Dear Ludovic,

thanks for forwarding the additional statement by the reviewer, which helps me to understand his point of view. In the first round of review, he criticized an “overstated stability claim” and I asked whether he refers to a specific definition of stability that differs from the basic one that I prefer. Since there was no response and no further discussion on this aspect, I concluded that the reviewer was not firm in this field, which is presumably wrong.

While I find the explanation via maximum and minimum (lines 128–132) more intuitive for the average readers, the reviewer obviously prefers the consideration of the energy (corresponding to the L^2 norm). Concerning the basic definition of stability, i.e., that perturbations remain bounded, the result is the same. Equation 4 in the comment states that the energy $\frac{1}{2}||s||^2$ decreases under all conditions. In the context of the widely used simple definition of stability, this ensures that perturbations do not grow, corresponding to unconditional stability. **I clarified that stability only refers to bounded growth of perturbations (lines 127–128).**

The following part is true, but not immediately related to stability in my opinion, although the reviewer uses the terms “linear” and “non-linear” stability. In this sense, “true dissipation” is not well-defined, except that it corresponds to a fully implicit scheme. There is no doubt that a fully implicit scheme has a better (higher) dissipation because the spatial pattern of D and ∇s is the same, as the reviewer writes. In the semi-implicit scheme, the patterns of D and ∇s may detach. This is exactly the “staircase” effect we see in the simulations, where flat and very steep regions in the ice surface develop and switch their roles through time. Nevertheless, the term at the right-hand side of Eq. 4 remains negative (and the last-term at the left-hand side is positive), which confirms that the staircase oscillations remain bounded.

Equation 7 defines a condition for “non-linear energy stability” in the form that the relative change in D must be small in each time step at each location. It is clear that this condition would ensure a “reasonable” solution also at large δt. MinSIA enforces this condition at least for moderate δt by the dynamic stabilization of the term $|\nabla s|$ in D, which seems to be some kind of “devil’s work” for the reviewer.

In sum, the combination of the semi-implicit scheme with the dynamic stabilization could be discussed in a more formal way based on “non-linear stability” as preferred by the reviewer. However, I would not find it helpful to introduce a very specific definition of stability (which I did not find to be given clearly in any of the cited papers), which somehow contradicts the simple and widely used definition.

So I finally agree that the discussion elaborated in the reviewer’s document is substantial, but I do not share the opinion that it is worth mentioning in the manuscript on such a formal level.

Best regards,
Stefan