

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

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We thank the reviewer for his helpful comments. We have revised the manuscript accounting for these comments.

**1. About event attribution.** The paper provides a detailed enumeration of scientific questions for which DA experiments have been used in the past. Noticeably, the attribution of single weather / climate events has not been mentioned. I don't know if this was intentional or not. This area of event attribution has received much attention recently - maybe even excessively, I agree. What I would call the "dominant" method used to perform event attribution calculations involves large ensembles of forced atmospheric experiments - which are not DAMIP style simulations. But the counter-factual (ie NAT-only) SST are usually constructed using

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a common DA analysis of long-term changes, in an ANT vs NAT decomposition. DAMIP like experiments are required here. Additionally, there have been efforts to assess how the final results (eg FARs) depend on the assumed (ANT and NAT) response patterns, which basically requires a MIP with histNAT simulations from different models. Overall, I think this is an important application, which requires DAMIP, and which could be mentioned.

A link to the C20C+ Detection and attribution project was already mentioned in the manuscript. This link is now be described in more detail:

'This effort will be further facilitated by using output from DAMIP simulations as input to the C20C+ Detection and Attribution Project (Stone and Pall, 2016) and other similar projects, which use ensembles of simulations of atmosphere-only models driven using observed sea surface temperatures and sea ice, and other similar experiments in which attributable anthropogenic changes are removed from the prescribed SSTs and sea ice to quantify the contribution of anthropogenic changes to individual extreme events. Such studies rely on historical simulations and historical simulations with natural forcings only, such as those included in DAMIP.'

**2. Comprehensive list of external forcings and how they are clustered into subsets.** The paper provides a very clear list of experiments, and in most cases, a clear list of forcing agents to be considered in these experiments. However, I suggest it might be useful to provide an "as exhaustive as possible" list of external forcings, and the individual forcing experiments in which they are supposed to be included (eg which are classed as "Aerosols", etc). I think this might be useful for several reasons: - there might be some inconsistencies with AR5, e.g., with respect to what is called aerosols. According to, e.g., the AR5 Fig 8.17, aerosols do not include NOx or NMVOC, while these species are included in the histAER experiment that is proposed here. The status of tropospheric Ozone with respect to GHGs was also somewhat unclear in CMIP5. -some external forcings are not included in any DAMIP individual forcing simulations. This

applies to LU, which is being treated in LUMIP, but also to Tropospheric Ozone, and maybe other short lived gases like CO. An exhaustive list could make this clearer. - NetCDF files from CMIP5 were usually specifying a list of forcing agents explicitly (at least for GHG species, in my memory), so this work would have to be done at some point anyway.

A comprehensive list of forcings is now included in Table 1, as requested. The forcing agents to include in the HistAER simulations have now been clarified. NOx and NMVOCs have been included because they will affect the concentrations of nitrate aerosol and organic aerosol in models which resolve the relevant processes in their aerosol schemes. The description of histGHG has been revised to make even more clear that ozone is held fixed in these simulations: 'This will ensure that tropospheric and stratospheric ozone are held fixed in all these simulations, and simulated responses in models with and without coupled chemistry are comparable.'

**Other minor comments \* p3 l15 and l17: I suspect that the appropriate reference is Ribes and Terray 2013, instead of Ribes et al. 2015.**

Thank you. These references have been replaced with references to Ribes and Terray (2013).

**\* p3 l21-22 "with those of ozone and land use changes": I suggest replacing by "with the response to other external forcings, most notably ozone and land use" or something of that effect, as I'm not sure that all forcings can be put into the categories GHG, AER, NAT, OZ and LU (see also comment 2).**

Suggested change made.

**\* p4 l2: "the other" is written twice**

One instance of 'the other' deleted.

**\* p5 1st paragraph: I suggest adding somewhere something like "The two approaches ["only" vs "all-but"] are equivalent if additivity holds, but might differ**

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otherwise."

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Suggested phrase inserted, but with the additional proviso that the equivalence only holds in the limit of large ensembles. With small ensembles the noise in the response pattern will depend on whether it is simulated directly, or is calculated from a residual. Inserted phrase: 'The two approaches yield equivalent results if additivity holds and in the limit of large ensembles, but can differ otherwise.'

Interactive comment

**\* p5 Sentence I5-7: it is not clear to me that the "all-but" approach is more appropriate than the "only" one for the second question mentioned... Is it what the authors want to say? As an alternative, causality theory wight be mentioned explicitly, as it seems to be the main motivation leaning towards an "all-but" approach.**

The latter part of the phrase has been revised to read: 'or to detect the contribution of a particular forcing to observed climate change.' In this case the "all-but" formulation is clearly preferable, since the assumption of additivity is not required. We consider that a discussion on causality theory would be beyond the scope of this manuscript.

**\* p5 I14: "linear additivity" has been indeed commonly discussed in the literature but it seems to me that, strictly speaking, only "additivity" is assumed in the experimental design. "Linearity", in my view, is more related to the use of analysis techniques based on linear regression, like optimal fingerprinting.**

'linear additivity' replaced with 'additivity'.

**\* p5 I21-22: Does this also apply to GHGs other than CO2?**

Text amended to clarify this point: 'All simulations used in DAMIP are driven by CO2 concentration rather than CO2 emissions. In common with all other CMIP6 simulations, concentrations of the other well-mixed greenhouse gases (WMGHGs) are also specified in DAMIP simulations.'

**\* p6 histALL: This is probably well specified in other CMIP6 documents, but I**

think it might be useful to add a quick description of the NAT forcing recommended in SSP. To my knowledge, there were no clear recommendations in CMIP5 regarding the volcanic forcing. Additionally, at least one modelling center decided to run historicalExt experiments with no volcanoes (consistent with the observation of no major eruption when the run was realised in 2011/12), while the RCPs were run with a volcanic background. This led to historicalExt runs which differ from the corresponding RCP runs over their common period. My understanding of the description given here is that such a discrepancy should be avoided in CMIP6, and I think it would be useful to state this even more clearly.

The natural forcings to be used in the 2015-2020 period have now been clarified: 'Time-evolving solar forcing, and stratospheric aerosol ramping up towards the piControl background level should be prescribed over the 2015-2020 period as prescribed by ScenarioMIP (O'Neill et al., 2016).', and a reference to O'Neill et al. (2016) has been added to the references list.

**\* p6 histGHG: To make the point even clearer, I suggest adding a sentence such as "Ozone (tropospheric and stratospheric) is excluded from GHG species [and is therefore supposed to remain roughly constant in these experiments]", eg at I21.**

The description already had a sentence noting that ozone is held fixed in these experiments. This point has now been made even more clearly by amending this sentence to read: 'This will ensure that tropospheric and stratospheric ozone are held fixed in all these simulations'.

**\* p7 I17: maybe add "(see Tier 3)" after ssp245NAT**

'(see Section 2.3)' inserted to refer the reader to the description of the Tier 3 experiments.

**\* Tiers 2 and 3: information on minimum ensemble sizes seems less precise for**

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those Tiers if compared to Tier 1 - maybe it might be said somewhere that the general rule is at least 3 for historical, at least (only) 1 for SSP... Or maybe I missed it?

In the introductory paragraphs of sections 2.2 and 2.3 we have inserted 'Minimum ensemble sizes are three for the historical simulations and one for the future simulations.' Note that this information was already included in Table 1, but we have now included it in the text for completeness.

\* Lastly, I also suggest adding somewhere something like "Tropospheric ozone (and possibly other external forcings, if any) is not considered in any of the historical experiments driven by subsets of forcings which are proposed in DAMIP.". Consequently, quantifying the response to this forcing would require subtraction, with a possible confounding effect related to non-additive responses to other forcings. Note that this choice doesn't seem inappropriate to me, but just it would be useful to make it clear in order to prevent misinterpretation.

We have added the following sentence to the description of histSOZ: 'Note that DAMIP does not include simulations isolating the effects of tropospheric ozone changes.'

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