

Interactive comment on “Detection and Attribution Model Intercomparison Project (DAMIP)” by Nathan P. Gillett et al.

Anonymous Referee #2

Received and published: 13 May 2016

Summary

The authors describe the Detection and attribution model intercomparison project (DAMIP) that is planned to contribute to the Climate Model Intercomparison Project phase 6 (CMIP6). The overall aim of DAMIP is explained as a framework for climate research institutions to use to produce a variety of climate simulations of the past 100 or so years, following a consistent experimental design. The authors propose that the experiments will aid in the understanding and attribution of observed past changes, and help in the constraining of projections of future climate change. A number of different experiments are devised, with different priorities, which build on those produced for CMIP5.

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This plan is very much welcome. Having a clear design will hopefully build on the qualified success of CMIP5. What is proposed is very clearly presented.

The manuscript is generally clearly written, with only a few typographic corrections needed - see technical comments below.

I recommend acceptance, after some relatively minor revisions - clarifications about the design and motivation for the choice of experiments.

I do have concerns about some of the actual proposed experiments, but I suspect the experimental design is set in stone, and the reviewer opinions and the other open comments may not be able to influence what experiments are included in the plan. However, I strongly urge the authors to consider the below comments about the specifics of the design and consider if tweaks can be made to improve the efficacy of what is planned and increase institutional engagement.

General comments

I fear, with respect to resources required, that the proposal is somewhat over ambitious. Just for Tier 1 the number of experiments and initial condition ensembles required, >1870 model years, may substantially limit the number of models participating in DAMIP. ScenarioMIP (O'Neill GMDD [2016]) say "the success of ScenarioMIP lies in the broad participation of the CMIP6 modelling groups in Tier 1 experiments...". Should the DAMIP plan have the same ambition? A much smaller population of experiments/ensembles for a 'Tier 0', to focus on a few important scientific questions, could encourage as wide a range of models to take part in DAMIP as possible. The remaining tiers can then be populated by institutions with more resources. It would be a shame

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to miss the opportunity to design experiments that would encourage greater institution involvement than there was for CMIP5 detection analyses. A 'lessons learned' exercise and finding out why some institutions didn't produce 'detection' experiments for CMIP5 might have been helpful.

There are several experiments that were not included in the original DAMIP proposal circulated within the CMIP community (http://wcrp-climate.org/images/modelling/WGCM/CMIP/ApplicationSummary_CMIP6-EndorsedMIPs_150408_Sent.pdf) i.e., tier 3 experiments - histCO2, histSOL and ssp245NAT. The inclusion of these experiments were also not discussed with other scientists at the IDAG (International Detection and Attribution Group) meeting held in February this year. The motivation for including these experiments should have wider community discussions, as it is not really clear how useful they are [See below specific comments about those experiments]. They may be done at the expense of more useful experiments.

More is needed to be said about the general type of analyses expected. In particular what is required for analyses using multi-model mean is not the same as what is needed for analyses on individual models. The ensemble size, especially for forcing factors with relatively weak response patterns, is much more important for analyses on individual models. As mentioned below, the recommendation of at least 3 ensemble members for histNAT, histSOL and histVOL etc. is not ideal for many types of analyses on individual models. However it may be more than sufficient for a multi-model mean analysis, where the total number of ensemble members will be much larger (e.g., Gillett et al, Journal of Climate [2013] and Jones et al, Journal of Geophysical Research [2013] both included both types of analysis using CMIP5 models).

Specific comments

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Page 3 Line 10 and elsewhere. As explained in the manuscript, specific analysis plans are not given, but it would be helpful to indicate how the contributions to observed climate changes from well mixed greenhouse gases and also aerosols can be extracted with the proposed tier 1 experiments. It is mistakenly stated that previous studies separated contributions from WMGHGs and aerosols (Page 3 lines 12-13 and elsewhere). Those studies actually separated contributions from WMGHGS and other (non WMGHG) anthropogenic factors, as well as natural influences. It may seem pedantic, but in fact the contributions from ozone and land use changes are potentially substantial important for some diagnostics and should not be excluded if possible.

Page 3 Lines 15-17. I think the wrong Ribes paper has been referenced here in two places. Ribes and Terray, Clim. Dynam. [2013] is much more appropriate, as it has a detection study on observations and examines differences when using different models. Ribes et al. [2015] does neither of those things.

Page 3 lines 20-23. If ozone and land use are not included with aerosols, what should they be included with? Are the authors proposing ignoring these forcing factors?

Page 4 line 13 and elsewhere. The use of 'early 21st century slowdown' should be discouraged. Like other terms attempting to describe this period, e.g., 'hiatus', it is ill defined at best and inaccurate and misleading at worst. The scientific literature has tied itself up in knots about misleading names for this period. The authors should be really clear about what they mean.

Page 5 Lines 26-29 This is an excellent recommendation. It was generally difficult to deduce what models did follow after 2005 for the GHG experiments. Proposing a specific SSP will help to maintain consistency in the future simulations.

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Page 6 Lines 1-5 Presumably (as is stated later) the natural radiative forcings for the 2015-2020 period will follow those as planned to be used by ScenarioMIP. It should be mentioned that future volcanic forcing is planned to be "ramped up" [O'Neill et al GMD 2016] between 2015 and 2025 to control levels and that future solar irradiance will contain a repeat of past solar irradiance variations. It might actually make sense to push back the ramping up (or down) of the volcanic aerosol until after 2020, to avoid introducing an artificial signal into the models at the end of the historical period. Something to liaise with O'Neill et al [2016] about?

Page 6 Lines 12-13 Is it wise to put simulations covering the period 2015-2020 in CMIP6 with the label 'SSP2-4.5'? That is inconsistent with what is required by ScenarioMIP for that label, i.e., they expect simulations to cover the 2015-2100 period. This could cause confusion.

Page 6 lines 14-17 There may be some diagnostics or filtering combinations it will be ok, but for characterising multi-decadal near surface temperature patterns of change in individual models, 3 ensemble is likely to be insufficient, e.g., Ribes et al [2015].

Page 6 Lines 21-25 It is excellent to see this recommendation. It is also worth giving a recommendation for land use changes. For instance at least one CMIP5 model (IPSL-CM5A-LR) may have included historic land use change in their historicalGHG experiment.

Page 8 lines 10 onwards It is not clear how useful a small ensemble of histSOL will be for many detection studies. The authors are rather over confident when they say (line 24) that "unambiguous characterization of each model's solar signal" can be deduced and "separate clearly solar and volcanic effects". Having only three initial condition en-

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semble members will make this very difficult for many diagnostics and filtering choices (Ribes [2015]). If analysts are interested in impacts of just the solar cycle, then better experiments could be designed. The ssp245Nat for instance may provide the required data for those needs.

Page 8 lines 27 It is rather over confident to state "will allow the characterisation of and attribution to volcanic influences." Three initial condition ensemble members are likely to be insufficient for multi decadal near surface temperature analyses on individual models (Ribes [2015]).

Page 8 Line 32 onwards the usefulness of an ensemble of histCO2 is not at all obvious and I wonder if it should have been more widely discussed within the detection and modelling communities before being proposed. The authors main reason for inclusion of this experiment seems to be helping to constrain TCRE (Eq 2 in Gillett [2013]). The TCRE (temperature change relative to cumulative emission of CO2) should be constant for a given model. So shouldn't the information provided by the 1% CO2 experiment be sufficient? If $\Delta T(1\% \text{ run @ } 2x)/E(2x)$ doesn't equal $\Delta T(\text{histCO2 @ } 2010)/E(2010)$ then the usefulness of TCRE is itself questionable. Thus eqn 2 in Gillett [2013] should just be $\text{TCRE} = \beta * \text{TCR} / E(2x)$ The authors also give another reason - that it would help to better characterise the uncertainties in TCR. I am not sure this makes sense. histCO2 would help in understanding uncertainties in the predictive power of TCR estimating GHG warming, not the other way round.

Page 9 lines 9-22 Depending on whether analyses using ssp245AER and ssp245NAT are using multi-model means or individual models, just asking for one ensemble member (Table 1) may not be sufficient to characterizes responses accurately.

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Page 9 lines 31-34 It should be mentioned that ozone and land use forcing factors will be folded up in the examples given, contributing to the "aerosol forcing uncertainty" and "natural forcing uncertainty".

Page 10 - section 3 Are there any MIPs that it would be vital to have involvement with? It is highlighted that RFMIP is close to DAMIP, but I wonder if it should be considered a vital MIP that should be done with the same model as used for DAMIP. There is a danger that some institutions will use one version of their model for some MIPs and other versions of their model for other MIPs.

Technical comments

Page 8 lines 12-13 The past solar cycle in solar irradiance has had cycle lengths of between 9 and 13 years.

Page 13 Line 9 Gregory et al should be on a new line.

line 14 page 15 Large font size

line 21 page 14 Submitted to what? I guess GMD?

line 32 page 15 Submitted to what?

Interactive comment on Geosci. Model Dev. Discuss., doi:10.5194/gmd-2016-74, 2016.