RESPONSE TO REVIEWER 3

General comments

1) **It would be helpful to explain more about the advantage of the Maxwell-EB rheology compared with the traditional elliptic curve rheology. The authors pointed out the capability to represent the extreme localization of damage and deformation (P4L13). However, there is a possibility that it comes just from the horizontal resolution of the grid cell in the model. I mean that the traditional plastic rheology might be able to reproduce the phenomena if finer grid cells are used. Thus I want to know why they consider the continuum elasto-brittle rheology should be more appropriate than the continuum viscous plastic rheology for this phenomena, and whether this rheology can be applicable to the general sea ice conditions.**

The difference between the VP and Maxwell-EB rheology is two-folds: it lies in the rheology itself (i.e., the viscous-elastic-brittle versus the viscous-plastic constitutive relationship) and in the prescribed damage (or yield) criterion. Of course, both aspects impact the simulated mechanical behaviour.

First, the goal in developing the Maxwell-EB framework was to suggest an alternative to the traditional Viscous-Plastic (VP) rheology that is more physically sound, as in recent years, the viscous hypothesis and other underlying physical assumptions of the VP model have been revisited and found to be inconsistent with the observed mechanical behaviour of sea ice in many aspects, e.g., with respect to the order of magnitude of the observed strain rates (*Weiss et al., 2007*; *Rampal et al., 2008*), the anisotropic distribution of ridges and leads and associated discontinuities in the velocity field on scales both small and large (> 100 km) (*Hibler, 2001; Schulson, 2004; Coon et al., 2007*), the relation between stresses and strain-rates (*Weiss et al., 2007*), the strength of pack ice in tension (*Weiss et al., 2007; Coon et al., 2007*) and the normal flow rule (*Weiss et al., 2007*) . The aim in building this new continuum model was to represent accurately the *deformation and drift* of sea ice. In particular, we wished to developing a modelling framework that allows representing both the small deformations associated with brittle failure and the large deformations occurring within a fractured ice cover. In the paper, these points are introduced in the last paragraph of the introduction and in the first paragraph of section 3 (which presents the model). As they are discussed in the first paper that presents the motivations for and the details of the Maxwell-EB rheology (*Dansereau et al., 2016*) and references to this paper are included both in the introduction and in section 3, we do not think repetition is needed in the present paper, which we wish to be relatively short and to focus on the implementation of the model on geophysical scales.

One particularity of the Maxwell-EB model is that the localization of deformation indeed *does not* depend on the spatial resolution, *in the sense that the tendency to localize damage and deformation at the smallest available scale, i.e., the scale of the model grid cell, is intrinsic to the rheological framework.* In other words, no matter the spatial resolution, the Maxwell-EB model reproduces a localized deformation. This point is also discussed in more details in the paper that first presents this new rheology (see *Dansereau et al., 2016*, section 6.1) hence, an in-depth discussion was not included here. The fact that the representation of ice bridges and leads does not depend on the choice of spatial resolution (over the range of spatial resolutions that allow resolving the flow of ice through the channel) is mentioned in the description of the simulation setup (p. 12, line 18 and page 13, lines 1 and 2) and discussed in terms of the representation of the thickness redistribution (figure 10b).

We also discussed the issue of spatial resolution in our response to reviewer 2. As mentioned in section 4 and discussed in section 5.2, we analyzed lower resolution simulations. These simulations show that the model reproduces a stable ice bridge, a clearly defined ice front, arch-like and linear leads upstream of the ice bridge, and a distribution of ice thickness with a tail that follows an exponential function as in the higher resolution cases. Since we did not perform VP model simulations, in the paper we did not speculate on the fact that VP models at very high resolution can or cannot reproduce ice bridges in narrow passages. However, we believe that if a model can reproduce ice bridges and other important processes *only* at high to very high resolution it is not good news, as the physics represented by a model should not be resolution-dependant.

Second, the main advantage of using a Mohr-Coulomb (MC) failure criterion instead of the elliptical yield curve for the damage criterion in the Maxwell-EB model is that the MC criterion appears in agreement with in-situ stress measurements (see figure 8 a). Also in agreement with observations, the current damage criterion allows accounting directly for some resistance of the ice in pure ($\sigma_1<0$ and $\sigma_2<0$) tension. As demonstrated in the present as well as in previous papers (ex., *Dumont et al., 2009*), resistance of the ice in tension is especially important for simulating stable ice bridges. This point is discussed in section 2.1. Another advantage of the MC damage criterion is that the cohesive strength of the ice (i.e., σ_t and σ_c) can be set and adjusted directly by varying the cohesion parameter, *C*, rather than indirectly, by changing the ratio of the ellipse.

To answer your last comment, the simulations performed here are indeed on a regional scale, and concern very specific flow conditions. However, the results gives us no reasons to think that the model could not be applied to more general conditions. On the contrary, the model proves to behave well in this "extreme" case, i.e., a case chosen especially to test the ability of the model to represent (1) the complex mechanical behaviour associated with the formation of an ice bridge and (2) the discontinuities in ice velocity, concentration, thickness, etc., associated with the presence of this bridge.

2) **Intuitively my feeling is that the floe size distribution of sea ice should also play an important role in the brittle ice rheology. Therefore, in the question at P17L7-8 it would be natural that the change in floe size distribution may also contribute to the phenomena. What do you think?**
This is a good point. The paper discussed in this section (*Gimbert et al., 2012b*) identified a mechanical weakening of the ice cover that is independent of an ice thinning and suggested that this weakening is related to the degree of fragmentation of the ice cover. A more fragmented ice pack is indeed in agreement with an evolution towards smaller ice floes. We now add a mention to this effect in section 5.1.3, paragraph 2. It is also consistent with a change in the shape (circularity) of the floes, a less cohesive state ice cover, an enhanced deformation and an increased ice drift (*Rampal et al., 2009*).
Of course, continuum models by definition, whether using a VP, EVP, EB or Maxwell-EB rheology, do not resolve ice floes per se nor the mechanical interactions between individual floes. Hence it would be interesting to explore this question further using a discrete element model (ex., *Rabatel et al., 2015*; *Hopkins, 2004*; *Herman, 2011*; *Wilchinsky et al., 2011*), that is, to try relating the floe size distribution to the cohesive strength of the ice cove in a quantitative manner.

2) **On the whole, I am somewhat concerned about why the authors did not pay so much attention to the horizontal scale. For example, the scales of ice bridges seem to be different depending on the straits. Accordingly the mechanism might be different depending on the regions. Could you explain how the Maxwell-EB rheology influence the results depending on the scales.**
We are sorry we might not understand this comment fully.
The horizontal scale of ice bridges is the width of the constriction point across which it forms. In general, the limiting span that can support a stable arch between vertical walls or in a vertical tube depends on several properties of the material (its density, cohesion, internal friction) and the friction between the material and the walls (*Richmond and Gardner, 1962*). In the case of sea ice, the presence of a stable ice bridge should depend on the cohesion of the ice cover, its thickness, concentration, etc., the friction between the ice and the coast (here we prescribe a no-slip boundary condition), but also on the wind and ocean forcings. *Rallabandi et al., 2017* for instance developed a one-dimensional theory for the wind-driven formation of ice bridges in narrow straits in a VP model and investigated the formation of a stable ice bridge at a given wind stress, maximum and minimum channel width, ice thickness and compactness in this model. A study of the limiting span of ice bridges observed to form in the Arctic with a comparison to Maxwell-EB model simulations would indeed be interesting but is beyond the scope of the present paper.
However, as mentioned on page 10, lines 6 to 8, simulations with different idealized domains (narrower, longer channels, smaller basins) were performed to verify that the dynamics described in the paper is not specific to the shape and dimension of the idealized channel. Moreover, the use of a realistic domain allows investigating the formation of ice arches at different locations, hence with different spans, in the Maxwell-EB model.

Although the description on the scale dependence (P19L17-25) is interesting, in general it seems that the localization of deformation depends on the grid cell size. Could you explain why this property is independent of resolution?
As mentioned in our response to your major comment (above), the tendency to localize the damage and deformation at the smallest available scale is intrinsic to the Maxwell-EB rheology. Hence there is no characteristic scale for the localization of damage and deformation in the model beyond the scale of the model element (see *Dansereau et al., 2016*, sections 6.1 and 6.2). Therefore, at all spatial resolutions, the simulated deformation is highly localized. In the present simulations, this translates into a localization of the mechanically redistributed, i.e., the "ridged" ice and an exponential tail of the ice thickness PDF at the spatial resolutions explored (2 km, 4 km and 8 km in the idealized channel case). This point is now made clearer in section 5.2.

Specific points:
***(P1L2)"on geophysical scales" I wonder if we can assume ice bridges and ridges to be on a geophysical scale. It would be preferable to describe the specific phenomena like "ice bridges on a few tens of kilometers".**
The model is used here to simulate the drift of sea ice through a channel that is 500 kilometres long and a few tens to hundreds of kilometres wide. Ice bridges and ridges are smaller-scale features resulting from the associated deformation of the ice cover. We believe it would have indeed been wrong to claim that the model was used on *global* scales, but the setup used here does qualifies this application as to apply on "geophysical" scales.

***(Figure 1) The red dotted line in Fig.1b is hard to see. Please make it more prominent. In Fig.1c there are two red dotted lines. I guess the northern one should be deleted.**
Yes, thank you for catching this.

***(P5L17) Please insert "Hibler"**

***(P8L9, Eq.3) I think "A" is not needed.**
The air and water drag terms in the momentum equation are indeed both multiplied by the ice concentration. This approach was suggested by *Gray and Morland, 1994* and *Connolley et al., 2004*, to account for the contribution of the ice-free and ice-covered fraction of a grid cell to the wind and water stress. *Connolley et al., 2004*, explains the necessity of introducing this weighting to maintain physical consistency in the free-drift limit. Without it, the free-drift solution of the momentum equation (when including the Coriolis term)

depends on ice concentration, i.e., ice floes with the same thickness would not be drifting at the same velocity based on their concentration, even in the limit of negligible mechanical interactions. Here, this "correction" is included for the sake of physical consistency, even if not strictly necessary since the Coriolis term is neglected in the present implementation of the model. We now add a reference to the work of Connolley et al., 2004 when introducing the form of the momentum equation solved here (Eq. A1).

This weighting approach is quite standard and was used for instance in the sea ice models of *Tremblay and Mysak, 1997*, *Lieataer et al., 2009*, *Danilov et al., 2015* (FESIM), and others. Interestingly, in the present model, it has effectively little effect on the simulation results, a point also noted by *Connolley el al., 2004* and *Tremblay and Mysak, 1997*.

***(P17L4) Please replace "than" by "that".**
Yes, thank you.

***(P19L10) I agree, but there are some discrepancies in the slope of the thickness pdf around 1 m. Is that a negligible problem?**
This discrepancy is explained by the fact that a uniform thickness of h = 1.0 m is prescribed as the initial condition in all simulations presented here. Hence we naturally expect a mode to stand out at h = 1.0 m. The tail of the PDF, which represents the ridged ice, is therefore the part of the distribution with h > 1.0 m. Here, the PDF was effectively fitted with an exponential function for all values of h > 1.0 m. The presence of the mode indeed results in a systematic misfit near h = 1.0 and fitting the distribution for larger values of h only gives a somewhat better fit. Nevertheless, the values of the coefficient for the goodness of the fit obtained here vary between 90% and 98% in the idealized and are > 95% in the realistic case.

***(P19L18) In the equation, h cdot nabla u should be h nabla cdot u.**
Yes, thank you for catching this.

***(P21L11-12) "prescribing a cut-off for biaxial compressive strength. . . appears unnecessary" I could not understand this. Can you add some additional explanation?**
As suggested both by in-situ stress measurements (see figure 8a) and the realistic numerical simulations performed here (see figure 8b), large biaxial compressive stresses seldom occur in the sea ice cover. This is an interesting result, since the flow conditions here are convergent over a large part of the domain. The stress states measured and reproduced by the model indicates that the ice fails frequently under pure tensile and biaxial tensile-compressive (i.e., shear) stresses (which is also illustrated in figure 5c). This point is further discussed in the response to reviewer 2.
Because large biaxial compressive stresses and pure biaxial compressive stresses, i.e., compressive states of stress involving little shear ($\sigma_1 \sim \sigma_2$), are marginal, imposing a biaxial compression damage criterion, would not significantly affect the number of damage events and propagation of damage in the Maxwell-EB model. The addition of such a cutoff is not supported (and not well constrained) by the observations. Instead, in-situ stress measurements suggest that the uniaxial (unconfined) compressive strength, σ_c and maximum tensile strength (or σ_t) are more relevant parameters to describe the failure strength of the ice cover.

To make this point clearer, we modify this paragraph as follow:
"Besides numerical efficiency, other advantages of using a simple redistribution scheme such as the one employed here is that no thickness redistribution function needs to be assumed and the redistribution is not directly tied to the prescribed failure strength of the ice. In the Maxwell-EB model, the prescribed strength is instead based on in-situ stress measurements, which point to a Mohr-Coulomb failure criterion and directly provide information on the order of magnitude of the shear strength and tensile strength. In particular, both the observations and numerical simulations here suggest that prescribing a cut-off for biaxial compressive strength (equivalent to the pressure, *P*, in VP models) is unnecessary. Instead, the uniaxial (unconfined) compressive strength, or sigma_c and maximum tensile strength, sigma_t appear to be more relevant to represent adequately the strength of the ice cover. The Maxwell-EB model presents the advantage that both these quantities are set through a single parameter, the cohesion *C*."

***(P23L10) "Hibler" is missing.**