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Interactive comment on "SICOPOLIS-AD v1: an open-source adjoint modeling framework for ice sheet simulation enabled by the algorithmic differentiation tool OpenAD" *by* Liz C. Logan et al.

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General comments:

The manuscript by Logan et al. describes the generation of an adjoint code by algorithmic differentiation (AD) for the ice sheet model SICOPOLIS. The authors give a clear explanation of the motivation for adjoint modelling and for the use of AD in generating adjoint code, and they produce and interpret simple example applications for both Antarctica and Greenland.

I thank the authors for producing a well written and easy to follow manuscript. As a

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Discussion paper

non-expert in adjoint modelling, I found P3 an excellent description of its context and capabilities. I found the demonstration sensitivity analysis in Figures 2-3 interesting as well.

I am not the best person to comment on validation of the adjoint vs. finite-difference code, but I found the authors' explanations generally supportive of their conclusions. Perhaps a bit more data (e.g. absolute value of the QoI) could be given to support the comment about numerical noise producing high misfit in Table 1, Columns 5 and 6.

I agree with the authors that understanding uncertain input variables and models' sensitivity to them is important for contextualizing ice sheet/sea level projections. However, I disagree with the framing of the first paragraph of the introduction, namely that (A) effective adaptation to/mitigation of sea level rise relies on reducing uncertainty in projections and (B) development of more sophisticated ice sheet models will help reduce uncertainty.

Regarding (A): The social-science literature of climate adaptation discusses assorted factors that affect adaptive capacity, many of which have little to do with the state of the science. If the authors are interested, they could refer to e.g. Lemos and Rood 2010 (WIREs Climate Change) for a discussion of the "uncertainty fallacy" in climate science.

Regarding (B): It is intuitive that improved understanding of ice sheet dynamics will help us produce models that give more physically-consistent ("predictable") results. But physical consistency does not always translate to less uncertainty. For example, models that include "tipping point" dynamics (or hysteresis and multiple steady states) are arguably more sophisticated than those that do not, yet future projections over a range of climate scenarios may show a wider, not narrower, range when tipping point dynamics are included. Initial efforts to improve model sophistication by including newly-understood or newly-proposed processes can also increase uncertainty in terms of inter-model or inter-scenario spread. A notable example is the widening of 21st-22nd century sea level projections shown in DeConto Pollard 2016 when the dynamics of marine ice cliff collapse and hydrofracture were included.

In a revised version, I suggest the authors strengthen the framing of the first paragraph to focus on the need for context and improved understanding, rather than leaning on the uncertainty angle.

The revisions I suggest to the introduction and in the specific comments below are relatively minor and should not impede publication. I imagine that many ice-sheet modellers will be interested in what the authors have shown here, and I look forward to reading follow-up studies using SICOPOLIS-AD.

Specific comments:

Figures 2 and 3 - both figure captions state that the [B] subplots illustrate sensitivities to July temperature. The text on P13,L3, Table 1, and later discussion refers to January or "summer" precipitation. Is there a mistake in the figure captions, or do the figures depict something not discussed in the text?

P4,L11 - "A model that can..." i.e. a forward model that can achieve the state deemed optimal by the Lagrange multiplier method? Does "reproduce the optimal behavior" refer to a model-vs-model or a model-vs-observation comparison?

P5,L10 - Is the adjoint code acceptable if the finite-difference-derived sensitivities approximate the adjoint-derived sensitivities, or is it the other way round? Intuitively I would expect that we accept the adjoint code if the sensitivities it produces approximate those derived by finite difference; that is, the finite-difference sensitivities are the "standard" against which the adjoint code is judged.

P5, Eq 3 - Given the explicit mention of "tolerance" in line 10, I might write the right-hand side of this equation with $= \frac{\partial \mathcal{J}}{\partial x} + \delta$, where δ is the accepted tolerance.

P5,L18 - This is a dense list of references without much discussion. Given that this manuscript focuses on an ice-sheet application, the study might be well-served by adding another paragraph to discuss specific distinctions among these past efforts in adjoint modelling of ice sheets and any notable contrasts with the present study.

P10,L2-5 - What is the initial geometry? It is a bit unclear whether the experiment is 100 yr near equilibrium, a 100-yr spin-up, or something else. I don't know that it matters for the adjoint process, but it would be nice to have some more clarity.

P13, Fig 3 - I understand that the point of showing the logarithm of the absolute value of the sensitivities is so that the reader can compare their order of magnitude, both across parameters and within-parameter spatial variability. Is there a reason that each subplot uses a slightly different colormap? Could one colormap be applied to all subplots to facilitate intercomparison?

P17,L4-6 - Is there a use for adjoint modelling in distinguishing between different possible parameterizations of these processes?

Technical comments:

P2,L23 - For balance, an example of a quantity that "parameterize[s] subgrid-scale processes or empirical constitutive laws" would be helpful. Perhaps iceberg calving or the routing of surface/basal meltwater would be an appropriate example to include?

P2,L30 - "key quantities of interest that represent integrated quantities of an ice sheet" -> "ice-sheet-integrated quantities of interest"?

P9,L22 - "...inherently non-differentiable, sometimes required..." is a comma splice. Replace comma by "and"?

P16,L11 - "Thus ... within the main trunk of SICOPOLIS" is a very long sentence and I had to read it multiple times to understand. Consider streamlining.

P16,L18 - The phrase in the parentheses is hard to parse and might be a run-on. The authors might consider replacing the remarks in parentheses with another full sentence or two to flesh out the thought.

P20,L1 - "Precipitation" or the "*sensitivity* to precipitation" is almost entirely positive?

P20, L3 - Typo "qsimulations"

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