First off, I disagree strongly with the statement that the ice thickness decline slowed down during CS2, there is no evidence of this, especially given the use of snow depth climatology (see Mallett et al. in TCD). All your statements about CS2 thickness variability should be stated with a caveat in that this assumes no interannual changes in snow depth, except as represented by first-year vs. multiyear ice. Thus, because this paper hinges on the 2011 minimum sea ice thickness as measured by CS2, then you will have to address the use of snow climatologies in these thickness estimates which are assimilated into your model. I'm also concerned about the lack of discussion on snow cover in general which plays an important role on thermodynamic ice growth as well as the timing of when bare ice and melt ponds form. Does the model not simulate any snow? The entire description of the modeling framework is too vague for this study. While references are given to the CMST model, you need to at least include some basic information such as resolution, atmospheric forcing data, etc. The entire methods section is weak and not suitable.

I also found the conclusions drawn often not supported by the data. In fact, the largest amount of ice in terms of area was not lost in 2011, and since you are further arguing that the ice was thinner, there is no way that you had the largest volume loss. The manuscript suffers from many of these types of inconsistencies, and vague statements without supporting evidence. Sadly I cannot recommend this paper for publication. It does not add any value to our understanding of processes in the Arctic, nor does it accurately portray the factors contributing to the "supposedly" anomalous thin ice in 2011.

Some specific line comments
Line 84: I don't believe you are using any method to track ice age, you are using a known data product and it should be stated as such

Line 89: you should also be aware if ice is advected towards the coast in the ice age product, the ice is lost (i.e. is transported onto land) so there will be a bias.

Line 90: you are not estimating anything here, instead you are using data from ERA5, unless this statement pertains to anomalies but then you need to specify how the anomalies are computed (i.e. relative to what reference period).

Line 113: I don't follow why you are only using a 6-year mean, you have a longer time-series and you should use it.

Line 125: I don't follow why you get enhanced winter melting in this region. I don't believe your residual term is entirely made up of thermodynamic processes, and I do not believe you have anomalous freshwater flux during this time. Where is the evidence for this? I think you are stretching your interpretations too far without the physics supporting these statements. What were your ocean and atmospheric temperatures in that region during that time?

Line 127: this is nothing new, divergence will result in thermodynamic ice growth that acts as a stabilizing feedback and there are many references the authors could cite about negative feedbacks. Further, the thickness of the ice to start the growth season also plays an important role in this feedback process, and none of this is discussed. There are also two recent papers suggesting that the thermodynamic ice growth may be slowing, one by A. Petty (GRL) using climate model simulations and one by J. Stroeve (TC) using CS2 data in CICE.

Line 132: you cannot simply state that increased melt was driving by atmospheric temperature net surface heat flux and other variables. That is vague and uninformative. There are numerous factors that play a role in melt, including the timing of ice retreat and opening of leads/open water areas between the ice floes. You should at least try to quantify the relative contributions.

Line 145: I do not believe your assessment of enhanced ice export out of Fram Strait from October 2009 to January 2010 as it doesn't really match with my own calculations from at least 1 December through end of January. It is actually the second lowest amount of volume flux through the Fram Strait.

Line 177: More sea ice lost in 2011 than any other year? Again you haven't specified over what time-period this analysis is being done for, and it would be good for you to put this into the context also of total ice area lost. If I compute the total ice extent lost between the maximum and minimum for each year, the maximum loss in total ice extent during summer is 2012, not 2011. And in fact 2011 is not even the second highest amount. If you are also arguing that you had thinner ice in 2011, then there is no way that you had more sea ice lost in 2011. Since this is an incorrect statement I didn't finish reading the rest. The entire paper is currently flawed, making statements that are not supported by the observations or the data used.