Review of Gladstone et al. "A Framework for Ice Sheet - Ocean Coupling (FISOC) V1.1"

Reviewer: Xylar Asay-Davis

I wish my name to be relayed to the authors, as I do not support the practice of anonymous review.

General Comments:

This paper describes a framework, FISOC, built on the Earth System Modeling Framework (ESMF), for coupling ice sheet and ocean components. Since ESMF is used for coupling in many Earth System Models (ESMs), the authors suggest that FISOC could provide an important stepping stone toward ice sheet-ocean coupling in an ESM. The paper describes the coupling infrastructure as well as the ice sheet and ocean components used for coupling verification. Then, the authors use two idealized test cases to demonstrate approximate conservation of mass and consistency between the grounding line as represented in each component.

The paper is well written and well organized. The figures do a good job at illustrating the design concept of the framework and its flexibility in addressing the unique requirements of the components it currently supports. For the most part, I find the description of FISOC and the verification experiments appropriately detailed and easy to follow. There are a few areas where I think more clarification or detail would be useful, as detailed below in the specific comments, before the paper is ready to publish in GMD.

First, the text mentions briefly that FISOC currently uses "sequential coupling" (but that "concurrent parallelization" would presumably require minimal effort). However, the text does not provide sufficient detail on how sequential coupling is performed, in particular what the conceptual start and end time of each component's coupling interval is. Nor is there any description of how this might be different for concurrent parallelization. I think these are needed to better understand the coupling strategy and, in particular, the inconsistencies between the geometry as represented in each component.

Second, there is no discussion in the paper about how the ocean components ensure ocean connectivity (if at all) and how this may need to be accounted for in the coupling. Would the ocean components allow melting in "subglacial lakes" (as emerge in the experiments of De Rydt and Gudmundsson, 2016) that meet the flotation criterion but are not connected to the ocean? If, so, this could drive unrealistic ice-sheet dynamics. Would these subglacial lakes be considered part of the floating area in the ocean component? If not, would this lead to a significant discrepancy in the geometric representation (or at least accounting) between the two components?

Third, while emphasis is placed on conservation, the interpolation methods used in the VE1 experiment are not conservative and therefore would not be appropriate for flux fields (like the melt rate) in ESMs. Relatedly, the results from VE1 demonstrate approximate, but not machine-precision, conservation of mass. Could this be improved by using conservative interpolation and also accounting for the mass accumulated in the coupler during a coupling interval?

Fourth, I found the geometry and design choices of the VE2 experiment hard to follow. It would be helpful to have a figure showing the initial side-view (x-z) geometry for the experiment as well as a cross-section of the geometry at 25 years shown in x-y in Fig. 5. It would also be helpful to have a velocity plot for the ice-sheet component similar to that for the ocean components in Fig. 6. The behavior of VE2 strikes me as quite dissimilar to realistic ice sheet/ice shelf dynamics in that the thickest part of the ice shelf is in an ungrounded region and (as near as I can tell) ice seems to be flowing out of the "inflow" boundary at x = 0. It seems like some acknowledgement of the rather significant limitations of this experiment are needed somewhere in the discussion.

As long as the authors make an effort to address these comments or explain their reasoning for not addressing them, I do not need to see a revised manuscript before publication.

Specific Comments:

l. 53: ESMF does not need to be redefined, since it was already defined on l. 47-48

l. 69-70: I think this is an important point that should be explored in a new subsection of section 2. Presumably, both components start at t=0. Which component runs first? Let's say it's the ocean. Once the ocean has finished a coupling interval, it has computed a melt rate. Is this averaged over the coupling interval or is the instantaneous value at the end of the coupling interval used? (This has important implications for how precise conservation of mass will be computed.) Presumably, the dD/dt is initialized to zero in the ocean component, so this is clear for the first coupling interval, but I will come back to this.

Then, let's say the ice-sheet component runs. It is able to apply the known melt rate for the coupling interval (10 days in VE1 and VE2), so there is no conceptual time lag here if the melt rate is a time average but there is one if it is an instantaneous value from t = 10 days. Based on the results of this coupling step, dD/dt can now be computed and interpolated to the ocean grid or mesh. This dD/dt is time-centered at t = 5 days, but will be applied over days 10 to 20 in the ocean component, so this is the source of a time lag that you discuss later.

If the components run in the opposite order, it is conceivable that the time lag could be placed on the melt rate instead of dD/dt. There is no explicit description of this, but I get the sense that this was not the choice that was made, since results from VE2 discuss a lag in D, not in melt rate.

If I am correct in assuming that the ice sheet component updates second, after the ocean, in a given coupling interval in the sequential scheme, this likely means you do not need to account for mass that conceptually accumulates in the coupler during a coupling interval. It seems important to me to discuss that “concurrent parallelization” will require a time lag in both dD/dt and in melt rate, since each component will be updated based on the state (or time average) at the end of the previous coupling interval. In this scenario, it would be important to keep track of the mass that conceptually accumulates in the coupler over a coupling interval over a time step. This is the approach used for fluxes between components in the Community Earth System Model (CESM) and Energy Exascal Earth System Model (E3SM), the ESMs I am most familiar with, and I think in other ESMs as well. Even in cases where components may run sequentially on the same processors, I do not think it is common to take advantage of this to remove the time lag in fluxes between components because of the conservation issues that could arise.

Again, I feel like some discussion of these nuances is missing from Sec. 2.

l. 85-86: ESMF does not need to be reintroduced and the citations are not needed because the acronym is already defined on l. 47-48 and the citation are already covered on l. 69-70.

l. 134: “All FISOC simulations to date have used a Cartesian coordinate system.” Is Elmer/Ice capable of using a spherical coordinate system? The BISICLES and MALI models that I have worked with both work only on Cartesian (polar stereographic) meshes, which requires special care to ensure flux conservation but can be handled as long as the coupling infrastructure is aware of the discrepancy in areas between the component models.

l. 155: “FISOC assumes that time-step sizes are not adaptive.” This seems quite restrictive to me and potentially unnecessary. It seems like this could use some discussion. I have worked with the BISICLES and Parallel Ocean Program coupled model called POPSICILES. In that model, we had a different coupling strategy and we always ran with concurrent parallelism over a coupling interval. We found many cases doing more realistic simulations where it was highly beneficial that BISICLES (which can perform adaptive mesh refinement) could refine its time step to handle a particularly tricky geometric configuration that might emerge spontaneously. We simply required that BISICLES perform a time step that exactly reached the coupling end time as the last step in a coupling interval. It seems like this strategy would also be compatible with FISOC, and therefore the requirement that the coupling interval is equal to the ice-sheet time step is unnecessarily restrictive. If there are important reasons for the restriction, it would be helpful if they are clarified. If this is not a strict requirement but rather has been the convention in simulations to date, this should be discussed.

Eq. (1): I think some more nuanced discussion is needed about the time-centering of dD/dt (which is at t - ½ Delta t) and when the dD/dt is actually applied in the ocean component (centered at t + ½ Delta t). You have dD/dt subscripted at time t but I do not think that is correct in either component.

l. 180: It is important to clarify that D is positive up. This might seem obvious from an ice-sheet modeling perspective, where D being positive down would not be an obvious choice since it can take either positive or negative values. But D in the ocean is often used for "depth" and is almost universally a positive quantity, so the choice of variable names and the sign convention are not intuitive for ocean modelers. There are also some later equations where I think the sign of D is not correct (as I will point out), leading to further confusion about the sign convention. A lot of confusion in the paper might be spared by renaming this variable "z_d" to go with "z_b" for the bedrock elevation/bathymetry.

l. 183-184: "but has the potential for the ice and ocean representations to diverge over time as a result of regridding artefacts": Isn't some part of the divergence in time likely to come from the fact that there is a time lag between dD/dt from the ice component and as applied in the ocean component?

l. 213: "...FISOC can pass the temperature gradient from the ice component directly to the ocean component." I think this requires some discussion. The temperature at the ice-ocean interface is computed on the ocean time step and could potentially have significant temporal variability within a coupling interval (e.g. because of ocean eddies). Would the ice sheet get the time-average of this field as one of the coupling fields, and use this to compute the temperature gradient? If so, this would result in the temperature gradient going back to the ocean having a time lag of 2 coupling intervals in the temperature at the ice-ocean interface. Maybe this doesn't matter.

An alternative approach would be to pass the temperature in the bottom ice layer (and the ice thickness) and allow the ocean model to compute the temperature gradient on its time step. The differences between these approaches is likely only to matter for particularly long coupling intervals but it might still be worthy of some though and some discussion. The choice is not entirely obvious, at least not to me.

Eq. (3): I believe the RHS of this equation needs a negative sign if D is positive up. Otherwise, the pressure would be negative.

Eq. (4): There is a sign problem with this equation, too. I am pretty sure it is that the whole RHS needs a negative sign again. If drho_o/dz were 0, you expect a positive pressure for a negative D. Later, drho_o/dz is given as a positive number, which is not physically reasonable if z is positive up. Density should decrease toward the ocean surface. But if that term is positive, pressure should increase because of increasing density at depths, so the term -0.5 drho_o/dz D_[O] is positive for negative D_[O], as it should be. If drho_o/dz is changed to be negative (as I think it should be), the sign of this term would also need to be flipped. In any case, there's something to be fixed here. The confusion may arise from an ocean component that uses a positive-down definitely of z, but I think the paper needs to pick positive-up for everything and stick with it.

l. 245: "z_b is the bottom boundary depth (bathymetry, aka bedrock depth)": Most times the term "depth" is used in ocean modeling, there is an implication that it is positive-down. The fact that the variable is called "z_b" might tend to counteract that but I would state explicitly that it is positive-up.

l. 246: “D_crit is a critical water column thickness (or depth)”: This one is strictly positive, and is unrelated to D, which I find pretty confusing. Again, renaming D to z_d would do a lot to help with this. By the way, I don’t see how the “or depth” bit applies at all in this context.

Eqs. (6) and (7): I’m having trouble following these. An illustration would help a lot, but some text carefully defining the variables involved might do the trick.

The original definition was “η is the free surface variable”. In the new context, it’s pretty hard to understand what η is. The best way I can understand it is that η + D_[O] is the ocean’s representation of the location of the ice-ocean interface, which is allowed to move up and down because of changes in ocean dynamic pressure. Maybe some explanation along these lines would be helpful.

As far as I can tell, Eq. (7) is equivalent to Eq. (6) except that D_crit is now a minimum ice-sheet thickness below flotation rather than a minimum ocean-column thickness? This is confusing and needs some explanation as to what exactly it means and why ROMS chose this definition instead of the simpler one from FVCOM. It is confusing to use the same name for variables with distinct meanings in the two models. Also, shouldn’t a slightly different D_crit be used for ROMS to get the same ocean-column thickness (assuming this is desired)?

l. 257: “as described in Section 2.8”: The current section is 2.8, so this must be a mistake. Maybe the reference is supposed to be to Sec. 2.5?

l. 272-274: “The coupling is purely geometric in that the ocean component passes an ice shelf basal melt rate to the ice component and the ice component passes a rate of change of ice draft to the ocean component.” This may be a nuance of interpretation but I do not think of the mass flux in the form of a melt rate as being a geometric quantity, so I would disagree that the coupling is purely geometric.

l. 280-282: I think it would be helpful to have an explanation for why the FVCOM simulation required a domain of a slightly different size. It is not clear if the sizes given in Table 2 are for both components or just the ocean component (in which case a row is needed for the ice component).

l. 295: “ρ_or = 1027 kg m^−3”: You give a slightly different value for FVCOM in Table 2 but this difference is not addressed here or anywhere else. Why the difference?

l. 297-299 and Eq (9): You gave a definition of the pressure at the interface in Sec. 2.7 already, and it was different from this for FVCOM. Presumably this redundant definition is not needed.

l. 303: I think “zero net accumulation” is a slightly confusing phrase here. I assume the idea is that ice sheet models typically have a field of net accumulation (called a) and that this is zero everywhere in this case. But it lends itself to the misunderstanding that there is accumulation but that the net effect is zero (e.g. averaged in time, space or both). Could this be simplified but just removing the word “net”?

l. 315 “ROMS specific details.” Nothing is given about vertical mixing or eddy parameterizations, whereas these details are given for FVCOM.

No details are given about how the three-equation parameterization is handled in either ocean component. For example, where are “far-field” temperature and salinity sampled? What parameters are used? (Are they the same for both models?) Which equation of state and equation for the freezing point is used in each.

l. 317: “FVCOM specific details.” In addition to the above, no details are given about time stepping for FVCOM as they are for ROMS.

Eqs. (10) and (11): As I stated in my general comments, I think a figure is needed to help better understand this experiment. A starting point would be a side-view (x-z) figure showing the initial ice-sheet, ice-shelf and ocean configuration as given by these equations.

Also, since rho_or is slightly different for the 2 ocean components, is (11) accurate and H is therefore slightly different for the two but D is the same?

l. 338: “No restrictions to ice flow are imposed at the upstream and down stream boundaries”. I have several difficulties here. First, some more explanation is needed about what “no restrictions” really means. Presumably, this means that ice is free to flow out of the boundaries. I do not see how ice can flow in through these boundaries if there is “no restriction”. Is it necessary to calculate stresses at the boundaries and, if so, how is this handled (in particular driving stress)? Why was an open boundary condition like this chosen at x=0? A more typical setup would place a solid boundary here so it acts as something of an ice divide. This would also make the direction of ice flow a lot less ambiguous.

That brings me to the second point, which I will discuss more below. The ice flow field is not discussed but I get the impression based on the thickness evolution that flow is happening out of both the x=0 and x=100 km (or 99 km) boundaries, so that the “upstream” and “downstream” directions aren’t well defined in this problem.

l. 364-366: I found this paragraph redundant to the paragraph on l. 268-270 and subsequent text. I realize it is nice to summarize things again from previous sections but this seems too repetitive to me.

l. 372-373: I don’t think “along the domain” and “cross-domain” are well defined directions because they take the perspective of the ice flow in a context where ocean circulation is being discussed. I would just call these the x and y directions.

l. 382-383: “The net mass change of the coupled system is more than an order of magnitude smaller than the mass change of the individual components for both experiments VE1_ER and VE1_EF.” I think this needs considerably more discussion. For ESMs, anything less than machine-precision conservation is not considered acceptable and is one of the most important mechanisms for diagnosing model inconsistencies. To accomplish this, conservative regridding is always used for flux fields (the melt rate in the case of FISOC, and the heat flux in the future).

Ice sheet-ocean modeling requires that special care must be taken to distribute that flux field to the ice-sheet component because melt should not get distributed to grounded cells by mistake but it also should not be lost in the regridding process because this would affect conservation. This issue is exacerbated by inconsistent representation of the grounding line between components. Is this taken into account in FISOC? If so, please discuss. If not, please discuss this as a potential issue for future consideration.

Aside from interpolation, conservation of mass may be inexact in FISOC because of the lag between when melt rates are computed in the ocean component and when they are applied in the ice component. I convinced myself when I was discussing the staggered coupling approach above that this is likely not the case in the current approach but it would be in an approach with concurrent parallelism. Even so, it would be important to diagnose that total mass going into the coupler is exactly equal to total mass coming out of the coupler after each coupling interval (i.e. after both components have run) or that the difference between these two is computed and stored within the coupler to be distributed appropriately at the next coupling interval.

Overall, I would like to see some discussion about why conservation of mass in FISOC is good but not machine-precision good.

l. 386-387: “While the initial slope of the lower surface of the ice shelf is the same in both VE1 and VE2”: I misunderstood this the first time I read it to be saying that the D’s for VE1 and VE2 were the same. They differ by 20 m but the slope is the same. I guess it’s fine as it is but I wanted to let you know about the confusion, in case you want to do anything about it.

l. 387: “the open inflow and outflow boundaries”: I remain confused about the open “inflow” boundary at x = 0. Is it really inflow? If so, how does open inflow work?

Fig. 5: First, as I stated in my general comments and as I think you are fully aware, this is an odd ice-shelf geometry. It is also not very intuitive to see thickness plotted as an x-y field, at least not for me. It would be more helpful in my opinion to have a more 3D plot similar to Fig. 3. It would also be really helpful to have a vector field for the ice like the one for the ocean velocity in Fig 6, especially because I want to see how much the weird geometry is due to outflow (instead of inflow) at x = 0.

Fig. 6: Are these fields interpolated to a common, regular grid? They look like they might be and, if so, this should be mentioned.

l. 415-421: It may be worth remarking that the difference in grounded area does not increase with time even with the Rate method, at least in this case.

Typographical and grammatical corrections:

l. 7: “these mechanisms” missing a space

l. 8: a comma is needed between “this” and “ocean”

l. 22: “(MISI) (Mercer, 1978; Schoof, 2007; Robel et al., 2019)”: I would combine these parentheses as you have done elsewhere: “(MISI; Mercer, 1978; Schoof, 2007; Robel et al., 2019)”

l. 38: Similar to above: “(ISOMIP+; Asay-Davis et al., 2016)”

l. 46: commas are needed: “Here, we present a new, flexible…”

l. 46: for consistency, a semicolon is needed instead of a comma: “(FISOC; Section 2)”

l. 47-48: “Earth System Modeling Framework” is a proper name so I think it needs to be spelled with the American version of “Modeling” that is used on their website: https://www.earthsystemcog.org/projects/esmf/

l. 53-54: “(Hill et al. (2004); Collins et al. (2005))” should be “(Hill et al. 2004; Collins et al. 2005)” without the nested parentheses.

l. 105: a comma is needed between “versa)” and “all”

l. 117: a comma is needed between “cases” and “a non-standard”

l. 120: “Regional Ocean Modeling System” is also spelled with the American spelling of “Modeling” in the documentation I could find: https://www.myroms.org/

l. 120: There should not be nested parentheses: “(ROMS; Shchepetkin and McWilliams 2005)”

l. 120-121: For consistency, this should be “terrain-following, sigma-coordinate” (with a comma and a second hyphen). In general hyphenation is used a lot more sparsely in this writing than I would use it but I fully acknowledge that that is a stylistic choice.

l. 123: Remove the nested parentheses: “(FVCOM, Chen et al. 2003)

l. 136: a comma is needed between “extrapolation” and “which”

l. 142: “time step” is not typically hyphenated but this may be a stylistic choice.

l. 174: a comma is needed between “used” and “this”

l. 176: a comma is needed between “large” and “occasional”

l. 194: a comma is needed between “case” and “the user”

l. 209: “ocean model ice shelf cavity shape” is quite a long compound noun…

l. 228: “kg m” needs a space or half-space

l. 247: a comma is needed between “Thus” and “cells”

Eq (7): Please remove the asterisk as a multiplication symbol. It is not needed and is not considered a valid multiplication symbol (outside of code).

l. 258: a comma is needed between “Study” and “dD/dt”

l. 275: I’ve left most of the hyphenation choices alone but I feel pretty strongly that “uniform-thickness” should be hyphenated.

l. 286: a comma is needed between “system” and “we”

l. 287: a comma is needed between “Therefore” and “the”

l. 288: a comma is needed between “corners” and “where”

Eq (9): should end in a comma, not a period.

l. 309: a comma is needed after “experiment” at the end of the line

l. 326-327: commas are needed after “VE1_ER” and “VE1_EF”

l. 338: “down stream” should be “downstream”

l. 360: a comma is needed between “interval” and “this”

l. 368-369: a comma is needed between “days)” and “the” and again between “years)” and “the”

l. 378: a comma is needed between “melting” and “the”

l. 380: a comma is needed between “system” and “the”

l. 388: commas should be removed from “...component and the relatively shallower ice in the grounded region both…”

l. 397: a comma is needed between “melting” and “as”

l. 400: a comma is needed between “2.8” and “the”