accepted? What about the strong deviation from the pre-industrial base state at

least in the future scenarios at least in the Arctic?
- For the IRF study, the paper should be structured more clearly to clarify the two aspects (CO2 and aerosols) to this, which use two very distinct approaches.
- for the imposed aerosol forcing, it would be good to indicate what the implied ERF due to aerosol-radiation-interactions and what the ERF due to aerosol-cloud-interactions are for e.g. year 2011.

Specific remarks

page 1
l14 relies
l19 the "roughly" might merit a sentence of explanation, or a reference.
L22 "is related" $\rightarrow$ maybe "is approximately linearly related"
is it worth mentioning that Eq. (1) actually defines the radiative forcing, and that the usefulness of F is linked to the degree to which α is independent on the exact nature of the process that generates F, so that comparing different F is sufficient to predict different ΔT for a given α (i.e. given model)?

Page 2
l17 "individual models" $\rightarrow$ "different models"?
L20: If sticking to the acronym "IRP" this should probably be spelled out "instantaneous radiative flux perturbation" (assuming the "R" represents "radiative")
l30: this sentence in my opinion is not understandable to somebody not experienced in the topic. The point is about efficacy, and so I'd propose to write "...suggests that IRP due to different forcing agents is not, in practice, a very good predictor for changes in surface temperature assuming constant climate feedback parameters, a point..."
l33: I'd suggest to be more specific: "...does not depend on global-mean surface temperature change (Sherwood et al., 2015)."

Page 3
l2 "accurate diagnosis", or rather diagnosis at all?

L5 Reference Hansen et al. (2005): I think it would be appropriate to also cite Rotstayn and Penner (J Climate 2012, 14, 2960-2975) who introduced the concept several years before Hansen
l6 "fullness of the model response" to be precise, perhaps "fullness of the rapid model response"

page 4
l2 "concentration or emission changes", since at least some models computed carbon- and aerosol cycles interactively
l15 One would expect a reference corroborating the sentence, rather than – once more – introducing the regression concept (Gregory).
L19 The sentence is not straightforward to understand. For both methods, only a given perturbation can be diagnosed, and this can be done for either approach. L22 It would be useful to specify as clearly as possible the simulation. I'd suggest to clarify the following things: (i) it is one annual cycle consisting of 12 geographical distributions of sea surface temperatures and sea ice fractional coverage; (ii) from how many years should the fields be derived? Average over any 30 years of the piControl run? (iii) should the monthly means be linearly interpolated to the individual time step, or abruptly change on the first of each month, 0 UTC?
L26 "without compromising accuracy" - I don't understand what is meant in this context.
L27 "present-day" needs clarification. Is this year 2011 as in CMIP5, or 2005 as in CMIP3? It would also be good this time to have the date consistent with the end of the DECK historical simulation.
The Tier-2 experiments need to be motivated. Is a signal from 0.1 Aer really detectable over the noise in a 30-year fixed-SST framework? Why a factor of 0.1 and not a factor of 0.5? Or why not – probably even more useful in interpreting the historical simulations – a year 1985 simulation?

Page 5

l2 Forster et al. (the cited JGR paper) say 30 years of integration time are necessary to characterize ERF. How is this done here? By 30 ensemble members and they reporting the forcing every year of the simulation? Or is a much greater transient uncertainty simply accepted if a single ensemble is run?

It seems Forster et al. (JGR) claim the base state does not matter much so a pre-industrial SST and SIC distribution is good enough. But is this not a main uncertainty when integrating into the future? How meaningful is a forcing diagnostics in the second half of the 21st century in the Arctic when pre-industrial sea ice cover is prescribed?

Page 6
l14: "both greenhouse gases and aerosols": It is quite unclear from Table 3 how the greenhouse gases beyond CO2, and aerosols are to be prescribed, in both the control and perturbed simulations. Only at some point in the text, it appears the atmospheric profiles should be not only cloud-free, but also aerosol-free. I didn't find any information about greenhouse gases beyond CO2.

Page 7
l1 Why not provide the selected profiles as supplementary material? It would be useful to specify in the paper the exact way the profiles are introduced. Is the vertical discretisation the model's one? Or is a common vertical grid chosen?
L4 "aerosol-free": What about greenhouse gases?
L8 "when finalized" why not put this up for review here as well?
L12: It would be good to make clear at the beginning of section 3 that there are two distinct and very different approaches to characterise greenhouse gas- and aerosol forcing. It would be also useful to split these two into to sub-sections, one on greenhouse gases, and one on aerosols.
L14: "radiative perturbations": does this imply a double radiation call, one with the current aerosol and a second one with all aerosol set to zero? The exact definition should be clarified, and also the necessary output. Is this requested for the top of the

atmosphere, or vertically resolved? Broadband in the solar spectrum, or spectrally resolved? Why not both, the clear-sky and the clean-sky flux?
L15: "surface albedo, aerosol" (with ",")
l17: It would be good to be exhaustive in the request list, i.e. to also include which other parameters (temperature, pressure, specific humidity? gases?) to provide.
L24: Maybe would it be possible to write why multiple reference models are advantageous? Is it expected that their results differ?

Page 8
l6 Another substantial contributor is the diversity in simulated cloud distributions (Penner et al., Atmos Chem Phys 2006 doi:10.5194/acp-6-3391-2006; Stier et al., Atmos Chem Phys 2013, doi:10.5194/acp-13-3245-2013)
l9: 20th century: rather a balance of sulfate and greenhouse gases.
L12: To be more precise: the continents adjacent to the North Atlantic
l17: "commensurately large", or why "larger" (than the emission changes?)
l26: "reducing temperature" may be misunderstood (the GHG effect mostly is dominant), why not – pertinent to the paper – say that they introduce a negative effective radiative forcing?

Page 9
l2 "perturbation" singular
l17 It would be useful to indicate the ERF of the MACv2-SP aerosol perturbation e.g. for year 2011.
l23 Why citing Eyring here?
L23: The sentence in brackets is difficult to understand and should be explained better.

Page 10
l33: But does the MACv2-SP provide a forcing more negative than -1 Wm-2 in 2011?

Page 11
l10 "addresses" (present tense)

l21: “quite useful” for what?

Page 12
Table 1
“Present-day” is not specified. Is this intended?

CMIP6 label/experiment id: can the dashes be omitted? I think having just words helps in some scripts, and this was common practice in CMIP5.

RFMIP-ERF-GHG Experiment description: It should be made very clear whether ozone (tropospheric? Stratospheric?) is considered a (greenhouse) “gas”. Can one not be specific by writing “CO2, CH4, N2O, Halocarbons, and O3 and CO precursor gases?

RFMIP-ERF-AerO3: Experiment description: How is O3 perturbed for models that include atmospheric chemistry, i.e. that require emission, rather than concentration, perturbations? Is this tropospheric ozone only?

RFMIP-ERF-LU Experiment description: gases or land-use?

RFMIP-ERF-4xCO2: Does this require another control simulation for models with an interactive carbon cycle (namely a simulation with pre-industrial CO2 concentrations, rather than emissions)?

RFMIP-ERF-AerO3x01: It should be clarified that the anthropogenic O3 and aerosol concentrations, or ozone precursor and aerosol (precursor) emissions should be scaled by a factor of 0.1. (same for RFMIP-ERF-AerO3x2)

Page 13
Table 2
RFMIP-ERF-HistNat Experiment description: I think “etc.” is very bad to use in such a protocol. It is necessary to very precisely say what should be varied. What could and should be thought of besides solar activity and volcanic eruptions?

RFMIP-ERF-HistAer Experiment description: What about ozone?

Table 3. “in forcing for .” for what?

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