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Interactive comment on "Landfast ice thickness in the Canadian Arctic Archipelago from Observations and Models" *by* S. E. L. Howell et al.

Anonymous Referee #2

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1 General comments

This paper presents an analysis of time series of land-fast ice thickness and related variables from four locations in the Canadian Arctic Archipelago (CAA), obtained from both observations and models. The observations presented are very interesting, giving potentially a unique view of the evolution of ice thickness in the CAA, as well as some insight into the climate and climate change in the CAA. The data, results, and conclusions are not ground breaking or spectacular, but can serve as a solid addition to our knowledge of the Arctic. The authors also use the observational data to evaluate model results. This is a useful exercise, but still needs some work.

The paper is mostly clear and concise. The authors' approach in working with the data

and models is also good in general. There are a number of points that need to be addressed before I can recommend publication, but I am confident that the authors can address those in a satisfactory manner. Given that I expect the resulting paper to become a good, relatively low-key, but solid addition to our understanding of the CAA climate.

My main concern regarding this paper is with the conclusions the authors draw at the end. These are too often poorly supported by the data, or even not at all. In some cases the discussion is lacking so that it is not clear what conclusions to draw. The most confusing aspect is the role of snow and temperature, where it is not always clear when we should be thinking about de-trended correlations or correlation between trends. It is also not clear how you calculate the correlation between maximum ice thickness and temperature and it's not immediately clear how this should be done (do you want to use a mean temperature over the growing season for instance?)

In particular, in the first paragraph of the conclusions the authors state that "[e]ven though warming is seen at all sites, changes in ice thickness is also attributable to variability in snow depth, which plays a dominant role in controlling the inter-annual mean and variability of ice thickness". Here the authors appear to be mixing the long-term trend in temperature (the warming) with the inter-annual variability, which they show is highly correlated with snow thickness (the de-trended correlation is correctly used for this). It seems to me that the inter-annual variability in maximum ice thickness is controlled by variability in snow thickness and that the long-term trend is caused by a long term trend in temperature. But I'll come back to this point below.

The authors then go on to say "[w]ithin the CAA, increases in snow depth are contributing to decreased trends in maximum ice thickness at Eureka and Alert but this far appear to be exerting less of an impact on maximum ice thickness at Resolute and Cambridge Bay". I don't understand how the authors reach this conclusion. The trend in snow thickness is only significant at Cambridge Bay so we can safely assume that only there can a trend in snow thickness contribute to a trend in ice thickness.

Turning to the relationship between ice thickness and temperature, in paragraph four of the conclusions the authors state: "the significant correlations at Resolute, Eureka and Alert suggest that the higher sensitivity to changes in snow depth could easily mask the warming signal on both fast and offshore ice". I don't really feel this has been shown in the paper. I very much expect this statement to be true, but it needs to be better argued.

The authors then go on to say "[t]he dependency between ice thickness trends and warming trends is only weakly present at Cambridge Bay ($r = 0.4$)", but in the text it is clearly stated that the given r-value is related to the de-trended correlation and has as such nothing to do with the thickness and warming trends.

My conclusions, after reading the paper (and not your conclusions) are that

- Snow thickness is the main driver of inter-annual variations in maximum ice thickness (high de-trended correlation at two out of three sites, and reasonably high correlation at the other two).
- Inter-annual variations in air temperature are only weakly correlated with maximum ice thickness (max $r = 0.4$).
- A trend in air temperature is the main driver of the trend in maximum ice thickness. This is because there is a significant warming trend at all sites, but only significant snow thickness trend at one.

In short the inter-annual variations in maximum ice thickness are caused by variations in snow, while the long term trend is caused by a long term trend in temperature. The fact that there is no ice thickness trend at Resolute is probably related to the timing of snow fall, but this is only mentioned once in the paper and not explored in any depth. The authors conclude that change in snow thickness, not temperature is the driver of both inter-annual and long-term changes, but I can only agree with the former part of that statement, not the latter.

Now, I don't have access to all the data and haven't spent much time on the analysis, so maybe I'm completely wrong and the authors right. But if this is the case then the authors need to make a more convincing point in their presentation of the results and conclusions.

In addition I'm also concerned over sections 4.2, and 4.3. First of all, the ORA-IP models are all run over a different, and much shorter time period than is covered by the observations. I therefore doubt that it is appropriate to use those to calculate trends that should be compared to the observed trends. In addition the first paragraph of that section left me very confused. You propose comparing figures 2 and 12, but the former shows thickness seasonal cycle, while the latter shows thickness trends. You talk about a general pattern, but this is too vague and qualitative for my taste. Also, I'm not sure there is a north-south pattern in the observed trends, we can at least not see this from table 2. In short, it seems to me you should focus on CMIP5 for the trend analysis because the ORA-IP periods are too short. It could be OK to use the longest runs, i.e. CGLORS, ORAP5.0, and PIOMAS (that runs from 1979, right?), but be careful.

2 Specific comments

l. 25 There is hardly a need for a reference to what land-fast ice is, but if you want one I would recommend the WMO definition. On the other hand, I would like to see a reference for the statement "this ice typically extends to the 20–30 m isobath". It is right, but should be backed up by some references (I think Hajo Eicken recently published a nice overview and you already have Mahoney et al. in your reference list).

l. 101 Here you say that the CMIP5 runs extend from 1980 to 2099, but later you say that you use the period 1955–2014. But doesn't the historical run go back to 1850?

l. 117 I would add PIOMAS to table 2 for completeness.

l. 162 You can do better than suggesting a shortened growth season. In the conclusions you cite Howell et al. who show a later onset of melt and if you combine this with your results you are conclusively demonstrating a shortened growth season.

l. 171 You should not tell us which trends there are, only to then tell us that they are all insignificant except one. In fact you should only discuss the significant trends and if you want to mention the others then don't spend to much time on them.

l. 179 Here you say that there is statistical correspondence between the ice and snow thickness trends at Eureka and Alert, but the snow thickness trends there are not significant. This does not make sense to me. Also, it is not clear what you mean by "statistical correspondence".

l. 186 I couldn't understand the sentence starting on this line. Please rewrite.

l. 207 For completeness you should note Parry Channel in figure 1. But I'm not sure about this condition, since ocean and ice dynamics presumably play a small role here. If you want a more quantitative measure than "a reasonable representation of the CAA" then I would suggest putting a limit on the resolution in the CAA.

l. 215 Picking single points to analyse is always a bit questionable. You should make sure, and state that the point selection does not affect your results, as long as the points selected follow your selection criterion. I suppose that there were more than one candidates for each model and location. Also, some of the model points are quite far away from the observation station. I noted especially points representing Alert and Cambridge Bay which are so far away from the stations that I would expect them to belong to a different atmospheric and oceanographic regime (see figure 12f).

l. 221 Here you say that the seasonal cycle is calculated over 1955–2014, but this does not fit your earlier statement about the CMIP5 models or the information in table 2.

l. 235 It is not clear to me why this statement should be true. Please elaborate.

l. 264 Which models are the "high resolution models"?

l. 276 In table 2 the trend is 4.3 ± 1, not 6.2 ± 2.4. There is also an inconsistency in signs in this paragraph.

l. 289 If the trends are not significant then we can't learn much from them.

l. 295 Again, trends that are not significant don't warrant much of a discussion.

l. 301 Don't talk about the insignificant trends as if they matter.

l. 324 This paragraph doesn't belong in your conclusions since it contains nothing from the current work.

3 Technical corrections

l. 20 Replace "two magnitudes" with "two orders of magnitude".

l. 131 Replace "2.27 m that is likely ..." with "2.27 m, which is likely ...'

l. 169 Here it sounds as if it's the linkages that are summarised in table 3, but this is not the case.

l. 176 The r should be italicised and the equations would look much nicer if the were within dollar signs (assuming you're using latex). I.e. $r = -0.66$, instead of r=-0.66.

l. 193 Replace “than the earlier period” with “from the earlier period”.

l. 213 Replace “good portion of year but not all year” with “good portion of the year, but not all year”.

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