In this study the authors combine remote sensing observations and numerical modeling experiments to assess the potential impact of the loss of sea ice buttressing on the flow and stability of glaciers in the Larsen B Embayment. Essentially, they find that sea ice is unlikely to provide a direct control on glacier flow and stability, though they suggest that it can have indirect effects. Overall I think the paper is pretty easy to read and the results seem robust and interesting. That said, I do think the paper would benefit from moderate revisions. Here are a few general comments:

Structure: The paper is written in the way that one might tell a story, which isn't necessarily bad, except that I find it a little jarring to go back and forth between methods and results (particularly in Section 2). I understand that the methods in this section are relatively basic, but I still think a different structure here is warranted. Perhaps start with a paragraph or two that that describes all of the data sets and how you analyzed them before getting into a description of the observations. It also seems that there are a couple of other papers out there discussing similar observations. Make it clear how this study is different or complementary.

Terminology: The authors use the expression "floating ice/melange tongues". I don't know what this is referring to. "/" typically means "or", so is this "floating ice" or "melange tongues"? And what is meant by "melange tongues"? And later, is "ice tongue" really referring to an "ice shelf"? At least in the Antarctic context, when I hear about ice tongues I usually first think of something like the Drygalski Ice Tongue, which is not bounded by fjord walls.

Sea ice model: I have some concerns about the authors use of a viscous flow model to describe stresses in sea ice, and I'm not sure that I follow why this model should provide an upper bound on the buttressing stress. At the same time, I think the authors could use their observations and some simple arguments to support their conclusion that sea ice buttressing is not directly important unless the sea ice is tens of meters thick—which would also help to back up their model results.

Whether you invoke a viscoplastic or purely viscous rheology, the depth-averaged tectonic (or resistive stress) should scale with the depth-averaged strain rate:

$$R_{xx} \propto \dot{\epsilon}_{xx}, \tag{1}$$

where I am taking x to be perpendicular to the glacier or ice shelf face and the tectonic stress is related to the Cauchy stress by

$$\sigma_{xx} = R_{xx} - P, \tag{2}$$

with P the depth-averaged glaciostatic pressure. The force per unit width acting on the glacier face is then

$$F/W = -H\sigma_{xx} = -HR_{xx} + HP. \tag{3}$$

To get the buttressing stress, you need to subtract the force from the depth-averaged water pressure P_w, which would also act on the glacier if the sea ice was removed. This gives

$$F/W = -HR_{xx} + HP - HP_w = -HR_{xx} + \frac{1}{2}\rho g\left(1 - \frac{\rho}{\rho_w}\right)H^2. \tag{4}$$

The reason that it's not clear to me that a viscous model will provide a maximum bound on the buttressing force (as stated in line 145) is that I don't know how a viscous rheology will affect R_{xx} compared to a viscoplastic rheology.

Nonetheless, the sea ice flow seems to be extensional in the observations, implying that R_{xx} is positive. In other words, the last term in Equation 4 would seem to provide a good estimate of the upper bound on the sea ice buttressing force. Unless H^2 is large, this force will be pretty small.

Perhaps it would be interesting to compare the modeled buttressing force to the quasistatic force (i.e., when $R_{xx} = 0$).

One advantage of framing the discussion around something like Equation 4 is that it doesn't very strong assumptions about the rheology (e.g., which may be inconsistent with sea ice literature). You can also look at the field observations to get an idea of the forces involved without worrying about the details of the rheology. Only if the flow is highly compressive would I expect to see large forces. Perhaps that is happening at scales that you can't resolve in the satellite data, but then I'm not sure that they would be resolved in a viscous flow model either.