EGUSPHERE-2024-453
Combined responses to the Reviews

Review #1 of the paper "Bringing it all together: Science and modeling priorities to support international climate policy", by Jones C.G. et al.

Overview

The paper considers international climate policy needs for science and modeling out to 2030, and lays out priorities across seven areas. These range from modeling coordination in support of the assessment reports (AR) of the Intergovernmental Panel for Climate Change (IPCC), to underpinning science foci, and also the required technological infrastructure. The paper is penned by a large team of European authors involved in IPCC, the Coupled Model Intercomparison Project (CMIP) and more generally the climate science and modeling enterprise.

The focus of the paper is important. As climate change aggravates, and the stakes for climate science and policy get higher, it is critical to have clear climate science and modeling priorities, and community coordination around those, to rapidly accelerate progress. The paper is also timely, as CMIP7 and IPCC AR7 are getting underway. The commentary adds the viewpoint of a segment of the European modeling community, to a number of manuscripts on modeling strategy that have recently been published or circulated across the international modeling community.

The priorities outlined in the paper are a reasonable evolution of what's already at play, and aim at addressing some current gaps. Overall, this Reviewer agrees with the points made in this article.

A number of specific major comments/recommendations are listed below, along with some minor points.

Overall the paper is well-written and a useful contribution to on-going community discussions around the future of climate modeling and underpinning science.

We thank the reviewer for their insightful comments and recommendations, the majority of which we agree with. Below we respond to each point raised and outline where in the revised paper we have added or modified text to address the specific concerns. The reviewer comments are in black text and our responses in blue. The revised paper is also included in this reply so the reviewers can see how our responses to their comments fit into the overall paper.

Major Points

1.1 Line 180. The type of infrastructure outlined here still reflects a linear model from modeling to services, whereby the modeling community decides what simulations to run and shares those with users. Given the rapidly evolving climate policy questions, we should prioritize the development of an infrastructure that supports co-production of information based on climate models (i.e., experiments that are responsive to the evolving needs); that supports ML/AI exploitation of both modeling data and observations to address service needs in a flexible manner.

We agree there is a need to enhance the co-production of information based on climate and Earth system models and ease the exploitation of model data. We have updated some of our text to better emphasize this. Concerning design of model experiments themselves, this is addressed at the international level in the preparation of CMIP7 etc. Our paper proposes to better integrate the chain from IAM to climate/ES and impact models. This will facilitate improved links with users of this data by generating a more internally consistent set of simulation data across the models and modelling communities involved, achieving this in a more rapid and efficient manner. Concerning model data, more flexibility and easier exploitation is certainly required, in particular the ability to support new ML/AI applications. Better integration with the impact community, as proposed, will also help better address user needs. However, a full co-production with users of the entire experimental chain is beyond what can be addressed by the infrastructure for CMIP, CORDEX and ISIMIP, and will require significant international investment and coordination to move certain parts of the IAM-CMIP-CORDEX-impact modelling chain to a more operational setting. We briefly discuss the benefits (and challenges) in moving in this direction in lines 290 to 320.

New text to address the points related to co-development and infrastructure flexibility have been added at lines 182 to 187 and in section 8.

1.2 Lines 240-280. Indeed, the lack of consistency and disconnects in modeling across IPCC WG1-WG2-WG3, and the relevant modeling frameworks (CMIP/CORDEX/IAMS, etc.) are major gaps that need to be addressed. The authors do a great job of explaining current shortcomings and what could be done to address them. In the recommendations (lines 270-280), it seems important to emphasize that: 1) continuing CMIP experiments is critical to the continued improvement of models and scientific understanding; 2) a common framework of protocols, forcings, evaluation metrics, etc. is necessary across the various modeling communities to address the disconnects (e.g., between CMIP and CORDEX); common workflows are necessary but not sufficient; 3) the recommended service oriented/quasi-operational activity and CMIP/CORDEX science activities should be well-connected, i.e. the service activity (from global to local scales) should be a purposeful spin-off of the CMIP, and service needs should be driving CMIP science.

Agreed and we have endeavoured to stress the importance of these suggestions with new text at lines 290 to 320 and lines 601 to 604.

1.3 Lines 285-300. This is a great set of questions to illustrate the climate/Earth system modeling needs to inform climate mitigation. It is increasingly clear that changes in aerosols are a critical factor in the Earth's energy budget and that future mitigation pathways need to consider aerosol/air quality policies. Hence, I recommend explicitly mentioning a question about understanding the interplay of GHGs and aerosols in determining future climate mitigation pathways.

Completely agree. Thanks for pointing this out. A major omission on our part. Some new text added to address this at Lines 361-362 and 734 to 746

1.4 Lines 305-315. In addition to carbon interventions through AFOLU, there are many other types of carbon dioxide removal (CDR) methodologies that are being proposed, including enhancing the ocean carbon uptake (mCDR). Hence, ESM should also capture relevant ocean carbon cycle processes not just land CDR processes. I recommend the discussion be amended in this regard.

Agreed and we have added some text at lines 384 to 393 to address this.

1.5 Lines 350-370. This is a great list of Earth system interactions to be examined. I would add "humans-climate/ES interactions" to this list, as humans are the current major driver of change at this point. Given the discussions regarding intentional climate interventions, as we learn about how air quality policies are affecting climate, as we are looking to diverse solutions

to the climate, biodiversity and socio-economic crises, it becomes increasingly important to factor in humans in ways that are more advanced than what we have thus far. I would recommend the authors make an additional effort in this regard across the paper.

We agree that more emphasis needs to be placed on human – Earth system interactions. Particularly with respect to unintended consequences arising from human actions designed to address climate or air quality mitigation and/or regional to national scale adaptation. We have added some text to address this at lines 436 to 440 and lines 461 to 464.

1.6 Lines 375-390. The focus of this section on improving regional climate information is appreciated, however the discussion could be improved in several respects. 1) Global variable resolution models and two-way nested global models, both achieving resolutions comparable to regional climate models, are now a reality. These should be mentioned along side more traditional regional climate models. 2) The paper mentions models "..all running in a tightly linked framework..". Indeed, this simulation workflow is needed. What's also needed but not mentioned, is a common, model-agnostic, evaluation framework, with metrics and standards applicable across various modeling methodologies; this is increasingly important as the types of modeling methodologies diversify, e.g. with the advent of AI-based models. 3) It is striking that ML/AI is not mentioned in this discussion. It is certainly a promising tool e.g., to get to higher resolution information from lower resolution models, again duly vetted as any other modeling tool.

Thanks for underlining those aspects. We now explain in the paper that the term "regional downscaling" is intended to cover all physics-based dynamical models that aim to represent at fine-scale the climate of a specific region of the world, whatever the technical choice. This includes limited-area models (LAM), variable-resolution GCMs (VRGCM) or two-way coupled systems and possibly very-high-resolution (mainly atmosphere-land) GCMs if they target the study of the regional to local climates. Our text is not meant to be specific to the LAM approach. We would like to stress that VRGCMs have contributed to CORDEX since its inception in 2009. We have added some text to clarify this in lines 472 to 482.

Concerning point 2) we agree on the need for a common and method-agnostic evaluation framework across global models and all forms of regional downscaling. We have added some text (lines 489 to 493) to address this point.

Concerning point 3), we now make it clear that ML-based techniques we view as an important and growing component of the umbrella term statistical downscaling (lines 480 to 482) and agree that ML-based approaches need to be carefully evaluated along with, and against, more traditional statistical and dynamical downscaling approaches (lines 489 to 493)

1.7 Line 450. This is a good description of the opportunity provided by CMIP7 to explore using higher resolution in a balanced way, considering also other important lines of research. What I see missing, is an explicit discussion of the value of model diversity, as we try to gauge uncertainty. If we had a perfect way to model climate processes, we could forgo that. But there are still significant uncertainties and errors and so model diversity continues to be crucial.

We agree that emphasizing the continuing need for model and parameterization diversity is important and have modified our text to address this; lines 565 to 571

1.8 Lines 470-480. I generally agree with the recommendation of creating a tighter linkage between the global climate modeling (CMIP) and regional modeling (CORDEX). However, we now have global models that can get to convective-scale resolutions at the regional level, via 2-way nesting and variable resolution. These types of models would inherently bring more consistency across spatial scales and could naturally allow for a convergence of the global and regional modeling communities. Some additional discussion of these diverse opportunities is necessary here.

As stated in our response to comment 1.6, we have now made it clear that “regional downscaling” as used in our paper, refers to the full range of approaches used to generate high-resolution realizations of climate over a specific region. We agree that variable resolution GCMs and (potentially) atmosphere-land only global models running at km-scale, for timeslice experiments, may well become a reality in the coming decade. We have added some text to cover this at lines 472 to 478.

1.9 Lines 520-535 A few considerations: 1) “Digital Twins of the Earth” (DTE) are, thus far, primarily focused on atmospheric dynamics rather than Earth system modeling; 2) The value of DTE for climate modeling will need to be evaluated with the same frameworks/metrics that we use for any new modeling tools; this value remains to be proven; 3) DTE for climate will need to explore uncertainties (modeling/data choices, internal variability, forcings, etc.); this is a “must” for any application to climate risk evaluation, as discussed in section 7; 4) it is not unique to DTE to attain km-scale resolution and be responsive to user needs. Global-convection resolving models and variable high-res climate models are a reality. The discussion of DTE should touch on all these points, to illustrate how DTE is an interesting concept but its application to climate modeling is quite aspirational at this point.

Thanks for making these completely fair points. We have added some text in lines 684 to 693 to highlight these points.

1.10 Lines 750-795. This is a very well-written description of the status of the modeling infrastructure and the gaps that need to be addressed. One point that seems worth emphasizing: we need to evolve to an infrastructure that allows greater co-production of information between the modeling centers and the users, and greater flexibility.

We agree and have modified the text to emphasize this point; see lines 182 to 187 and in section 8.

1.11 Section 9. A few things seem worth addressing here and throughout the paper: 1) Climate policy requires that we project out at least 100 years from the present time. It is critically important that CMIP7/AR7 cover at least the period until 2130. 2) Aerosols should be mentioned explicitly as something that we need to study in conjunction with changes in the carbon budget, as we examine future climate mitigation pathways. 3) It is notable that the paper does not touch on modeling of solar radiation modification. As these types of intervention/geoengineering are being proposed, it seems important to document potential impacts and uncertainties. 4) The paper does a good job of discussing the importance of quantifying uncertainty and how it flows across modeling systems. However, the importance of understanding predictability (what we think we can predict and why) is not explicitly addressed. 5) Lastly, the recommendation to be more inclusive of global South scientists is meritorious and could be even more convincing if the paper were to include views from co-authors from that region.

Points 1, 2 and 3 Agreed and we have modified or introduced new text in lines 1001 to 1031.

Point 4: We have mentioned this in lines 1055 to 1057. We wish to point out that we did not intend covering the topic of seasonal to decadal climate prediction. Rather this paper is mainly concerned with (uninitialized) projections. This is why we have tried to carefully use the word “projection” rather than “prediction” throughout. Although, we agree that understanding what is potentially predictable or not is of great importance.

Point 5: We fully agree, and you are right that we should have made a greater effort to engage with Global South scientists at the outset of this paper. The co-authors of this paper, and the recommendations therein, to an extent are the result of discussions we have had in a number of EU-funded research projects over the past ~5 years, and therefore primarily reflect our collective viewpoint. Within that collective opinion, there is a strong desire for our (European

and national) funding agencies and governments to recognise the need (and benefit) of funding Global South climate research. A number of co-authors have worked quite extensively with Global South scientists in international activities like; CMIP, CORDEX, ISIMIP, AgMIP, VIACS over the past 2 or more decades, and therefore have some insight into what might help. That said, you are completely right that we cannot (and should not) speak for Global South scientists. Rather we should do what we can to ensure their voices can be fully heard at relevant international fora. Our hope is that this piece might stimulate European funding agencies to ensure they provide funding and the practical means for Global South scientists to engage with European scientists on long-term projects, ideally structured around international efforts, such as those being pushed by WCRP. Examples would include CMIP, CORDEX, ISIMIP and AgMIP. It is probably a bit late in the day for us to invite Global South scientists to be co-authors on this paper. We have therefore made it clear the views expressed are opinions of this group of European researchers. We hope this paper may allow us to engage more strongly with Global South scientists in the future.

Minor Points

1.12 Abstract. It seems important to add an upfront qualifier, that this is a perspective from a group of European authors.

This has been added, though in the introduction (lines 194 to 200) as we feel it fits better there than in the abstract, and in the summary (line 1082).

1.13 Line 75. The stated time horizon for the priorities outlined in the paper is 6 years, out to 2030. Given what is outlined, this does not seem realistic given the inertia in the enterprise and also the type of recommendations that are provided. We understand the desire to be relevant to the CMIP7/AR7 cycle but it would be more realistic to talk about a 10-year time horizon.

Agreed and we have modified the time horizon to be “the coming decade”.

1.14 Figure 1. This figure could be improved so that it is readily understandable. The boxes could identify the primary function in plain language and add the acronym in smaller font, e.g. “Global Coupled Modeling – CMIP”, “Regional Climate Modeling - CORDEX”, etc.

Figure has been modified to address these suggestions

1.15 Throughout the paper, acronyms should be spelled out. For instance, in lines 540, 585 what are EffCS, TCR, TCRE?

As far as we can tell, every acronym (apart from IPCC and UNFCCC, which we think do not need defining) is defined the first time it is referred to.

1.16 Line 450. “..and for supporting climate change adaptation.” Such references should be amended throughout the paper to also include mitigation, where it is applicable.

Done

1.17 Line 590. “For RCMS too short evaluation runs..” This is another place to iterate the need for a common protocol/evaluation framework for regional scale modeling, agnostic of the modeling tool, whether it is a GCM, RCM or an AI based model.

We have added text to emphasize the need for this at lines 489 to 493 and lines 764 to 768

Review #2 of “Bringing it all together: Science and modeling priorities to support international climate policy” by Jones et al.

The manuscript is an opinion article presenting a large number of authors' views on the (past and) present state of the international Earth system / climate modeling efforts, including assessment, impact, regional, etc. modeling and their recommendations on a number of priority research areas moving forward. As such, it will be another addition to the recent surge of similar opinion articles (some cited already in the manuscript). While I will respect the authors' opinions in my review because of the nature of the manuscript, I will offer a few comments and suggestions for authors' consideration below.

We thank the reviewer for their insightful comments and recommendations, the majority of which we agree with. Below we respond to each point raised and outline where in the revised paper we have added or modified text to address the specific concerns. The reviewer comments are in black text and our responses in blue. The revised paper is also included in this reply so the reviewers can see how our responses to their comments fit into the overall paper.

Reviewer comments and recommendations

2.1 As I indicated above, there has been a recent surge of similar opinion pieces which advocate for similar approaches going forward based on lessons learnt from the previous related efforts. The current effort is certainly more comprehensive than the others, but it will be useful to mention these recent reviews / opinions up front to provide the context and the need for the present manuscript, essentially answering why the community needs another such piece.
We have referred to these earlier perspectives in our paper and outlined how we take what is suggested in these articles a bit further in terms of details, scope or ambition. In particular, see lines 290 to 320.
https://www.nature.com/articles/s41558-023-01909-9
https://www.nature.com/articles/s41558-023-01849-4
https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2023AV001086
https://doi.org/10.1093/acrefore/9780190228620.013.933

2.2 As far as I can tell, all the co-authors of this manuscript are from European institutions. So, this article represents ONLY a "European" view of how these truly international – not just European – efforts need to be done. International community is not just Europe! This should be made clear.
We have made this clear in the introduction (lines 194 to 200) and in the summary section (line 1037)

2.3 The title may be interpreted to imply that the community does Earth system and climate modeling in service to climate policy only. I do not think that this is the intention of the authors. It is important to clarify that Earth system science stands on its own and a subset of related efforts serve the international climate policy.
Fair point. We propose a modified title:
Bringing it all together: Science priorities for an improved understanding of Earth system change and to support international climate policy.

2.4 We are in the era of co-development, co-design, co-planning, co-analysis, co-etc. of all these efforts with the communities that are impacted by climate change. While the article mentions mitigation, adaptation, etc. and related efforts, given the author list, all of these views and recommendations do not really reflect the views of impacted communities and the Global South for that matter. To avoid "we know the best for your community" perception, please be mindful of this and add caveats, acknowledging that

these are only “European” views and recommendations and that they may not reflect the true needs of the impacted communities.
Agreed and we have added text stressing the importance of co-production, analysis etc at lines: 183, 296, 922 and 928.

Agreed and caveats have been added to stress these are views from a group of European researchers only: Lines 194 to 200 and Line 1082.

2.5 Many of the challenges and issues covered in the article are not new. They have not been addressed for many reasons – some are discussed in the manuscript. The article states a target date of 2030 to accomplish some of these while starting some progress on the others. This is a rather tall order. It will be good to discuss what changed over the last few years that make the authors think that there can be significant progress on these challenges, especially noting that CMIP7 timeline is rather short with quite a few of the recommendations need to start very soon, if not now.
We have changed our proposed timeline to the “coming decade”. We have also emphasized that some work may deliver into IPCC AR7, but that these timelines are very tight. The CMIP7 science MIPs are likely to continue through to 2030, so we have highlighted that much of our proposed work will deliver into “an improved understanding of the Earth system and Earth system change”, with subsequent benefits for climate policy arising later e.g. IPCC AR8 and the 2033 Global Stocktake.

We have added some new text (lines 325 to 352) that outline aspects we feel have changed the situation over the past ~5-10 years that make us feel a number of key areas we discuss are now ripe for rapid development over the coming decade.

2.6 Related to #5 above, I suggest including a review of what the real and perceived impediments have been to date to accomplish the discussed recommendations and a discussion of what the impediments are going forward. Otherwise, I fear that this piece will be another “opinion” piece to be added to the existing ones without really addressing such impediments in a concrete way.
We have tried to address this request (not the easiest one to address) through new text at lines 325 to 352 and lines 995 to 999.

2.7 The manuscript has the feel of written by several authors. I suggest that the lead authors go over the acronyms, definitions, etc. carefully, making the manuscript more coherent. Machine learning is mentioned / discussed as a way forward in many of the sections with similar sentences. Should it have its own section and discussion, perhaps at the end, tying things together? There are also quite a few sentences that are long with multiple groups of a few words separated by commas. Such sentences are rather difficult to parse and understand. An example is the sentence on lines 526-529. Please rephrase these sentences.

We have gone through the manuscript and tried to homogenize the style and make the paper read more easily. All acronyms (except IPCC and UNFCCC which we feel do not need defining) are defined the first time they are mentioned. We have tried to break up excessively long sentences (as much as possible!).

On the topic of Machine Learning and AI. We agree this is potentially transformative for the science of Earth system change. Where we feel it can make solid contributions over the coming decade to the challenges we identify, we have tried to indicate this. That said, with respect to (i) generation of emission scenarios, (ii) Earth system modelling and global ES projections and (iii) climate change impact modelling, we do see the potential for ML and AI to make significant contributions, but nevertheless feel that the more established (physics/Navier-Stokes based) approaches to modelling still

have a great deal to offer over the coming decade. We have tried to be careful not to write this paper as a review of ML/AI potential or as a perspective piece on ML and AI. Such papers are appearing in the literature, and we refer to a number of these. We prefer not to have a specific section on ML and AI but rather leave that to other papers more specifically dedicated to that topic.