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Geoscientific Model Development Discussions

Interactive comment on "An integrated assessment modelling framework for uncertainty studies in global and regional climate change: the MIT IGSM-CAM (version 1.0)" *by* E. Monier et al.

Anonymous Referee #1

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Review of: An integrated assessment modelling framework for uncertainty studies in global and regional climate change: the MIT IGSM-CAM (version 1.0)

By: E. Monier, J.R. Scott, A.P. Sokolov, C.E. Forest, and C.A. Schlosser

Overview:

The authors present an integrated assessment modeling framework that features one-way coupling of the MIT Integrated Global System Model (IGSM) with NCAR's Community Atmosphere Model (CAM). The model appears to be particularly useful for uncertainty quantification given its flexibility and capability to vary economic parameters (and thus future emissions uncertainty) and climate parameters simultaneously. The paper

presents a set of 21st century climate change experiments for two different emissions scenarios and three sets of climate parameters that span the likely range of climate sensitivities. The authors analyze strengths and weaknesses of the model in simulating past and future surface temperature and precipitation changes at the global to regional scales. Much of the analysis is compared against multi-model ensembles from the CMIP3 and CMIP5.

The paper is well-written, informative, and provides a comprehensive description of the IGSM-CAM model. It could be a substantial contribution to the fields of model development, uncertainty quantification, integrated assessment, and climate change science. However, I was unsatisfied with some of the diagnostics and evaluation of the model, and I was left with lingering questions about the model's performance and capability in making reliable projections. I think the paper is a great fit for GMD, but I would like to see more model diagnostics in order to make the case for using IGSM-CAM to do uncertainty quantification at regional scales. I recommend publication in GMD after minor-to-major revisions.

Major concerns

-The modeling framework represents a single model and therefore a single model structure. I'm concerned that potential biases and model errors within CAM are simply being propagated across the entire IGSM ensemble. Perhaps coupling IGSM to a different atmospheric model would result in entirely different projected ranges. I think the authors should address these issues a bit more in the text. It could also be useful to compare the IGSM-CAM results with coupled CCSM3 or CESM, which also uses CAM as its atmosphere component (though the authors never explicitly state in the ms which version of CAM they are using). This could at least provide some information about whether the biases are shared between this modeling framework and NCAR's coupled GCM.

- I gather from the text that the coupling between IGSM and CAM is one-directional,

in that SST, land use change, GHGs/aerosols simulated by IGSM are used as input to CAM. It would be interesting to check the consistency between the modeled state of CAM and IGSM's 2D atmosphere for overlapping periods. How do the zonal averages compare between the models? Is the mean state and projected trends preserved? The authors show that 2100 global projections of SAT and precip are consistent between previous IGSM runs and the IGSM-CAM model, but some additional diagnostics looking at spatial aspects of the agreement (zonal averages) would perhaps provide insight into the capability of the model to simulate regional changes... in particular for the hindcast period as well.

- A central focus of the paper is on evaluating IGSM-CAM projections based on agreement with CMIP5 projections. I don't really see the value of these comparisons, since the forcings (emissions) are not the same between IGSM and CMIP5, as well as other limitations such as the aerosols not being spatially resolved in IGSM, which makes regional comparisons problematic. In order to evaluate the model's usefulness for projections, it would probably be better to focus more on evaluating the model against the historical record. This is done somewhat in Figures 4 and 5, but I think more diagnostics would greatly benefit the ms. Does the model reproduce past changes in emissions? What about model error for different time periods? Can it reproduce the mean state, seasonal cycle, and observed trends over the past 50 years? I think these types of comparisons would provide more insight into the model's utility and better highlight its strengths and weaknesses, particularly at the regional scales.

Specific Comments:

P2214,L7: Which version of CAM?

P2215,L13: How does IGSM enable structural uncertainties to be treated as parametric uncertainties? What do the authors mean by structural uncertainties?

P2216,L6-7: How do the authors quantify "efficient"?

P2219,L25-27: The authors state that a bias is present in the seasonal cycle of SST, but anomalies agree with observations. What kind of anomalies are the authors referring to? Is the model capable of simulating realistic variability (anomalies) in eastern equatorial Pacific (ENSO), which can remotely affect regional climate through teleconnections? Further, how well does IGSM-CAM simulate ENSO variability and the associated teleconnections? This is important for assessing the model's skill at simulating regional surface temperature and precipitation.

P2220,L3: The statement that IGSM-CAM does not account for spatial distribution of aerosols is troubling, given the potential impacts of aerosols on regional climate through dynamic feedbacks, such as linkages to the monsoonal circulations (e.g. ramanathan et al., 2005–pnas; Meehl et al., 2008–journal of climate). Can the authors reconcile this limitation in using the model to address regional climate change?

P2222, L24-26 – (Description of Figure 4): The comparison between IGSM-CAM error and the CMIP ensemble mean error is not particularly useful. It could be helpful to also see the direct comparison between IGSM-CAM with CESM (CMIP5) or CCSM3 (CMIP3), which uses CAM as the atmosphere component. Do these models share the same biases? However, such a comparison may also be problematic depending on which version of CAM is being used, and the stand-alone version of CAM may be optimized to different parameter values than the coupled version.

P2222, L24-26: Figure 4– the relatively large error over North America and Europe for IGSM-CAM is particularly concerning, since the paper's focus is on regional scale climate projections. Do the authors have any explanation for the lack of agreement in these areas? P2223, L7: How do the precip biases in IGSM-CAM compare with the NCAR model?

P2223, L22-23: I don't think Figure 5 makes the case that IGSM-CAM's skill in simulating precipitation is "reasonably good". A more fair assessment would probably be that it shares many of the same limitations as other CGCMs.

P2225, L19-22: I'm skeptical about the claims of local extreme temperature changes, given these regions also coincide with large model biases compared to observations of around 3-4C for the mean annual temperature (Figure 4). More evaluation of the model in the context of the observational record is needed to make these claims more plausible. It could be helpful to show seasonal model/data agreement for different time periods (do the model errors and biases change with time and forcings?).

P2226,L18: It would be good to also see hemispheric comparisons in plots 9 and 10

P2226,L25: How many CMIP5 models are used in making these plots?

P2226,L29: "CMIP5 model" should be "CMIP5 models"

P2227,L12: The large disagreement between precip in CMIP5 and IGSM-CAM is startling. Is it possible to decipher how much of this is due to differences in emissions versus using the CAM model? Is there perhaps some regional differences due to the fact that IGSM does not contain spatially resolved aerosols? I don't think agreement between IGSM-CAM and CMIP5 is necessary, since this constitutes a different modeling framework (with an additional human component and thus different forcings), but some additional discussion is warranted here.

P2228,L17: The authors repeatedly state that the model is more computationally efficient, but they never explain or quantify this statement.

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