RESPONSE TO REVIEWER 1

Minor and Technical Comments

Abstract, first sentence. You need to tell the reader what "EB" is and what the model does before launching into a discussion of its implementation. Like this: "The Maxwell Elasto-Brittle (EB) model uses a sea-ice rheology that allows tensile stress... blah blah [one sentence about the model]. This paper presents a first implementation of the..."

This sentence is now added to the abstract: " This continuum model, called Maxwell-Elasto Brittle (Maxwell-EB), is based on an Maxwell constitutive law, a progressive damage mechanism that is coupled to both the elastic modulus and apparent viscosity of the ice cover and to a Mohr-Coulomb damage criterion that allows for pure (uniaxial and biaxial) tensile strength."

Abstract, lines 6-8. This sentence is a bit awkward. I suggest: "In agreement with observations, the model captures the propagation of damage..." etc. (and delete "are all represented" at the end of the sentence).

Agreed: we made the suggested modifications.

Abstract, last sentence. "weakening of the ice cover" and "shorter lifespan of ice bridges" – over what time period? You should add something like "in the 2000s relative to the 1980s and 1990s". And then "with implications in terms of increased ice export" – say that this would be expected because sea ice is expected to continue thinning in the future. In fact, maybe this is a positive feedback on the loss of ice: as it weakens, it drains from the Arctic faster, which weakens it further.

We are not specific about a time period for these two processes in the abstract because they have not been deduced to occur at the exact same time: the weakening of the ice cover estimated by *Gimbert et. al. 2012b* occurred over the period 2002-2008 relative to the period 1979-2001 while the shorter lifespan of ice bridges reported by *Barber et al., 2001* occurred in the 1990s relative to the 1980s (see section 5.1.3). The formulation used in the abstract reflects the aim of the experiments presented here, where is to relate in a general sense a mechanical weakening of the ice cover to a shorter lifespan (or absence) of ice bridges and increased ice export, i.e., not to reproduce a trend in the lifespan of ice bridges observed in recent years. Moreover, we do not believe that speculating about a future ice thinning is relevant at this point in the paper: actually, the numerical experiments presented here aim to relate a shorter lifespan of ice bridges and increased ice export to a *genuine mechanical weakening* of the ice cover, i.e., a weakening that is *independent* of a decrease in ice thickness. Mentioning an expected thinning of the ice cover in future years in the abstract would turn the attention away from this important point.

Page 1, line 15. "expenses" should be "expanses"

Page 1, line 21. "May 2005" should be July 2010.

Page 2, line 12. Change "allowed demonstrating" to "demonstrated"

Page 2, lines 18-20. "shorter lifespan of ice arches" and "increased ice export" – over what time period?

Here we added the reference time period for the mechanical weakening reported by *Gimbert et. al. 2012b*. To clarify the point made in the response to your earlier comment, we modified this sentence as follow: "we also discuss how the mechanical weakening of the ice cover estimated over the period 2002-2008 relative to the period 1979-2001 (*Gimbert et al., 2012b*) can be linked to a shorter lifespan of ice arches and consequently, to an increased ice export through Nares Strait".

Figure 1. The dates on the images are in the format DAY-MONTH-YEAR. Is that standard for The Cryosphere? It might confuse U.S. readers, who often use MONTH-DAY-YEAR.

Yes, DAY-MONTH-YEAR is a standard for the *European* Geosciences Union.

Figure 1, caption. Put "(MODIS)" before the word "reflectance", not after.

Page 4, line 10. 0.32 degrees is not 4-10 km.

There was a word missing there: 0.32 degrees *of longitude* is indeed equivalent to 4 km in the Lincoln Sea and 10 km in Baffin Bay (taken from *Rasmussen et al., 2010,* page 163: "The model horizontal grid has a longitudinal grid size of 0.32° resulting in a grid size ranging from 4 km in the Lincoln Sea to 10 km in the Baffin Bay"). We now remove the mention to degrees and give the resolution in kilometres only to avoid any confusion.

Page 4, lines 21-24. For the statement about the sea-ice thickness PDF you can cite: Lindsay, R. W. 2013. Unified Sea Ice Thickness Cli- mate Data Record, 1975-2012. Boulder, Colorado USA: National Snow and Ice Data Center. http://dx.doi.org/10.7265/N5D50JXV. Web site: http://nsidc.org/data/docs/noaa/g10006-unified-sea-ice/

Thank you for this reference.

Page 5, line 3. Change "pioneer" to "pioneering"

Page 5, line 9. "dubiously" – Why it is dubious to base the modelling framework on energy conservation?

Basing the estimation of the strength of the ice cover, i.e., equating deformational work to the sinks of energy that are taken into account in this framework is dubious in the sense that all of these sinks are very hard to estimate, and do not account for other processes such as ice crushing, buckling, frictional contacts between ice blocks in the rubble pile, etc. However, we agree that the use of the adjective "dubious" is unnecessary and nonobjective. We rephrase this passage in an clearer and more objective manner as follow:

"The relation between the redistribution process and the strength of the ice (often characterized by a pressure, *P*) in this modelling framework is based on energy conservation principles : the deformational work is equated to the work done in building ridges, which is partitioned between potential energy changes (*Thorndike et al., 1975*), the frictional dissipation in ridging (*Rothrock, 1975*) and dissipation in shearing deformation (*Pritchard, 1981*), all of which are very hard to estimate. This theory does not take into account other mechanisms such as crushing, buckling, flexural breakage, inelastic contacts and frictional sliding contacts between rubble ice blocks (*Hopkins, 1998*)."

Page 5, line 18. There is an error in the citation of Hibler's 1980 paper. William D. Hibler III should be "Hibler", not "III". Also on page 23, line 10, the author is Hibler, not III.

Yes, thank you for catching this.

Page 5, line 19. "sensible" should be "sensitive"

Page 6, line 5. Instead of "recall", better to write "review"

Page 6, line 7. Instead of "passage", I think "connection" is the intended meaning

You are right, "passage" might not be the right word: we changed it for transition.

Page 6, line 9. After "elastic modulus" write "(E)" because it is used three lines later in the definition of lambda.

Page 6, equations (1) and (2) – you need to say that "mu" is the internal friction coefficient.

Yes, thank you.

Page 6, lines 27-28. About healing: healing allows the level of damage to DECREASE – more healing, less damage. But the damage parameter d INCREASES as healing increases (d=1 is undamaged). So you have to be careful when describing healing. Verbally, healing means a SMALLER AMOUNT of damage. But numerically, healing means a LARGER VALUE of d.

The formulation in lines 27-28 is correct: the *level of damage*, *d*, has been introduced in lines 15 and 16. It is not employed the same as "damage". Here it is specified that the *level of damage* re-increases at most to the (numerical) value of 1.

Here I add "the level of damage *variable*" to make the distinction clearer.

Page 7, Figure 2, caption. Several things: – It is awkward to have complicated equations in the caption. Furthermore, the components sigma11, sigma12, and sigma22 are not used anywhere else in the paper, so I think it would be OK to delete the equations entirely. Otherwise, they should probably go in the main text. That's just a suggestion, there is nothing wrong with the current presentation. – Second sentence: "The thin solid lines represent the damage criterion in the case of C=0." To make this clearer, consider: "The thin solid lines radiating from the origin represent the damage criterion in the case of no cohesion (C=0)." – What is the shaded region in the figure?

Agreed: the equations are now removed. We also included your comment about the cohesion. The shaded region is also removed: it has actually no meaning relative to the simulation results presented here.

Page 8, line 7. Change "assimilated to" to "simulated as"

OK

Page 8, line 27. I think max[0,(1-A)] should be max[0,A-1]. This is supposed to describe "the excess concentration" when A > 1. If A > 1 then max[0,1-A] = 0, which doesn't make sense. The excess concentration should be max[0,A-1].

Yes, thank you very much for catching this: this is a typing mistake and the right form is indeed given in Eq. 6.

Page 9, equation 7. This equation doesn't make sense either. When A > 1, h+ = 0. I think this equation should be h+ = max[0,A-1]h.

Yes, the same mistake was repeated there. We however insist on the fact that this term is implemented in the correct form (h+ = max[0,A-1]h) in the code (see response to reviewer's 2 comment).

Page 9, line 18. Change "of being" to "to be". Change "sturdy" to "steady" Page 9, line 19. Capitalize "Lincoln Sea"

Page 10, line 27. Acceleration and advection terms are neglected in equation 3. Does this mean the solution is steady state, with no time dependence?

No: in the originally submitted version of the paper, the acceleration and advection term were neglected only in the momentum equation. The time derivatives and advection of all other variables (σ, h, A, C, d) were accounted for. Hence the solution was therefore not steady state. However, as mentioned in our response to reviewer 2, and as expected from a dimensional analysis of the momentum equation (see *Dansereau et al., 2016*, section 4.1.4), including the advection and acceleration terms in the momentum equation does not affect the results and conclusions of this paper. In the corrected version of the paper, both terms are included in all simulations (which needed to be run again due to an error in the thickness redistribution scheme, see our response to reviewer 2). This does not impact the results.

Page 11, Figure 3 caption. Delete the first "(a)" because the caption later refers to (a) and (b).

Page 11, line 11. "quenched disorder" – this needs a reference.

You are right, and more precision was needed on this point. This paragraph is now modified and this sentence is added after line 15 : "In all simulations, the disorder introduced in the field of cohesion is quenched (*Hermann and Roux, 1990*): it is set once, at the beginning of each simulation, and is passively advected with the ice flow".

Page 11, line 16. Beaufort SEA. Also, Weiss et al (2007) is cited in connection with Figure 8(a), but the figure caption cites Weiss and Schulson (2009).

Weiss et al., 2007 are cited in connection with the in-situ stress measurements in the Beaufort Sea, but figure 8(a) does show figure 13(a) from *Weiss and Schulson, 2009* (which presents the same measurements as in *Weiss et al., 2007*, figure 2A, but with the convention that compressive stresses are positive, as in the present paper). The reference to *Weiss and Schulson, 2009*, is now added in line 16 to make the connection to figure 8(a).

Page 12, line 10. Delete "the upstream part of", so the sentence reads, "...prescribed on GAMMAin and GAMMAout..."

Page 12, lines 14-15. "The equations of motion... Galerkin methods are used to handle advective processes" – but the advection terms in equation 3 are neglected. So I don't understand this. Does it refer to advection in equations 4 and 5?

The advection of ice momentum only is neglected here. All other variables are advected with the ice flow.

Page 14, line 2. "deformation rates" – I think this should be "high deformation rates"

Yes, thank you.

Page 14, line 3. "downstream of the channel" should be "downstream of the constriction"

No: here we precise "in the interior" of the channel, hence downstream of the constriction point, and "downstream of the channel", i.e., of the opening point.

Page 14, Figure 4. This figure should be bigger, so that it fills the full width of the page. It's a bit difficult to see the peaks of wind stress in panel (a) – bigger would be better.

Agreed: this figure is now resized.

Page 15, line 23. I think "Fig 4b and 4c" should be "Fig 5b and 5c" Page 15, line 27. I think "Fig 4b" should be "Fig 5b"

Thank you for catching this.

Page 16, line 24. The text says "Weiss et al (2007)" but the Figure 8 caption says "Weiss and Schulson (2009)"

We changed the reference there to *Weiss and Schulson, 2009*.

Page 18, line 2. "ice landfast" should be "landfast ice"

Page 18, line 8-9. How does the ice become weaker without thinning? Is it because the ice strength is temperature-dependent, so increasing the temperature would make it weaker? Is it because the composition of the ice could change, such as increased salinity?

Here, the mechanical weakening refers to a more fragmented/fractured ice cover (*Gimbert et al., 2012b*), and so (as mentioned by reviewer 3) an evolution towards smaller and different shape (more circular) ice floes. This point is now made clearer in section 5.1.2 (page 17, line 12, see response to reviewer 3). We neglect temperature and salinity effects.

Page 23, line 10. The first author is Hibler, W.D. III Page 25, line 27. Weiss and Dansereau 2016 – is the DOI correct?

Page 26, Figure 5 caption. At the end of the last sentence, "red arrows on (a)" should be "red arrows on 4(a)"

Page 27, Figure 6 caption. Delete the first "(a)" because the caption later refers to (a) and (b).

Page 29, Figure 8 caption. Delete the first "(a)". Capitalize "Beaufort Sea" C5

Page 31, Figure 10 caption: – line 2: "is of 10" should be "is 10" – lines 5 and 6: "insert" should be "inset"

I recommend deleting these two sentences: "The tail of the distribution..." and "The temporal evolution..." because the last sentence of the caption explains the same thing as these sentences.

Agreed, these sentences are removed.

Page 31, Figure 11 caption:

- **line 2: "is of 10" should be "is 10"**
- **line 5: "insert" should be "inset"**

Agreed, and the repetitions mentioned in your previous comment were also removed from this caption.

Please note: I have not flagged all the minor grammatical corrections that should be made by an editor, for example:

Page 9, line 10. "is of 100%" – delete "of" Page 9, line 26. such AN ice bridge Page 10, line 8. "in Cartesian" should be "to Cartesian" Page 10, line 9. Delete "is" Page 11, line 8. employed IN the simulations Page 11, line 12. "by randomly by" – delete second "by" ...and so on...