

Interactive comment on “Rapid decline of Arctic sea ice volume: Causes and consequences” by Jean-Claude Gascard et al.

Jean-Claude Gascard et al.

jga@locean-ipsl.upmc.fr

Received and published: 26 March 2019

Response to Referee 2 Let us first respond to specific comments Title : “Need to take out consequences”? We agree and will delete not only “consequences” but also “causes” in the title. Line 37/38 “Arctic sea ice decline is one of the most obvious effects of climate change” and “not the most representative”. We agree. This is what we meant. Line 38/39. “Incomplete sentence”. We will delete it or rephrase it. We just meant that space observations were very instrumental for a better understanding of what is going on in the Arctic. Line 41-42 “Missing closing parenthesis”. Sorry about that. We will correct it. Line 43-45. “No citation for the 2007 minimum?” Yes we have 4 citations for the 2007 minimum (lines 47 and 48). We will move them from lines 47-48 up to lines 43-45. Line 45-46. The referee suggested to remove the sentence regarding

C1

IPY and the unprecedented amount of new results regarding Arctic sea ice. No problem we will remove this sentence since it was not our intention to make a complete survey of IPY “new results.” The referee is right “it has to go in a scientific article” but this was not our main objective. Line 47-48. “Those citations need to go” and “no scientific reason for these citations”. This is in contradiction with the question raised line 43-45 by Referee 2 for the 2007 minimum that occurred precisely during the IPY. We could certainly move these citations from line 47-48 up to line 43-45. Line 49-50. Zhao et al (2018) cited in the last reference lines 634-636, mentioned “Record low sea ice concentration in the central Arctic during summer 2010.” We could certainly better introduce this citation in the text line 49-50. The word “summer” was missing there. Line 50. Please change “the whole time record” to the “absolute record”. OK we will do so. Line 53-55. “unclear sentence”. “please revise”. We will remove this sentence. It does not bring anything to the text. Line 56. Remove “peculiar and intriguing”. OK we will do so Line 63. “Who did this?” In fact we contributed to it but not only us of course and we can provide an extensive list of who contributed in introducing Arctic sea ice volume. Most of these contributors are listed in the references. No problem we will clear up this point. Line 78-89 and line 91. “This is not an introduction”. Referee 2 is correct. We will restructure this part of the text separating what is the real introduction from what is something else (such as methodology and results). So we do agree with the referee’s comment. Line 91-92. “Needs a reference for PIOMAS right here at first mention, also needs to write out the full name for PIOMAS”. PIOMAS was first mentioned in lines 73 -74 together with a reference and the full name. Line 118. These 2 periods are not “overlapping”. Referee 2 is right; “overlapping” is not the proper word and we will delete it. Line 140-144. The “bouncing ball anecdote” should be removed. We agree with Referee 2 and we will remove this sentence. “There are much better papers to reference for internal variability” and we do agree. This is why we cited (3 times) Swart et al 2015 since we found it very appropriate and very relevant to our own findings. We will make sure this aspect will be further discussed in the text together with main outcomes in the conclusion. Line 148 “why are the CMIP models

C2

able to better reflect the observed variability” and “better than what?” is not the proper question we addressed. We referred to a citation of Song 2016 “Using the averaged projection of those climate models from the 30 CMIP5 models that can better reflect the observed sea ice volume climatology and variability, it is shown that the September sea ice volume will decrease to 3000 km³ in the early 2060s and then level off under a medium-mitigation scenario”. It is not our intention to analyze all the CMIP model results that have been extensively published. Our intention is only to refer to those publications that are relevant for us and our study regarding Arctic sea ice volume estimated by and /or from CMIP models in order to compare it with our own results based on the FDD approach and PIOMAS. As a matter of fact, we found three very relevant papers 1/ “Assessment of sea ice simulations in the CMIP5 models” by Shu et al 2015 in *The Cryosphere*, 2/ “Change of Arctic sea ice volume and its relationship with sea ice extent in CMIP5 simulations” by Song 2016 in *Atmospheric and Oceanic Science Letters* and 3/ “Influence of internal variability on Arctic sea-ice trends” by Swart et al 2015 in *Nature Climate Change*. Line 154 “the CMIP5 models are not very good”and “sorry but none of this makes sense”. We are not sure what does that mean? This is Referee’s 2 own assessment regarding CMIP5 model results. Of course there are significant differences between CMIP5 estimations and PIOMAS projection. In particular we found a time difference of about 20 years in reaching the 3000km³ sea ice volume minimum described by Song 2016. But we think it is interesting and important to compare PIOMAS results with CMIP5 results as Song started to do it in 2016 (*Atmospheric and Oceanic Science Letters* Vol 9 NO.1, 22-30). Line 158-159. “Who introduced the idea of ice volume from FDD?” We introduced this idea following also some previous studies such as Maykut (1986). The idea is old (but not “odd”) and we applied it to a new data set (ERA Interim). One of our main conclusions is that FDD sea ice based calculations match well PIOMAS sea ice volume in particular when estimating Arctic sea ice growth rate in winter (113 km³ per day deduced from both FDD and PIOMAS as well). Line 166-168. “Why focus on that period?” The period is defined by the ERA Interim reanalysis. From a purely scientific point of view this period

C3

is interesting because it extends over 40 years exhibiting significant changes and also takes advantage of coherent satellite observations starting during the late 70s. Line 169. “What was the previous section if not methods?” We will correct that together with lines 78-89 and line 91 (see previous comments). Line 175. “ERA Interim needs a citation at first appearance.” We cited Simmons et al 2004 and Berrisford et al 2011 in line 180. We could move it up to line 175. No problem. Line 325. “confuses” not scientific. OK we will remove it. Line 329. “we cannot publish “purely arbitrary”stuff”. We do agree with referee 2 that “purely arbitrary” is not an appropriate word to qualify the choice we made with the FDD approach and a comparison with PIOMAS. This is an option that we can justify. We will rephrase this sentence accordingly. Line 332. “How robust the results are?” As we said our main intention was not (and still is not) to write a paper regarding all the various approaches to estimate Arctic sea ice volume. Our main intention is to compare the FDD approach for estimating sea ice volume with PIOMAS in particular and also with CICE for a special case. We also compared Cryosat-2 in order to have an observational perspective. Note that Cryosat-2 and PIOMAS sea ice volume estimations have been recently published (e.g., Tilling et al 2017). We found interesting to compare both with the FDD approach (figure 12). The robustness of the results is mainly based on 40 years of calculations documenting both the long term trend and the interannual variability. Line 359 “dangerously” not scientific. “Please remove.” We agree and we will remove it. Line 409 “It seems quite clear”. “There is still significant debate on this point which needs to be reflected in the paper.” We do agree with referee 2 comment and will proceed accordingly. Line 416-417. “We can seriously predict”. We completely agree with the referee’s comment. First of all “seriously” is not an appropriate word in that context and so we will remove it. Second we are not really making “prediction”. So this is a double mistake from our side and we thank referee 2 for pointing it out. Line 433 “blue Arctic” was first used in Newton et al 2016. We will remove it. “CMIP5 models have been discussed in many, many papers”. Yes this is correct and this is why we selected some of the CMIP5 results related to studies made by Song (2016), Shu et al (2015), Swart et al 2015. See our previous comments

C4

line 148 and line 154. Line 438-441. “several papers have looked at just that” “need to cite those”. Again we did mention Shu et al (2015), Song (2016) and Swart et al (2015) to foresee how the sea ice will transition to an ice free summer Arctic over the 21st century under different scenarios and with the impact of internal variability. As suggested by Referee 2 we will update this aspect by looking at Sigmond et al 2018, Jahn 2018, Notz and Stroeve 2018 that all represent more recent studies (2018) than those we used (2015-2016). We want to make sure our 2015-2016 references are not outdated by most recent ones (2018). Line 443-457. “not a single reference in the whole paragraph”. We do agree with the referee. There are obviously missing references in this paragraph. Thank you for bringing our attention to that important point. We will fix it.

Main points of the study Line 459. “Ice volume was not directly compared with ice extent and thickness”. What is supported by PIOMAS analysis is a reduction of 75% of sea ice volume at the end of the summer for the most recent period (4000 km³) compared to Arctic sea ice summer during late 70s early 80s (16000 km³). During the same period of time sea ice extent has changed from more than 6 million km² to about 3 million km² (that is 50 % reduction for sea ice extent). Implicitly and only based on sea ice volume reduction and sea ice extent reduction, it would imply a sea ice thickness reduction of 50% over the same time period (this is purely mathematical). The word “more sensitive” comes from the fact that we compared a 75% reduction for sea ice volume to a 50% reduction in sea ice extent and in sea ice thickness over the same period of time. We plan to discuss more the sea ice extent and thickness in the revised version of the text in addition to sea ice volume. Line 465-466. “No future projections at all”. We will avoid using the word “prediction” since PIOMAS has no real predictive capabilities. But here we are actually making “projections” based on the tendency observed during the past 40 years, including both the long term trend and the interannual variability. Line 466-468. “Bitz et al 2004 showed that thin ice actually grows faster, so it is probably not linear”. In fact the notion that thin ice grows faster than thick ice was already well understood during the 80s (see the geophysics of sea

C5

ice by Untersteiner 1986) and this was largely taken into account again in our study and supported by our analysis. We presented 3 quadratic relationships relating FDD and sea ice growth and one linear relationship to identify the differences and the sensitivity for sea ice volume estimations. It appears that the important effect of the snow layer on sea ice is increasing the importance of the linear terms of the relationships between FDD and sea ice growth as also mentioned by Maykut in 1986. Line 469-471. “Again no analysis of CMIP5 models in this study”. It was (and still is) not our intention to analyze CMIP5 models. We took advantage of CMIP5 published results (Song and Shu et al. already mentioned and also Swart et al 2015) in order to compare them with other sea ice volume estimations such as those based on PIOMAS for the past 40 years. The main result is to indicate a lag of about 20 years between the CMIP5 models and PIOMAS in sea ice volume decline. This is not plagiarism. We can again add up the two CMIP5 references (Song and Shu et al) in order to clear up this aspect. Line 472-475. “Not shown in this paper. Here what was looked at was ice volume over the observed period.” We did refer to Swart et al 2015 (cited 3 times in our paper) revealing from CMIP5 models a 7 year internal variability that is also appearing from PIOMAS as well during the past 40 years. So we could recall again Swart et al study line 472-475. Line 492-493. The importance of the snow layer for applying the FDD method. We did spend quite a bit of time and text and figures (3 and 11) to explain the importance of the snow layer for applying the FDD method and we demonstrated how sensitive it is. This was also mentioned by referee 1. We do agree with both referees that the snow layer on top of sea ice is a very important aspect. We concluded from our FDD approach that the quadratic relationship linking FDD and sea ice including a 5cm snow layer depth on top of it, was providing the best fit when compared to PIOMAS. Line 494-496. The albedo feedback in winter is negligible. We do agree. Indeed this is not a scoop. Line 516. We can delete this last sentence. Maybe this is premature. We agree with the referee that we need to use different reanalysis product and this is what we just started to do by using ERA 5 instead of Era Interim. We also realized that applying an old method to new data set is triggering lots of questions and comments

C6

and we agree with the fact that time is needed for all of us to get more familiar with this new approach. Sorry again for the bad quality of the figures that we have all reworked and improved. This was also noted by the referee 1. We realized the bad quality of the figures did not help understanding our paper and we do apologize again. General comments “Half of the main outcomes are not supported by the analysis done” according to referee 2. In the main conclusion we identified 12 main outcomes. What are those half unsupported outcomes ? Obviously the two referees have questions about the methodology we used for the FDD approach and this is quite easy for us to rectify. It is so simple and straightforward that we did not think it was necessary to give all the details. But we agreed with both referees to provide all the information about the methodology we used. Regarding referee 2’s comment about “the methodology being absolutely not reproducible”, we disagree. It is perfectly reproducible and we could easily provide the software we used (additional materials). We also do believe FDD results match PIOMAS variability well. We strongly suspect the bad quality of the figures being instrumental for some misunderstanding expressed by the two referees? We have already improved the quality of the figures. References. It is quite possible that we missed some specific reference at a proper place in the main text and as we already said (lines 443 to 457) we can correct it by inserting more citations. We feel quite uncomfortable with referee 2’s comment about “plagiarism”. This is a very severe and unjustified accusation. We were able to respond point by point to all the remarks made by Referee 2 as far as those remarks were specific and explicit. We agreed with most of them and we disagreed with few. So there is no need for unjustified and excessive accusation. There is also obviously a question about the language we used (english) since none of us are native english speaking scientists. We apologize for this inconvenience but we ask for the indulgence of the referees and editors since in addition to science, we have to make it in a proper language. Overall, we would like thank referee 2 for his (or her) time and many constructive comments.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-2>, 2019.