Anonymous Referee #1 comments and response

We would like to thank the reviewer very much for the time and effort put into reviewing our manuscript. A point-by-point reply (in blue) to each comment (in black) by Referee #1 are given below. Specific changes in the manuscript are written in blue italics.

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.

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.

We agree that the manuscript improved when introducing terms and equations more clearly. Specifically, we added information about the additional force, as explained in response to the technical suggestions for p.7 l.8 and l.10, p. 16 and p. 17.
Besides that, we have rewritten the introduction such that it is clearer that the FS-SSA coupling proposed is more a domain decomposition method than actually coupling the stresses themselves. Specific comments are addressed below (suggestions to p. 16-17).

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.

Thank you for acknowledging the importance of this manuscript. The interpretation of Pattyn and others (2013) is addressed in the technical suggestion p.2 l.13. The section on a friction law that requires limited resolution around the grounding line (Gladstone and others, 2013) was included because it explains the numerical experiment used in this study. We agree that it was confusing to place this section in the introduction, and may give the impression that we advertise using such a friction law. Therefore, we have moved this part to the description of the numerical experiment where the sliding law in question is applied (Sect. 4.2.1), which will also avoid other confusion with respect to the sliding law (technical suggestion p.21).

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.
Thank you for pointing out missing information, this will be addressed in technical suggestions p.6 l.1-5, p.7 l.21 and p.14 l.16-18.

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.
Thanks for pointing these inconsistencies out. We have made our notations more consistent, by writing eq. (4) (revised manuscript eq. (5)) in velocity derivatives as well. The velocity dependence is removed from eq. (5) and also from Sect. 3.3 where the viscosity was written with velocity dependence.

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.

We show the difference between FS and SSA for the marine ice sheet experiment in comment to the technical suggestion p.12. However, we do not agree that a case where the FS and SSA exhibit a significantly different behavior is necessary to assess the capability of the coupling method. On the contrary, coupling the FS and SSA is only feasible in an area where the FS and SSA are alike, otherwise a coupling cannot provide a continuous velocity field. In cases where part of the domain is such that the FS and SSA exhibit a significantly different behavior, the most important task is to find a suitable coupling location, hence where SSA starts to become applicable.

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.
Thank you, we apologize for the confusion in the numbering.

p.1 l.2: “their non-linearlity” → “the non-linearity”
Done.
p.1 l.2: “are used” → “are commonly used”
Done.
p.1 l.8: “periodical temperature” → “periodic temperature”
Done.
p.1 l.10: “modeling an ice sheet complex” → “modeling a complex ice sheet”
We have changed it to ‘*a marine ice sheet*’.
p.1 l.15: increased attention to what? (missing words)
Thank you, we have reformulated the first sentences (see next comment).
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)
Thank you for pointing this out. As mentioned in reply to the first major comment, we have updated our references and stress that the main *uncertainty* comes from dynamic changes.
p.2 l.1: remove “strongly”: some materials rheology are much more non-linear that ice.
Thanks, the equation is strongly non-linear in a mathematical sense, but we agree that it may be confusing to call the rheology strongly non-linear so we have removed “strongly”.
p.2 l.7: add Hindmarsh (2004) as a reference for the hybrid models
Thank you again, this reference is added.
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.
We have rephrased the interpretation of Pattyn and others (2013), to state that it requires inclusion of vertical shearing and not necessarily Full Stokes and also included one more reference (Pattyn and Durand, 2013) to support this interpretation. Also, we have added a reference to MISMIP+.
“In MISMIP3d, GLD differ between FS models and SSA models, with discrepancies attributed to so-called higher order terms which are neglected in SSA models but included in FS models (Pattyn et al., 2013). Based on these model intercomparisons, it is advised to use models that include vertical shearing to compute reliable projections of ice sheet contribution to sea level rise (Pattyn and Durand, 2013). It should be noted that the experiments in MISMIP3d were idealized, laterally extruded 2D geometries with quite small sideward disturbances and MISMIP+ (Asay-Davis et al., 2016) may give more insight on realistic situations.”

p.2 l.15: “complexes” → “systems”
We have rephrased the sentence, such that this suggestion is not applicable anymore.
p.2 l.17: add a reference for the Ice Sheet System Model
Done, Larour et al. (2012) is added.

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.

We agreed that the important question is to figure out which sliding law is more accurate. As mentioned in reply to major comment 2, this section was included because it explains the numerical experiment used in this study. To avoid confusion, we have moved this part to the description of the numerical experiment where the sliding law in question is applied (Sect. 4.2.1).

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

Done.

p.3 l.15: remove “highly”

Done.

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.

Agreed, we have reformulated the manuscript,

“Due to the velocity dependence of the viscosity in Eq. (\ref{eq:Glen}), the FS equations are non-linear. Therefore, many approximations to the FS equations have been derived .. “

to

“Due to the velocity dependence of the viscosity in Eq. (\ref{eq:Glen}), the FS equations form a non-linear system with four coupled unknowns, which is time consuming to solve. Therefore, many approximations ..”

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

We assume that the Shallow Shelve Approximation is meant here. Thanks for pointing this out, we agree that the manuscript benefits from a more detailed description of the SSA and changed:

“For ice shelves, the Shallow Shelf Approximation (SSA), has been derived by dimensional analysis based on a small aspect ratio and surface slope (Morland, 1987; MacAyeal, 1989), such that conservation of linear momentum (Eq. (2)) simplifies to “

to

“For ice shelves, the Shallow Shelf Approximation (SSA), has been derived by dimensional analysis based on a small aspect ratio and surface slope (Morland, 1987; MacAyeal, 1989). This dimensional analysis shows that vertical variation of u and v is negligible, such that w and p can be eliminated by integrating the remaining stresses over the vertical and applying the boundary conditions at the glacier surface and base. Then, the conservation of linear momentum (Eq. (2)) simplifies to”

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

See previous comment.

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.

Agreed, we clarify this by changing

“The SSA equations are still non-linear through $\bar{\eta}$, but since vertical variation of u and v is neglected, and w and p are eliminated, they are less computationally demanding than FS. “ to

“The SSA equations are still non-linear through $\bar{\eta}$, but since w and p are eliminated and vertical variation of u and v is neglected, the 3D problem with 4 unknowns is reduced to a 2D problem with 2 unknowns. Therefore, the SSA model is less computationally demanding than FS.”

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

Thank you for your suggestion, a sentence is added. "*The distance between x_c and x_GL, defined in Eq. (17), is denoted d_GL.*"

p.4 l.5: "parametrized" → "represented"
Done.
p.4 Eq.(9): z* does not seem to depend on N in eq.(10).
Thanks for pointing out the confusing notation, the dependence disappears when assuming the hydrostatic balance. We have clarified this by writing "*In line with Gladstone et al. (2017), instead of modeling N, a hydrostatic balance is assumed to approximate z*, ..*"
p.5 l.14: "volume gain" is confusing, rephrase.
Rephrased to "*$a_{s/b}$ is the accumulation ($a_{s/b}>0$) or ablation ($a_{s/b}<0$) in meter ice equivalent per year.*"
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 ($H \bar{u}$, $H \bar{v}$)
Agreed, thanks for pointing out this simplification, it is changed in the revised manuscript.
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.
Thank you for pointing this out, we have rewritten the section such that it is more clear what is meant with 2D and 3D. This section is meant as a theoretical estimate of the memory and performance of a FS-SSA coupling, regardless of the implementation of the coupling, details on what is done for the mesh in the specific coupling presented in the manuscript will be provided in a later section (see comment to p.6 l.1-5 below). We agree that it is not very clear now that we do not consider a specific coupling, and added as a first sentence to this subsection:
"*The reduction of the memory required for a FS-SSA coupling by domain decomposition, compared to a FS model, can be estimated. This estimate is independent of the specific implementation of the coupling between the domains.*"

We have rephrased (see revised manuscript):
"*The number of nodes in Ω_{FS} is then approximately $(1 - \theta)N_h N_z$ and in Ω_{SSA} it is θN_h, neglecting shared nodes on the boundary. For a 3D physical domain, FS and SSA have 4 and 2 unknowns, respectively. Hence, the memory needed to store the solution with a coupled model is proportional to $2N_h (\theta + 2(1 - \theta)N_z)$. For a 2D simulation, where FS has 3 unknowns and SSA only 1, the memory is proportional to $N_h (\theta + 3(1 - \theta)N_z)$.*"

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.

Many thanks for emphasizing this, we have added the following:
"*First the velocity $\vec{u}$ (using FS or SSA) is solved for a fixed geometry at time t. The mesh always has the same dimension as the physical modeling domain, but $\vec{u}_{SSA}$ is only solved on the basal layer, after which the solution is reprojected over the vertical axis. Then, the geometry is adjusted by solving the free surface and thickness advection equations using backward Euler time integration.*"
Details on the connection between the two parts of the domain are added later, to Section 3.3, see comment to p. 8 l.29.

p.6 l.12: How accurate is the residual free bubbles method?
Different stabilization methods are extensively studied in Gagliardini and Zwinger (2008), and the residual free bubbles method is recommended there, mainly since it provides a better choice on low aspect-ratio elements. This citation is added to the manuscript.

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.
Thanks for pointing this out, similarly it is also added that the force boundary condition is of Neumann type. We changed “*The FS velocity at x_c provides an inflow boundary condition to the SSA equations.*” to “*The FS velocity at x_c provides a Dirichlet inflow boundary condition to the SSA equations.*”

p.7 l.6: What are A and b? Define them.
In other articles, such as Gagliardini et al. (2013), it seems sufficient to define A as the FE system matrix. We have rephrased such that it is more clear that ‘system matrix’ is a definition in FEM terminology.
“*The SSA equations are linearized, and by means of FEM discretized. This leads to a matrix representation Au = b, where u is the vector of unknown variables (here horizontal SSA velocities). In FEM terminology, the vector b that describes the forces driving or resisting ice flow is usually called the body force and A the system matrix (Gagliardini et al., 2013).*”
p.7 l.8: What is A SSA ? Define it.
A_SSA is A without the Dirichlet conditions being set, more detailed explained in the next comment.

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.
We agree that information on the way the Dirichlet conditions are set in Elmer/Ice may clarify the derivation of the contact force, therefore we have added
“In Elmer/Ice, Dirichlet conditions for a node i are prescribed by setting the i'th row of A to zero, except for the diagonal entry which is set to be unity, and b_i is set to have the desired value.”

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.
We prefer to describe the coupling in words in the main text and use more mathematical notation and derivations in the Appendix. Since Referee 2 did not complain about this, we prefer to keep it as it was.

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.
Yes, we agree that this would be more consistent. However, since the coupling requires a (very close to) constant velocity over the vertical, this choice will not significantly affect the results, but is much more straightforward to implement.

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.

Thanks for pointing out this missing information. To ensure continuity, H_SSA (x_c) = H_FS (x_c)is applied as a boundary condition to the thickness equation. We have added this to the revised manuscript:
"At x_c , H_SSA = H_FS is applied as a boundary condition to the thickness equation. First the surface evolution is solved for Ω_FS , then Ω_SSA follows."

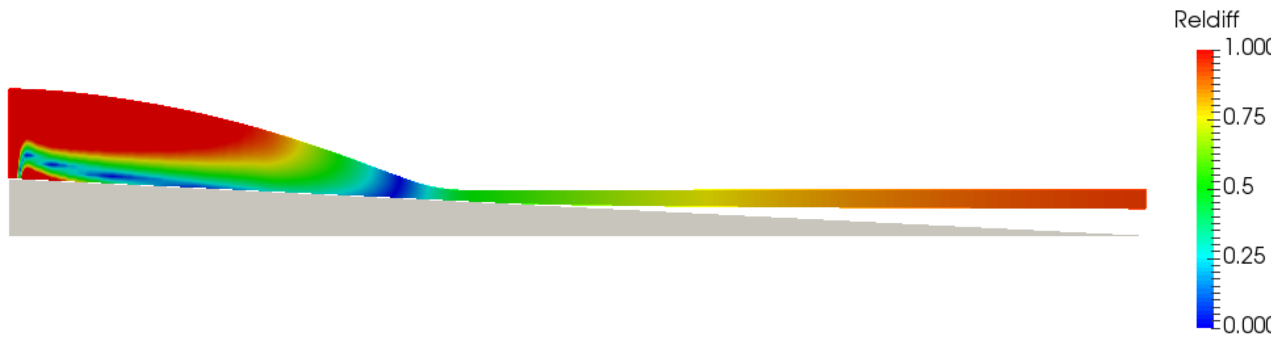
p.8 l.29: How are the free surface equations solved to ensure continuity of the solution between the two parts of the domain?
We have added the same information as mentioned in previous reply to the algorithm:
"Surface evolution by free surface equations (Eq. (14) for Ω_FS
Surface evolution by thickness equation (Eq. (15)) for Ω_SSA , with H_SSA (x_c) = H_FS (x_c)."

p.10 Fig.3: Consider using round up the numbers in the colorbar. Also "where x c " → "with x c "
Thanks, done.

p.11 l.24: What are the basal conditions applied for this set-up?
We have added a reference to section 2.2.1 where the sliding law is presented, and moved the text on mesh resolution and sliding laws that here, from the introduction:
"Gagliardini et al. (2016) showed that resolving grounding line dynamics with a FS model requires very high mesh resolution around the grounding line. However, Gladstone et al. (2017) showed that the friction law assumed in this study (see Sect. 2.2.1) reduces mesh sensitivity of the FS model compared to the Weertman friction law assumed in Gagliardini et al. (2016), allowing the coarse mesh used here."

p.11 l.25: Describe the "SPIN" experiment.
We have added more information about the SPIN experiment (added information in italics).
"*First, the experiment 'SPIN' in Gladstone et al. (2017) is performed, starting from a uniform slab of ice (H=300 m), applying the accumulation in Eq. (22) for 40 kyr, such that a steady state is reached."*

p.11 l.11 "minimal" → "small/limited"
Here, "minimal" did not refer to 30 km (which would make it replaceable by "small"), but to d_GL, since 30 km is not the exact distance before applying SSA, but it is the minimal distance between x_GL and x_c (upon mesh resolution this becomes 32.6 km in this case). We have changed
"this small difference shows that the minimal distance d GL before applying SSA is sufficient."
to *"this small difference shows that d _GL=30 km is sufficient."*
p.11 l.11: "For the used mesh resolution" → "For this configuration"
Done.
p.11 l.13: "equal for" → "equal to 30 km for"
Done.
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.
Done.
p.12: results for both diagnostic and prognostic marine ice sheet experiments with a pure SSA model should be added for comparison.
Below, the relative difference [%] between FS and SSA is shown, the relative difference in the grounded part is up to 1.8 percent where u_FS is at least 5 m/yr (not surprising, since basal friction is higher there), on the shelf the relative difference does not exceed 1% (compared to below 0.5% for coupled model). However, we do not consider it necessary to add this result to the manuscript, as argued in response to the last major comment.



p.13 Fig.6 and Fig.7 captions: “solid line” and “dashed line” → “solid lines” and “dashed lines”
Done.
p.14 l.3: “follows” → “comes”
Done.
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?
The mesh is not changed from 2D to 3D, as addressed in comment p.6 l.1-5. We add information to section 3.3 The algorithm, regarding the setup of the domain decomposition:
“First, the shortest distance d to the grounding line is computed for all nodes in the horizontal footprint mesh at the ice shelf base. Then, a mask is defined that describes whether a node is in Ω_FS , Ω_SSA or at the coupling interface x_c , based on the user defined d_GL. Technically, the domain decomposition is solved by the use of passive elements implemented in the overarching Elmer code (Råback et al., 2016), which allow for deactivating and reactivating of elements. An element in Ω_FS is declared passive for the SSA solver, such that is not included in the global matrix assembly of A_SSA , and vice-versa.”
and regarding switching between domains:
“An element may switch from Ω_SSA to Ω_FS , for example during grounding line advance. Then, the coupled iteration either starts with the initial condition for u_FS if the element is in Ω_FS for the first time, or the latest u_FS (t) computed in this element, before it switched to SSA.”
This holds for both horizontal and vertical velocity. The distance is computed based on the coordinates of the nodes in the basal mesh layer, according to Eq. (16) (Eq. (17) in the revised manuscript), by comparing coordinates and minimizing (called DistanceSolver1, the exact code can be found at
https://github.com/ElmerCSC/elmerfem/blob/d2f371327855ba4b73a0038dcc8ecf4400d25e07/fem/src/modules/DistanceSolve.F90)

p.14 l.5: What is A_FS ? Define it.
Mentioning that it is the system matrix for the FS equations should be enough as definition, as also done in for example Gagliardini et al. (2013).
p.14 l.7: “and and”
Thanks.
p.14 l.19: “the computational work will decrease significantly”. This is quite speculative and should be at least replaced by “is expected to”
Replaced.

p.15 l.11: “multiplying with”: multiplying what?

The entire equation is multiplied by a test function, this is a standard approach to derive the weak formulation on which the FEM is based. We have rephrased:
"After multiplying Eq. (2) with a test function v and integrating over the domain \Omega_FS".

p.16 l.24: "partly coinciding": why partly? There is only one interface between the two subdomains.
Yes, there is only one interface, but strictly speaking they only coinciding at the base, where SSA is solved, not entire on the vertical axis. We add information in parentheses:
¨partly coinciding with \Gamma_{FSint} (but of one dimension less)"

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.
u_h is the finite element approximation of the solution of (A6) and is the solution of (A9) with a particular choice of u_h and v (to be formal).

p.16 l.14: What is f SSA ? Is that how you define it: f SSA = Aṅ? In the case why is the first integral over Γ SSAint and the second one over Γ SSA ? If not, what is f SSA ?
f_{SSA} is defined in Sect 3.1, and now repeated in (A14).
p.17: 20: What about the across flow direction?
Many thanks, we have added information about the lateral boundary condition, denoted as *Γ_l*, to the Appendix, below Eq. (A3):
"*Furthermore, there is a lateral boundary Γ_l for Ω_FS ∈ R^3 , where the normal component also vanishes: v|Γ_l ·n = 0 and we assume a vanishing Cauchy-stress vector for unset boundary conditions to velocity components, such that the integral over Γ_l vanishes.*"
The same holds for the SSA domain, hence the lateral boundary only affects the definition of the test space in Eq. (A7) and the boundary integral vanishes.

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.
There is a force balance on the boundary at Γ_{FSint} and $\Gamma_{SSAint}. The term on the boundary in (A4) is equal to the term on the boundary in (A11) plus the pressure which is eliminated in SSA. Since SSA is integrated in the z-direction, the force has to be scaled by H.
If the integrals in the weak form are equal then the forces in the integrals are equal because they are equal for any v, in particular one with local support. A.n corresponds to the part of \sigma.n depending on the velocity u. The pressure part of \sigma.n corresponds to the cryostatic pressure on the SSA boundary.

p.21: There is no mention in the paper of what friction law is applied.
The friction law is found in Eq. (9) (Eq. (10) in the revised manuscript).

References

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