The comments from the reviewers are in black font and our responses are in blue font.

1 Hypothesis for the diagnostic damage model, i.e., fractures accumulate much faster than they advect.

The choice of stress threshold in the model must have a significant impact on the results, as it directly influences where and how easily damage initiates. In the first experiment, the authors use $\sigma_t = 0.02$ MPa, a very low threshold that contrasts with higher values used in other studies (e.g., Krug et al., 2014; Sun et al., 2017), sometimes calibrated to match observed calving rates. While I understand that the model?s focus is on damage accumulation rather than explicit calving, this low threshold promotes rapid damage development across broader ice regions than higher threshold values. This, combined with the feedback effect of damage on the source term (i.e., $f = f((1-D)^{-k})$, means that initial damage quickly propagates in subsequent timesteps. Given these sensitivities, a discussion of the implications for using a low σ_t, as well as the role of the damage rate factor B, would help better justifying the hypothesis.

Without this, as a reader, I feel like the value picked for σ_t and B (I could not find the value) affect the realism of the hypothesis $\delta \gg 1$, leading to fast damage creation rate. It makes it look like the experiment is built to match the hypothesis. While I do not see any argument to revoke the hypothesis, I do not think either that the results validate the hypothesis?although later experiments seem to show little effect of σ_t which could be specific to the experiments? The sentence ?This agrees with the theory that where $\delta >> 1$,fractures accumulate much more rapidly than they are advected away by ice flow? is therefore a bit of a stretch to me. Since this hypothesis and the first experiment condition the definition of D_{acc} . I think that these limitations should be better discussed

We thank the reviewer for this point and acknowledge that some of this discussion was not clear enough in the initial submission. The thesis of the diagnostic model is that the rate of damage accumulation is rapid enough that it generally occurs within a model timestep and, therefore, does not need to be explicitly modeled. While we agree that the choice of the stress threshold significantly affects the extent of damage produced, the stress threshold does not play a role in the rate of damage production; this is modulated entirely by the two fracture parameters B and r, as well as the stress scale (which depends, in the flowline model, on ice rheology, the velocity scale, and the length scale).

We also agree that the value of σ_t is quite a bit lower than we would expect in natural ice sheets. This value was chosen because of the simplicity of the flowline model; it produces generally very low stresses, requiring a low stress threshold in order to produce fracture. Since we wanted to study the importance of the rate of damage accumulation, we had to pick a stress threshold that would produce some amount of damage. Since the value of the stress threshold does not appear in δ, it does not affect the validity of the results. To show this, we will rerun Figure 2 for a different stress threshold value, $\sigma_t = 0.05$ MPa (which we will include in the Supplement), which will show largely the same results as in Figure 2.

The parameter study in Figure 2 does, effectively, consider the role of B (and all other parameters within the fracture timescale) on this hypothesis. If we use the Pralong and Funk 2005 fracture source term, the fracture timescale is defined as:

$$[t_f] = B[\tilde{\sigma}]^r \tag{1}$$

and therefore varying the fracture timescale effectively varies the fracture parameters, and the hypothesis that fracture accumulation is much faster than flow advection does not apply for low values of B. We present Fig. 2 in fracture timescale space, rather than B space, to make it generally applicable to many different damage models (that is, someone could calculate the fracture timescale of a different damage model and determine if this hypothesis is valid using Fig.2). Therefore, the applicability of the hypothesis is explicitly not dependent on any particular damage model but is instead presented as a more general theory.

The applicability, however, to a specific problem does require determining the fracture and flow timescales. Doing this, barring any intrinsic or physical understanding of fracture timescales, requires us to assume some damage model and calculate the timescale to verify where it falls in δ space. This is where we, for illustrative purposes, apply the Pralong and Funk model for comparison. The specific values of B and r are those widely applied in other glaciology studies building on the Pralong and Funk model [11, 7] and were originally constrained by laboratory experiments [9]. We will improve clarity of these points in the text in Section 2.

2 Unsymmetrical damage for a symmetrical geometry and numerical artifact

I would suggest the authors discuss whether such factors could contribute to any asymmetry observed in their model results and clarify the type of stabilization methods employed, as well as any expected artifacts. Addressing these potential sources of numerical asymmetry (and diffusion) would strengthen numerical aspect of the study.

Since the MISMIP+ configuration is symmetrical along the central flowline, any asymmetry in the modeled damage field (or other model output) may indicate numerical artifacts rather than genuine physical behavior. Artifacts of this nature often arise from issues like model stability (e.g., CFL condition) or other challenges in the advection scheme, particularly in handling diffusion-free processes like damage. In these cases, artificial diffusion or other stabilization techniques are often used, which can introduce numerical artifacts.

This is a great point, it is correct that these asymmetries do not arise from the model configuration, given that both the MISMIP+ geometry and the forcing are symmetric. While there are no stabilization techniques imposed in the solver, this asymmetry could be due to a number of factors. We agree that numerical diffusion does occur in Eulerian advection schemes, and small amounts of numerical diffusion could explain these asymmetries. This could also arise from physical symmetry breaking, in which asymmetric patterns can arise in systems that contain symmetric equations and forcing due to, for example, small errors from numerical approximations amplifying. Given that reductions in the timestep do not have significant effects on the solutions (Supplement Fig. S4), the solution appears stable towards numerics and thus this could be evidence for physical symmetry breaking explaining these asymmetries. This could also arise due to asymmetries in the mesh itself, which can be seen in some of the grounding line profiles. While we don't believe this affects the scientific conclusions of the paper, we agree that it is important to explain these artifacts. We will add a discussion of these asymmetries in Section 3 when discussing the MISMIP+ results. In the revision, we can also discuss methods for addressing these numerical issues, such as the Material Point Method employed by previous papers [6, 7, 8].

3 Specific Comments

Line 17: I found this first sentence a little strange, I guess that the authors refer to ?viscous ice flow? as opposed to ?elasto-brittle calving?. Maybe consider rephrasing this.

This sentence is intended to describe the viscous response of ice sheets, rather than brittle calving. We have simplified this to just say "Ice flow".

Line 30: the term ?instability? is not really clear here. I would detail a little more the processes it refers to.

We've specified more clearly what we mean in that line.

Line 40: I would expand the ice sheet model timescale to as low as ~ 10^{-1} year. More and more models are interested in seasonal changes (without going to visco-elastic models interested in tidal effect, ...).

Done.

Line 60: I would change ?accumulated fractures? for ?accumulated damage? since CDM was used to enhance damage, until damage was deep enough to trigger a fracture over the entire column with LEFM.

Done.

Line 72: switch the order of the two citations

Done.

Line 102: ?We evaluate the applicability?

Done.

Line 106: I think that TC writes equations as this: ?Eq. (1)?. Think about correcting this for other references to your equations

Done.

Equation 5: You therefore assume that $[t_a] = [t]$ to simplify your first term but isn?t $[t]$ the resultant of the advective ($[t_a]$) and the fracture ($[t_f]$) timescale? If guess you assume $[t_f] \ll [t_a]$ to make the simplification but this relies on the hypothesis you do after. I might be missing something here.

The timescale $[t]$ that we had defined in the first submission was describing the timescale of the flow problem, rather than the fracture problem, and therefore was by definition the advective timescale. In this revision, we have defined the timescale of the flow problem up front and removed any use of $[t]$, to avoid confusion.

Line 130: how do you justify this hypothesis for a typical damage model?

Thanks for this point; we acknowledge that this should have been made far more explicit in the original submission. The statement itself is intended to be a hypothesis that is justified in the remainder of Section 2 (and we have now stated as such in that line).

As for the justification itself, this lies in the comparative timescales of flow to fracture, which is a general statement that can be applied to any damage model. Any such damage model can be evaluated using Equation 6 to determine what the ratio of timescales is. However, we acknowledge that, since Section 2 had focused only on using the Pralong and Funk 2005 version of the damage model to evaluate the validity of the hypothesis, this was not obvious. In this revision, we have added a new section 2.5: Reconciling the Diagnostic Damage Model with Other Damage Models in 2D, which we believe makes this point stronger for two reasons. First, it uses the 2D MISMIP+ geometry and model setup, rather than the simpler flowline model. Second, it directly compares the result of three different damage models: the diagnostic model proposed in this study, the Pralong and Funk 2005 model, and the Sun et al. 2017 model. We show (see Figure 1 in this response) simulations (with no changes to melt or surface accumulation) that all three models, when set up in a consistent way, produce approximately the same behavior and damage fields.

Importantly, we make the argument that this diagnostic damage model reconciles the two common approaches for modeling damage in ice sheets: the power-law source term of Pralong and Funk 2005 [11] and the Nye Zero Stress approach of Sun et al. 2017 [12] (and many other studies). In this new section, we explain that the diagnostic damage model approximates the Pralong and Funk model when their damage rate factor B is sufficiently large. We also explain, which we did not do well enough in the original submission, that when using experimentally-constrained damage parameters in the Pralong and Funk model, the accumulation of damage is sufficiently large for the diagnostic model to approximate the full transient model. We also show that the diagnostic damage model produces the same behavior as the model of Sun et al. in a certain configuration: that is, when $\sigma_t = 0$ and the stress criterion is the maximum principal stress, which are fundamental assumptions underlying the Nye Zero Stress model. Notably in Figure 1, a cross-glacier rift does not form, as it does in the main text. This is because these simulations do not include any basal melt forcing.

In this way, the diagnostic damage model can be thought of as a more general approach to the Nye Zero Stress approximation, in which we generalize the assumption for any σ_t and any stress criterion (and later in the paper we explore the implications of these choices). Finally, we also argue, but don't directly show, that the diagnostic damage model also reconciles the strain-rate-based approach of Albrecht et al. 2012, 2014 [2, 1], in the limit where their damage accumulation factor $\gamma \gg 1$.

We address the specific comment that the results shown in Sun et al. 2017 appear to have less damage than is shown in this study below, where the reviewer specifically makes this comment.

Line 135: could you precise if you use a linear or a non-linear (typically $m = 3$) Weertman friction law. I could not find the information in the supplementary material.

We've now specified that we use a nonlinear Weertman sliding law with $m = 3$.

Line 174: you mention $\sigma_t = 0.02$ MPa as an arbitrary value. Other studies use much higher values (at least about 0.1 MPa) (e.g., Krug et al. 2014; Sun et al., 2017). For Krug et al. (2014), these values have been calibrated with observations of calving rate. Although I understand that your model only aims to simulate damage (without going to calving or riging), I think that the choice of σ_t is very important and will largely affects the result. Low σ_t allows to easily damage the ice in many regions, and due to the term $(1 - D)^{-k}$ initiated damage leads to even more damage at the next timestep. It also depends on the value of B in Eq. (8).

We've addressed this in more detail above. For the purposes of the flowline model, this value is set low enough to produce damage in a model that does not produce such high stresses as are common in natural systems. Since σ_t does not affect the hypothesis of timescales, we do not believe this affects these results. They do, however, affect the significance of damage to mass loss, as explored in Section 3, and there we use more realistic values of σ_t and explore the effect of uncertainty in this parameter. We have added text to improve the clarity of this point. We will also rerun Figure 2 for a different σ_t value to show that it produces largely the same result.

Lines 216?224 and general statement about Section 3: I think that the limitation of your hypothesis is visible in your supplementary material where Damage seems particularly high and potentially overestimated. For example, your damage is much higher than Sun et al. (2017) but it might also be due to the difference in the criterion used to calculate the damage source () might also present biases due to other limitations in the physical model and its numerical implementation).

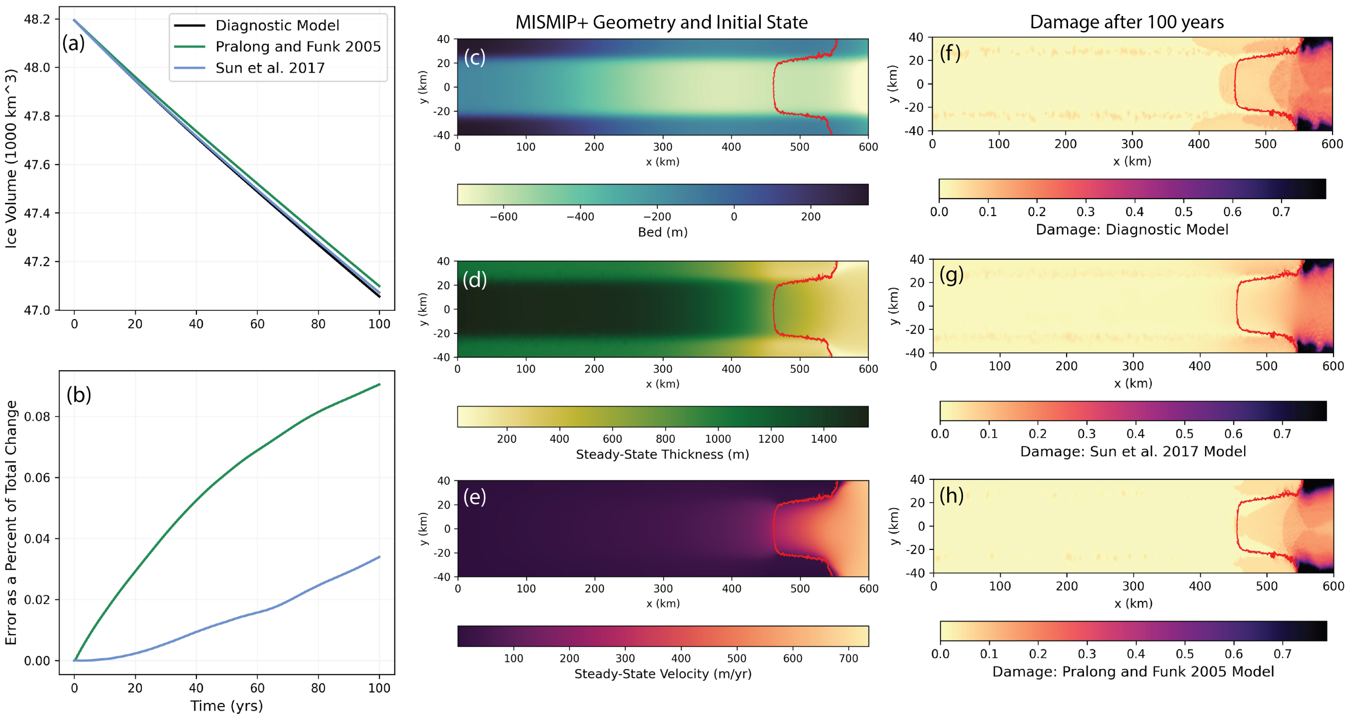

Figure 1: **Comparison between 2D transient and diagnostic damage models:** We set up a 2D model geometry as prescribed by the MISMIP+ configuration [3] and run this from a steady state (c-e) without climate forcing with couplings to three different damage models: the model of [11], the model of [12], and the diagnostic damage model proposed in this study. We will refer to the models of [11] and [12] as “full” models. We show mass loss as estimated from each of these three models (a) and the errors between the full models and the diagnostic damage model (b). The error is reported as the mass loss from the full model coupling minus the mass loss from the diagnostic damage model coupling, scaled by the total mass loss from the full model coupling. We show the final damage fields after 100 years for each of the three model couplings (f-h). The red line denotes the grounding line position.

This is a great point and one that we neglected to explain in detail in the original submission. It is true that the diagnostic damage model produces more damage than that shown in Sun et al. 2017 [12]. However, it is not a deficiency in the diagnostic damage model but rather a slight difference in the physics underlying both models. In the new Section 2.5, Reconciling the Diagnostic Damage Model with Other Damage Models in 2D, we will show that the diagnostic damage model and the model of Sun et al. can be derived as the same result, but the model of Sun et al. follows the Nye Zero Stress approximation assumptions of $\sigma_t = 0$ MPa and a stress criterion of the maximum principal stress (see Figure 1).

However, it's absolutely true that our implementation of the Sun et al. model still produces more damage than is shown in the original paper. This is now explained Section 3.1: Impact of damage production and evolution (lines 455-460). Both the diagnostic damage model and the model of Sun et al. represent damage accumulation with depth as occurring due to stress opening up cracks and ice pressure counteracting damage accumulation. However, [12] defines ice pressure counteracting damage accumulation as $p = \rho_i g z$, as the overburden pressure. This is consistent with previous studies (e.g. [4, 10]). However, our calculation of stress includes the horizontal normal stresses, such that $p = \rho_i g z - \tau_{11} - \tau_{22}$. This ultimately produces less pressure counteracting damage accumulation and, therefore, more damage. Therefore, this difference is not due to the form of the diagnostic damage model but just the physical parameterization of ice pressure. We've made this explicit in this revision.

You allow for your model to reach values of D up to 0.99 to avoid null denominator in the effective stress model and numerical stability and convergence issues. However, is $D = 0.99$ a realistic value for CDM. While $D < 1$ is a numerical condition to avoid infinite ice fluidity, I think that for too high damage values, especially when over large areas and deep into the column, the CDM really shows its limitations as we continue to simulate something that is far from being continuous as continuous. Later you mention in Section 3 that you set up $D_{max} \sim 0.8$. Can you precise if this is only for the MISMIP+ experiment and that you use $D_{max} = 0.99$ in the previous experiments?

Yes, $D_{\max} = 0.99$ for the flowline model and $D_{\max} = 0.8$ for the 2D simulation to ensure convergence with the numerical solver. This is now stated more clearly for the flowline model. As for what a physical value of $D_{\max}$ is, if we assume that D, in this case, is really representing the loss of stress-bearing ability of a representative volume of ice, then in theory it is true that D can reach values of 1 (or very close to 1) as the ice loses its ability to bear stress. Whether this is true for ice is not well-constrained, as far as we know. There are studies that suggest that ice should fail in a brittle manner at lower values of D, and some previous studies have parameterized this as a "critical damage" [5]. The specific relationship between fracturing and viscosity, however, has not been widely studied in ice (though is a direction for future work). However, this relationship between stress and the damage variable has a theoretical foundation in materials science principles and is widely adopted in damage studies in glaciology, so without another framework we have chosen to use the most widely-agreed-upon framework.

Line 230: I don't really understand the point of calculating the percentage difference in grounding line position ager 1 year for different δ. You then check the same error after a longer time (e.g., $10^2 - 10^4$ years) for which your diagnostic model is supposed to be more valid, which to me is a much better way to look at the ?error? of the diagnostic model.

This is a great point. We have rerun Figure 2 to 1000 years, rather than 1 year; see Figure 2 in this response.

Line 266: you mention healing as a form of damage sink in the advection equation. You might consider mentioning that the right end side of Eq. (1) is a ?damage source/sink? when you present the equation.

The text does mention that the right hand side of Equation 1 can be either a source or sink "f is the damage evolution function describing the rate of change of damage due to deterioration or healing". We

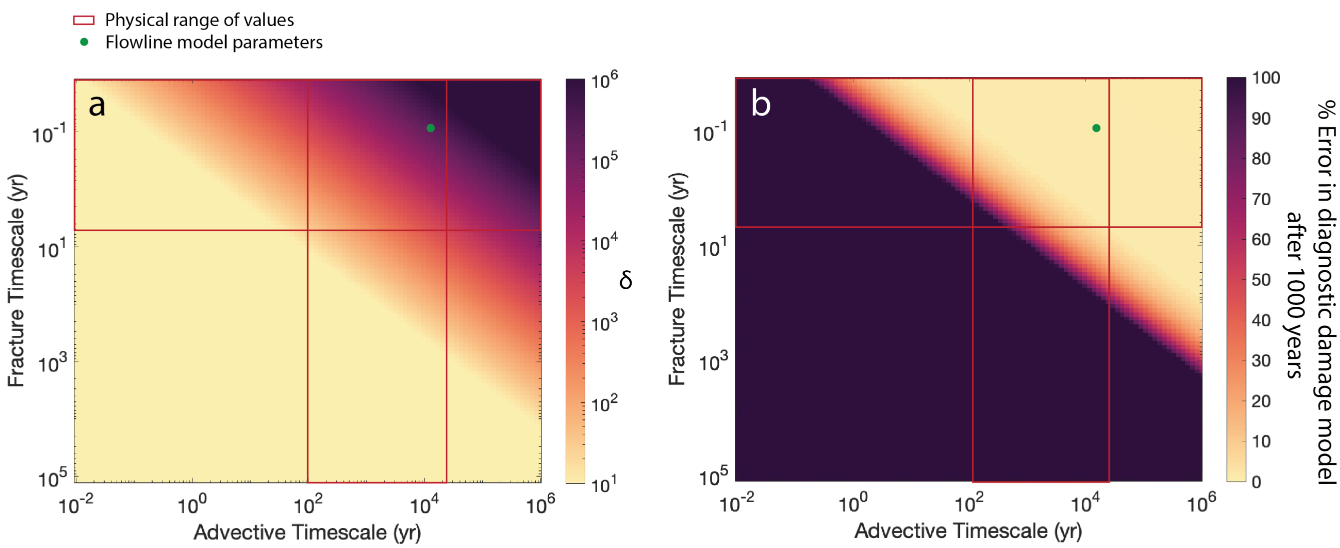

Figure 2: **Timescales for which the diagnostic damage model is applicable:** Parameter space of fracture timescale and advective timescale for (a) nondimensional parameter δ, (b) the error in the grounding line position using the diagnostic damage solver (the difference between grounding line position found from coupling the flowline model with the transient [11] damage model and the grounding line position found from coupling the flowline model with the diagnostic damage model, scaled by the total grounding line change in the transient model) after a simulation time of 1000 years. The red box represents likely range of values of advective and fracture timescales based on geometry of ice streams and the physics of fracture, and the green dot denotes the parameters used in the flowline model.

acknowledge, however, that this terminology wasn't consistent and that we continued to use "source term" for the remainder of the study. We have replaced all uses of "source term" with either "source/sink" or "damage evolution function" more broadly.

Line 286: Could you precise the resolution of the mesh in the vicinity of the grounding line?

This is now specified.

Line 310: I would add a comma: ?We note that in 3D, cracks are ...?

Done.

Lines 310?315 (this could actually be a third main concern): The model is Shallow Shelf (SSA) and therefore computes horizontal velocities considered constant over the ice column (plug flow). You can therefore assume null vertical velocities but this would greatly limit damage deep into the column (i.e., no vertical advection of the crevasses). You might also neglect ?significant? vertical stress components, potentially leading to inaccurate stress calculations.

Assuming mass conservation, you can recompute vertical velocities (based on surface and basal accumulation/melt and some assumption on the distribution) but you would need to assume a vertical distribution. Could you give more detail here?

Since a lot of the damage creates close to the grounding line where the SSA solution and the resulting stress computation is more prone to errors, what impact do you think the Stokes approximation has on the damage solution (you mention the role of longitudinal, lateral and shear deformation on crack closure later but not on the creation of crevasses/damage them/itself)? This would be interesting to discuss.

Thank you for this point; the ice flow model icepack does solve the SSA equations and therefore neglects some vertical stress components and the vertical advection of damage. Given how uncertain rates of vertical advection in ice sheets are, we believe that including vertical advection may add even more uncertainty than including it would. We agree that vertical velocities may be calculated from accumulation and basal melt, but the assumption of vertical distribution is a significant one and we are not aware of good constraints on this. Furthermore, the effect of ice overburden pressure may be to close cracks that advect into compressive stress states, and the mechanisms of crack opening and crack healing in compressive states is not well understood. Since most of the damage accumulation occurs on the ice shelves, where the SSA is likely to be most applicable, we believe the most significant effect may be to reduce the amount of damage on grounded ice by not accounting for vertical shear as a potential crack opening mechanism. We have discussed these assumptions in a new paragraph in the Discussion section.

Line 336: ?SUpplement? → ?Supplement?

Done.

Line 346: I suggest ?... and run the model for 100 years in two simulations?. instead of ?... and run the simulation for 100 model years in two simulations? .

Done.

Line 515: The blurriness of sharp cracks is also due to stabilization techniques for the advection equation. Stabilization techniques often rely on artificial diffusion, even for small timesteps and or small CFL numbers.

In this case, the blurriness of sharp cracks would likely exist even without any stabilization techniques due to the longer timestep size and significant increases in damage with each timestep. This will be explained more clearly in the revision.

Line 523: From this statement, I understand that there is no advection of damage in the column except for the fact that if the ice base is melted, a larger part of the column could be affected by damage? As I said in a previous comment, a part of the ?vertical advection? of the damage could therefore be due to a numerical vertical diffusion of the solution. I think this could be better discussed here.

This is correct, there is no vertical advection of damage in this model. There should not be any numerical vertical diffusion of damage in the model, as there are no “numerics” associated with the diagnostic damage model in 3D. It’s not obvious whether, if we incorporate vertical stresses, more of the ice column would be affected by damage. As mentioned in the reply to the previous comment, this would be a question of whether, once those cracks advect into a net compressive stress states, they would remain or heal. The assumption of SSA is now explored in the Discussion section.

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