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Geoscientific Model Development Discussions

Interactive comment on "Dynamically coupling Full Stokes and Shallow Shelf Approximation for marine ice sheet flow using Elmer/Ice (v8.3)" *by* Eef C. H. van Dongen et al.

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1 Summary:

This manuscript proposes a new method to couple the Full Stokes and Shelfy Stream Approximations so that different parts of a model domain can rely on different approximations of the stress balance equations. The idea of combining different stress balance approximations has been around for some time, with limited success concerning the inclusion of the Full Stokes equations, so it is great to see a new method being proposed. After the description of the method, several diagnostic and prognostic examples are shown to assess the accuracy of the solution and the gain of the coupling in terms of computational time.

There are several points either unclear or missing in the manuscript that are detailed below, including two critical ones that preclude me from accurately assessing this new method in the current version of this manuscript. The first one is that it is really difficult to follow the derivation of the coupling, because the equations are hard to follow. I understand that this is a very technical problem, but the goal is to describe in a way that is accessible to most interested readers, so some clarifications are needed. The second one is that there is no example showing the impact of the coupling method in a case where the Full Stokes and Shelfy Stream Approximations exhibit a significantly different behavior. This might be the case for the marine ice sheet experiment, but results from the Shelfy Stream Approximations are never shown, so it is impossible to undoubtedly assess the capability of this new coupling method.

2 Major comments:

The main point of this paper is to describe a new coupling method between the Full Stokes and Shelfy Stream Approximation equations. Unfortunately the current version of the manuscript is written in a way that makes understanding this new method quite challenging. First of all, all the derivation is hidden in the appendix, while it should be the core of the paper. Second, the appendix is a list of equations with no clear path to follow the demonstration, often jumping from one equation to the next with no explanation. Specifically, there should be a few sentences at the beginning explaining the method used to derive the additional force applied at the boundary between the two subdomains. It should be clearly stated when new terms are introduced and where they come from, or if it is a new definition (Let us define X as ...). I tried to understand the derivation of the force, but I must say that I am still quite confused by several parts despite spending ample time on it.

I find the introduction a bit biased to justify the need of this new coupled model. Having the opportunity to combine different stress balance approximations is indeed a genuine idea worth pursuing, and worth some investigations. So there is no need to emphasize the importance of solving a Full Stokes contact problem at the grounding line when recent studies suggest a limited impact (Pattyn and others, 2013), or to advertise using friction laws that require limited resolution around the grounding line (Gladstone and others, 2013) because it is easier to do with a Full Stokes model that would otherwise require too much computational capabilities to be solved at very high resolution around the grounding line. I would like to see the introduction a bit more in line with the literature, which would not diminish the importance of this manuscript.

The manuscript details the coupling between the Full Stokes and Shelfy Stream Approximations, but there are no details about the other difficulties caused by this coupling. Especially, there is no detail on how the domain is discretized into a 2D and a 3D part, and how this division evolves with time (how is the distance to the grounding line computed? how are elements switched between 2D and 3D?). There is also no detail on how the surface evolution is connected into the two parts of the domain. In one part of the domain, only one equation describing the thickness evolution needs to be solved, while in the other part, two equations describing the evolution of the upper and lower surface elevations need to be solved. Similar to the stress balance equation, some explanations must be added to explain the coupling in the surface evolution equations.

The notations in the equations are not always consistent, for example between eq.(4) and eq.(6). They both describe a similar quantity but one is based on the components of the tensor, while the other one is based on the velocity derivatives, for no obvious reasons. Same for η and $\bar{\eta}$ in eq.(3) and eq.(5). They both depend on the velocity, but in one case the dependence is explicitly stated while it is not in the other case, which tends to be confusing. There are also many terms introduced that are not necessary, adding more confusion.

Results for the marine ice sheet experiment with a pure Shelfy Stream Approximation

should be added to see how different the solution is from a Full Stokes model. The objective here is to assess the algorithm in a case where the Shelfy Stream Approximation and Full Stokes solutions are different. We don't know here if this experiment leads to different results with the two stress balance equations, and therefore there is no evidence that the coupling works in a case where the two solutions are different. So until we see how the coupling works in a case where the Shelfy Stream Approximation and Full Stokes equations lead to significantly different solutions, it is not possible to assess the capability of this coupling method to correctly produce an accurate solution that needs Full Stokes on a part of the domain.

3 Technical suggestions:

Note that the line numbering on each page starts with a different number, so I did my best to be clear but it might sometimes induce some confusion.

p.1 l.2: "their non-linearlity" → "the non-linearity"

p.1 l.2: "are used" → "are commonly used"

p.1 l.8: "periodical temperature" → "periodic temperature"

p.1 l.10: "modeling an ice sheet complex" → "modeling a complex ice sheet"

p.1 l.15: increased attention to what? (missing words)

p.1 l.16: Quantify "much". Also it seems that at least in Greenland, the majority of the changes are caused by surface mass balance changes (Enderlin and others, 2014).

p.2 l.1: remove "strongly": some materials rheology are much more non-linear that ice.

p.2 l.7: add Hindmarsh (2004) as a reference for the hybrid models

p.2 l.13: I disagree with the interpretation of the Pattyn and others (2013) results. In my opinion they show that models including membrane stress and whose grid resolution is sufficiently small capture grounding line evolution in a relatively similar way.

p.2 l.15: “complexes” → “systems”

p.2 l.17: add a reference for the Ice Sheet System Model

p.2 l.23-25: The important question is not so much if friction laws depending on the effective pressure law are faster, but to figure out which ones are more accurate and a allow a good description of the bedrock underlying the ice.

p.3 l.7: “strain rate” → “strain rate tensor”

p.3 l.15: remove “highly”

p.3 l.16: I don’t see the link between the non-linearity of the Full Stokes model and the derivation of simplified approximations. Most the approximations are also non-linear, and the main purpose of these approximations is to solve a system with less than four coupled unknowns, as is the case with Full Stokes problem.

p.3 l.20: So what terms are neglected in the Shallow Ice Approximation?

p.4 l.2: How is p eliminated?

p.4 l.2: I think the main difference that should be explained is that in the Full Stokes case, one has to solve a 3D problem with 4 unknowns, while in the Shallow Shelf Approximation, one has to solve a 2D problem with 2 unknowns.

Fig.1 caption: What is d_{GL}? Also change “coupling interface” and “grounding line” by “the coupling interface” and “the grounding line”

p.4 l.5: “parametrized” → “represented”

p.4 Eq.(9): $z^{\star}$ does not seem to depend on N in eq.(10).

p.5 l.14: “volume gain” is confusing, rephrase.

p.5 l.18: Why introduce the ice flux here. This is a new quantity that is never used in the paper, and could be simply replaced by its description $(H\bar{u}, H\bar{v})$

p.5 l.20-23: This 2D/3D explanations are a bit confusing. It is not clear if the SSA part of the model is still in 2D or 3D but the equations are solved only in 1D or 2D on a layer of the model, or if the mesh is indeed changed to 1D or 2D for the SSA, in which case it is not accurate to refer to 2D and 3D models only, and it would be more accurate to say 1D/2D and 2D/3D. Also, there are no details on what is done for the different parts of the mesh.

p.6 l.1-5: As mentioned just above, we don’t have any detail on how the discretization of the problem is done in the two parts, and how they are connected. For example, how is the domain decomposed into 2D and 3D (or 1D and 2D), and how does this evolve with time. Details need to be added to understand this part, especially the connection between the two parts of the domain.

p.6 l.12: How accurate is the residual free bubbles method?

p.7 l.3: “the SSA equations”: maybe add here that they are used as Dirichlet boundary conditions right away, that will be more clear.

p.7 l.6: What are A and b? Define them.

p.7 l.8: What is A_{SSA}? Define it.

p.7 l.8: “without the Dirichlet conditions”: this is really confusing. It is really hard to follow what is going on here, as none of the terms are explained or detailed.

p.7 l.10: see Appendix A: the derivation of the force applied at the interface of the Full Stokes and Shelfy Stream parts of the domain is the core of the paper. This explanation of where this force comes from and how it was derived need to be entirely moved to the main manuscript, and not hidden in an Appendix.

p.7 l.12: Why not take the depth-averaged velocity? This would be more consistent with the Shallow Shelf Approximation that computes a vertically integrated solution.

p.7 l.21: The surface evolution is solved differently for the two parts of the domain, but no detail is provided on how these two parts are connected, how the equations are actually solved (together or one after the other), and what is the impact in terms of continuity of the surface elevations, or feedback between the two parts.

p.8 l.29: How are the free surface equations solved to ensure continuity of the solution between the two parts of the domain?

p.10 Fig.3: Consider using round up the numbers in the colorbar. Also "where x_c" → "with x_c"

p.11 l.24: What are the basal conditions applied for this set-up?

p.11 l.25: Describe the "SPIN" experiment.

p.11 l.11 "minimal" → "small/limited"

p.11 l.11: "For the used mesh resolution" → "For this configuration"

p.11 l.13: "equal for" → "equal to 30 km for"

p.11 l.15: Add that the temperature is varied for 500 years and then kept constant for the remaining 2500 years. The equation of the temperature and its description look quite contradictory.

p.12: results for both diagnostic and prognostic marine ice sheet experiments with a pure SSA model should be added for comparison.

p.13 Fig.6 and Fig.7 captions: "solid line" and "dashed line" → "solid lines" and "dashed lines"

p.14 l.3: "follows" → "comes"

p.14 l.16-18: For the prognostic experiment, how is the repartition between the two subdomains computed and set-up? Especially, how is the distance to the grounding

line for each point computed (in 3D), and how is the mesh changed from 2D to 3D and vice versa when the grounding line evolves. For elements that are switched from the Shallow Shelf Approximation to the Full Stokes approximation, what is used for the velocity at the beginning of the step, especially the vertical velocity?

p.14 l.5: What is A_{FS}? Define it.

p.14 l.7: "and and"

p.14 l.19: "the computational work will decrease significantly". This is quite speculative and should be at least replaced by "is expected to"

p.15 l.11: "multiplying with": multiplying what?

p.16 l.24: "partly coinciding": why partly? There is only one interface between the two subdomains.

p.16 l.11: Eq.(A6) and Eq.(A9) are the same but just rearranged. u_h is not solution of both as it is the same one.

p.16 l.14: What is f_{SSA}? Is that how you define it: $f_{SSA} = A\dot{n}$? In the case why is the first integral over Γ_{SSAint} and the second one over Γ_{SSA}? If not, what is f_{SSA}?

p.17: 20: What about the across flow direction?

p.17 l.22: I don't understand where it comes from. You are trying to estimate the last term of Eq.(A4) to apply this force to the Full Stokes part of the domain. The force applied by one subdomain on the other and vice versa are equal, so instead you try to estimate the second term in eq.(A11). But is it ok to have A instead of σ? And how do you get from the weak form to the local equation This sounds pretty abrupt.

p.21: There is no mention in the paper of what friction law is applied.

4 Bibliography:

Enderlin, Howat, Jeong, Noh, van Angelen, and van den Broeke, An improved mass budget for the Greenland ice sheet, Geophys. Res. Lett., 2014.

Gladstone, Warner, Galton-Fenzi, Gagliardini, Zwinger, and Greve, Marine ice sheet model performance depends on basal sliding physics and sub-shelf melting, The Cryosphere, 2017.

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