Geoscientific
Model Development
Discussions

Interactive comment on "Description and validation of the ice-sheet model Yelmo (version 1.0)" *by* Alexander Robinson et al.

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The paper under review is a model description of Yelmo, a new ice sheet model written in Fortran. The model is open source, and this reviewer successfully examined and downloaded the source from github, and compiled and ran the model, with unimportant technical difficulties only.

Yelmo has a very conservative design, with essentially no new physical features or submodels, at least as described here. If the publication standard at GMD is that the model is geophysical and is described by the submitted paper then this standard is unequivocally met. However, a basic description of a model, as part of its (evolving) documentation, should be part of its source code release. It is not clear that such

model documentation is a publishable document; indeed a user's or developer's manual should match the changing versions of a code and address how specific capabilities are exploited. Because I suppose that the publication standard here is beyond that of such documentation, I have the following major concerns.

Concern 1. A new model should be justified by new directions for research, new ideas, and new capabilities. The most important concern: what is the direction of this work? Substantial effort has been expended on Yelmo but it is not actually clear for what purpose. Open source ice sheet models exist with its capabilities, or with substantially greater capability (e.g. BiSCLES, CISM, ISSM, OGGM, PISM, Sicopolis), and all of these are forkable and to varying degrees modular. The authors of Yelmo chose to develop a new model, and not to add new capabilities to an existing model, despite some ideas coming from Sicopolis. So, where is this new one going?

Concern 2. The "intended for collaborative development" and "flexible and user-friendly infrastructure ..." claims in the abstract, and repeated in various ways in the paper, are not demonstrated in any substantial way. For example, there is no demonstration that only minimal code extensions are needed to add a new capability. (Presumably this would be a great deal easier in e.g. Python than in Fortran anyway.) Does the model actually represent improved infrastructure for adding new capabilities? Noting that modularity does not, by itself, imply extensibility, if the model is extensible then the paper should demonstrate it.

Concern 3. The model verification mistakes of the past are recapitulated here. The EISMINT1 moving margin (MM) "benchmark" represented a failure of the community to read the literature 25 years ago, but there is no excuse now. As clear from textbooks (van der Veen, 2013, 2nd ed.) and well-known paper papers (Bueler et al, 2005), the Halfar (1983) exact solution is a full replacement for the MM experiment, which offers exact knowledge of what an SIA model should do. Regarding the EISMINT2 and MISMIP stuff, there is some excuse for using the benchmarks (though no evidence is given that the Yelmo runs offer more than the most common capabilities). The age-

model testing via a (divide) analytic solution is applauded.

Concern 4. Does the model run in parallel? The paper does not demonstrate or consider this but the tools seem to be suitable for it. In particular the Lis linear algebra library (ssisc.org/lis/), and the biconjugate gradient method, are used for the membrane stress solver component (SSA), and the library claims parallel performance. The concern here is that a lack of parallelism is exactly one of the limitations of many previous ice sheet models, thus limiting their attainable resolution.

Now we turn from concerns to suggestions and questions.

The disadvantage of the GMD paradigm, of peer-reviewed publication of snapshot model descriptions, even users' manuals, is clear in this paper. Author and reviewer effort is devoted to a rapidly-out-of-date paper instead of (for example) devoting that effort to improving the software itself, or its evolving online documentation. Please see the mission statement of a different publication model: https://joss.theoj.org/about. The idea of the Journal of Open Source Software is to treat the software itself, and its evolving online infrastructure, as the reviewable object.

A surprise about the design of Yelmo was the decision to use only a temperature variable in the energy conservation equation. To a significant degree this means energy is not actually conserved. The alternatives, of course, include the replacement of temperature by an enthalpy variable, or the addition of a field to make a temperature/water-content pair; either makes possible energy conservation in polythermal ice. On the one hand there is no question that near-base polythermal ice is a concern for all ice sheet simulations; I would not want to see any serious treatment of ice sheet time scales without it. On the other hand, one would want the model to also be flexible enough to work for temperate mountain glaciers or Greenland outlet glaciers. The authors seem aware of this issue, but to have simply not bothered. To the extent that there is a question here it is: what was gained by this choice?

The above paragraph deliberately ignores the second-to-last sentence of the paper

reporting a "plan to transition to" enthalpy, which only begs the question. Why not build the model based on the current state of the art from the beginning? This reviewer would be delighted to see a failed-or-not attempt to build a highly-principled and highly-conserving new model instead of a recapitulation of prior deficiencies.

This reviewer had understood that Sicopolis (Greve) and GRISLI (Ritz) were models under active development which had capabilities roughly a superset of Yelmo. Is this true or not? Is this a fork of either model? (It seems not.) Is Yelmo already justified by its modularity and API design, somehow lacking in Sicopolis and/or GRISLI? It would help readers to expose these aspects of the design.

The Antarctica validation results are acceptable but suggest no capability which would draw-in researchers to use this model. To compare rather directly, a just-published paper suggests the power of many modern ice sheet models to account for the dynamics of the Antarctic ice sheet, namely the initMIP-Antarctica paper Seroussi et al 2019 (https://www.the-cryosphere.net/13/1441/2019/). It lists 16 models of Antarctica (and 33 researchers), almost all of which would seem to have capabilities equal to or exceeding that of Yelmo. So where is this model going that is different, and why should we hope for new knowledge from it?

In summary, a new model like Yelmo, containing no significant new physics or model mechanisms, could in theory be useful. It could be a better piece of software than other offerings, it might have better performance, or it might be able to process input data faster or more flexibly, or it might just be implemented better and have better documentation. This paper does not convince me of any of it.

Line-by-line, generally minor comments:

p 2 line 1: Lipscomb et al 2019 does not solve Stokes.

p 9 line 23: Hard-coding BCG for the SSA solve is a bad idea. Have there been experiments with AMG in Lis? Can a performance comparison be reported?

p 11 section 2.4: I am surprised by the temperature variable. If the claim is that converting to/from the enthalpy variable is too costly, then this should be stated.

p 14 line 13: The EISMINT1 moving margin experiment has no justification *whatsoever*. Please use the Halfar (1983) solution so that you know the exactly-correct prediction of the SIA. See "Test B" in Bueler et al (2005), and add "Test C" from that source if you want variety.

p 14 line 16: "and thermodynamic": I hope not! EISMINT1 results depend on constant temperature (isothermal) ice, so energy will not be conserved.

p 14 line 17: ""Type-I" discretization models": The fact that Yelmo agrees with other particular numerics is not relevant; EISMINT1 reported groups of results that way so as to expose a flaw not propose a standard. Instead, please take the opportunity to compare model results to exact predictions (a.k.a. analytical solutions) of the continuum model when available, *which they are in this case*. Beyond Halfar (1983) and Bueler et al (2005) for the SIA, there are exact solutions of the SSA, including results in van der Veen 1983), Schoof (2006), and Bueler (2014).

p 14 line 29: This idea of smoothing is described in the reference Bueler et al (2007)as "non-physically 'smeared'". Whatever the meaning of this fiddle, it is not physics and the reference says that. (I think the point was that if *only* the temperature variable is symmetrized then the instability goes away, which modelers probably knew at the time but none had reported concretely. That is, the instability does not occur in a variable-softness model unless the softness variation is transported in 3D.) Supposing the EISMINT2 nonsense is valuable at all, please don't offer model users this smoothing, which hides physics.

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