

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

Anonymous Referee #2

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Overall: This paper attempts to assesses the causes of the rapid decline of the Arctic sea ice volume. For this task Freezing degree days are used, based on ERA-Interim temperatures, and results for derived ice volume are compared with PIOMAS an Cryosat-2. Unfortunately, I have to recommend that the paper should be rejected. I hesitate to write such a bad review, but unfortunately I feel it is justified for this paper. I started taking detailed notes, but then stopped for the middle part of the paper as it became clear that there is way to much wrong to make this publishable even in a major revision. The paper lacks scientific rigor, both in the analysis technique, which admittedly used the data sets it used for “purely arbitrary” reasons and in it’s writing. In terms of the writing, there is a lack of citations where other people’s work is described (bordering on plagiarism as many of those results are then listed in the summary under “main

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outcomes resulting from this study". A student at a university would be reported for plagiarism for submitting this paper!), hard to follow writing style with unclear structure, and imprecise writing (many in-precise and sometimes hyped terms, such as "dangerously", "seriously predict", "confuse sea ice retrievals"). The methodology is also not at all well described and it is absolutely not reproducible. At least half of the "main outcomes resulting from this study" are not supported by the analysis done, and several of them are actually at odds with other research that did actually study it, as well as hyping climate change impacts ("dangerously approaching"), which is not scientific nor justified by the data. The paper in many parts reads like a review, but without proper citations in reporting others people's work. So neither as an original contribution nor as a review do I see merit to publishing this manuscript, especially in a highly regarded journal such as The Cryosphere.

Some specific comments, to illustrate the many things wrong with this paper that led me to conclude that reject is my recommendation:

- Title: Need to take out "consequences", as consequences are not analyzed, just extensively speculated about. But all that needs to go, as none of it is backed up by analysis.
- Line 37/38: This is not correct: "actual Arctic sea ice decline is one of the most representative characteristics of climate change.". Arctic sea ice decline is one of the most obvious effects of climate change. Not the "most representative"
- Line 38/39: "In the past, the main aspect concerned Arctic sea ice extent largely based on space observations. " Incomplete sentence.
- Line 41/42: Missing closing parenthesis "an abrupt decline in the Arctic winter sea ice cover in 2005 and 2006"
- Line 43/45: No citation for the 2007 minimum?
- Line 45/46: I like the IPY as much as you, but this statement needs proof to back it up

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or it has to go in a scientific article: “Thanks to the IPY stimulating a major effort from the scientific community, the first decade of the 21st century ended with an unprecedented amount of new results regarding the Arctic sea ice.” Are those all attributable to the IPY (Which should be possible to track down based on acknowledgements and IPY publication database). But I don’t see why this statement needs to be here in the first place, so I would recommend removing it.

-Line 47/48: Those citations need to go, we have no idea what they are cited for scientifically, apart from being IPY products. No scientific reason for these citations, so please remove them.

-Line 49/50: “Zhao et al. (2018) described a strong decrease of sea ice concentration in the entire central Arctic in 2010.” In what season? Annually? Needs more context. Wasn’t another minimum.

-Line 50: Please change “The whole time record” to “the absolute record”

-Line 53-55: Unclear sentence, please revise. “This continuous chain of events maintained a strong motivation among scientists for Arctic sea ice research both from a modeling and experimental point of view taking advantage of new technologies for observations and more sophisticated models. “

-Line 56: remove “peculiar and intriguing”, it’s imprecise, open to interpretation, and not a scientific term

- Line 63: Who did this? “In order to better analyze and understand Arctic sea ice evolution, an important step was accomplished by introducing Arctic sea ice volume.”

-Line 78-89: This should be in a methods section. Not the introduction

-Line 91: “In this Introduction”: this is not a introduction, it is a methods or results section (not clear). Please revise

-Line 91/92: Needs a reference for PIOMAS right here at first mention. Also needs a

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-Line 118: These two periods are not “overlapping”, so why does it say “overlapping” here?

-Line 140-144: Unclear why this bouncing ball anecdote is included here, it is not referred to again in the following sentence. So should be removed. There are much better papers to reference for internal variability which should be cited instead (Winton 2011, Kay et al 2011, Swart et al 2015 as cited, etc). The bouncing ball is useful to explain internal variability to a lay audience, as done by Ed Hawkins in his blog as quoted, but has no place in a paper in The Cryosphere, read by cryospheric scientists.

-Line 148: Why are the CMIP models able to better reflect the observed variability? Averaging them all mutes the variability and the thickness and area, and hence, volume, in many of them is very wrong (Massonnett et al 2018). Also, better than what? “Averaged projection from 30 CMIP5 models that can better reflect the observed sea ice volume climatology and variability”

-Line 154: Exactly, the CMIP5 models are not very good. Sorry, but none of this makes sense

-Line 158/159: Who introduced the idea of ice volume from FDD? Needs a reference here as well as much, much more information on this method, as it seems very odd. It also doesn't match PIOMAS variability well, as shown by the reply to Review 1 comments. And it is unclear why one would want to use that over PIOMAS in the first place, that case needs to be made first. Using reanalysis temperatures over the Arctic likely includes large errors. What is the point of using this old method? What are the details of the method? Has to be reproducible, so needs a lot more detail.

-Line 166-168: Why focus on that period? Should have a scientific reason as to why.

-Line 169: What was the previous section if not methods?

-Line 175: ERA-Interim needs a citation at first appearance.

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- Line 325: not scientific “confuses” sea ice thickness retrievals
- Line 329: We can not publish “purely arbitrary” stuff. The sensitivity to all choices has to be investigated, so that we know if any conclusions are robust.
- Line 332: For simplicity we did not want to open a new section by inter-comparing different data set and co-evaluating various models.” Well, for scientific accuracy, and to publish a paper, one has to do at least some assessment of how robust the results are, if the choice of the data sets was “purely arbitrary” rather than following some well thought out plan.
- Line 359: “dangerously” not scientific, please remove
- Line 409: “It seems quite clear” I think there is still significant scientific debate on this point, which needs to be reflected in the paper.
- Line 416/17: “We can seriously predict a quasi disappearance of the Arctic sea ice in summer during the coming decades.” How? You didn’t do any forward modeling, and didn’t even describe extending the trend into the future. So how can you “seriously predict”? And what is “seriously predict” in the first place? “seriously” has no place here, or in any scientific writing.
- Line 433: “blue Arctic” was first used in a scientific paper, as far as I know, in Newton et al (2016). Needs to be cited.
- The fact that the CMIP5 models have a huge spread of ice-free conditions has been discussed in many, many papers and the IPCC report. You didn’t show that, you didn’t even look at the CMIP5 models. So you need to cite those studies. Just to name a few: Overland and Wang 2010, Massonett et al 2012, Jahn et al 2016.
- Line 438-441: Several papers have looked at just that, 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. Need to cite those (Sigmond et al. 2018, Jahn 2018, Notz and Stroeve 2018)

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-Line 443-457: Not a single reference in the whole paragraph, but it's all on previous work. Needs references.

-Main points of the study

-Line 459: Ice volume was not directly compared with ice extent and thickness, so how did the study show that ice volume “It is more sensitive”? Not supported by the analysis done.

-Line 465-466: No future projections at all, so how was the projection of Wang and Overland 2009 confirmed? It wasn't so this again isn't supported by analysis done.

-Line 466-468: “Considering that today 75% of the Arctic sea ice volume melted during summer, it would not take long to melt away the remaining 25% of Arctic sea ice in summer.” Not if it is all linear, but other people's work (Bitz et al 2004 or so) has shown that thin ice actually grows faster, so it's probably not linear. And again, this wasn't shown here in any way, so not supported by analysis.

-Line 469-471: Again, no analysis of CMIP5 models, so this point is also not shown here. It is taken from other's work.

-Line 472-475: Not shown in this paper, and other people's papers actually don't show that, using models and actually looking at an ice-free Arctic. Here what was looked at was the ice volume over the observed record.

-Line 492-293: Exactly, that is one factor that really troubles me about this FDD method, and a sensitivity study is needed before writing a paper on it, to see how it changes the results.

-Line 494-496: Of course the albedo feedback isn't important in the winter, there is no sunlight so albedo doesn't matter. Not really a new insight, even though I don't know who first wrote about that.

-Line 516: “We would like to suggest using more extensively Arctic sea ice volume

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deduced from cumulative FDD in particular to evaluate the impact of climate change on Planet Earth in the future. “ I can not suggest that, as first you need to show how it varies using different reanalysis products, and assess the effect of snow on sea ice. Also, I don’t even think anyone can use it, as the methodology isn’t clearly explained, and I have a lot of questions about it.

-Figures: All way to low quality (fuzzy).

- Figure 5: can’t tell different years from each other, needs a better color scheme.

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