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

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First of all we would like to respond to the reviewer's first two general comments and a specific comment (comment 4). As we said "FDD cannot explicitly account for MYI". This is due to the fact that we started integration for sea ice growth H=0 at t=0 (line 213). The key word here is "explicitly". But we did show (lines 224 to 227)"a sharp double peak FDD spatial distribution" followed by "a double peak sea ice thickness distribution typical of the Arctic Ocean and related to FYI and MYI"(line 249). That meant the FDD approach is able to implicitly account for MYI, at least partially. This is because surface air temperature (at 2m), the parameter used for calculating FDD, depends on sea ice thickness (i.e. thicker MYI versus thinner FYI spatial distribution) via air-ice-ocean interactions. Initially it was not obvious that FDD would be appearing

spatially like a double peak distribution similar to Arctic FYI and MYI. As a matter of fact the FDD double peak spatial distribution appeared very clearly for all the time series we analyzed extending over the past 40 years except for last year (2018) when only a single peak FDD distribution showed up. This is more likely a tipping point regarding MYI evolution, indicating MYI is vanishing to the point it is getting more and more difficult to distinguish MYI from FYI. The Eumetsat maps representing MYI in March and December 2018 are still questionable. The definition of MYI is quite broad and vague. On many instances MYI is everything else but FYI and it could represent many different things such as second year ice, deformed ice, ridged ice rather than MYI and still be interpreted as old ice. The new fact revealed by the FDD approach for not being able to discriminate MYI from FYI in 2018 for the first time in 40 years is an important result. Of course it does not mean that MYI has completely disappeared (not quite yet). Consequently we are going to reformulate our statement (lines 26-27 and 281-282) in a less controversial way based on the fact that the FDD approach is ringing a bell concerning the MYI vanishing (rather than having disappeared completely). Let us respond quite specifically to all the other important points raised by the reviewer as far as general comment 2 are concerned. According to the reviewer "FDD are useful for the evaluation of FYI growth". We do agree "the snow cover is not represented realistically". The reviewer is right the snow cover is important and we did pay attention to it even if we have not solved the issue thoroughly. We showed how important and influential is a thin (few centimeters) snow layer on top of sea ice (see figure 3 and figure 11).The best fit between FDD and PIOMAS was obtained with a snow layer of 5cm uniformly distributed (figure 11). With better snow layer observations over sea ice we could easily take this snow effect into account in a more realistic way. But we are still lacking a realistic snow cover for the Arctic Ocean unfortunately. "ice dynamics are not taken into account". This is true and there is a justification as demonstrated by Stroeve et al. 2019 (lines 620-621) ice dynamics are contributing much less than thermodynamics for sea ice growth in winter 2016-2017. Based on CICE (the Los Alamos model) results ice dynamics was contributing to +1 to +4 cm for sea ice growth compared to -11 to -13 cm for thermodynamics according to Stroeve et al. (2019). “of course the winter ice growth is very much driven by thermodynamics”. Not only we do agree with the reviewer but the FDD approach is proving this is true. Compared to the 11 to 13 cm thermodynamic ice growth reduction mentioned by Stroeve et al. 2019 based on CICE, the FDD approach gave a very similar results (14 to 15 cm sea ice thickness reduction). This makes us confident about the FDD approach being realistic and not too simplistic. “the authors find a rude correlation between the FDD volume and the PIOMAS estimates”. It is true that the FDD approach and PIOMAS sea ice volume estimates were giving very different answers at the beginning of the time series at a time when MYI was abundant. That was 30 to 40 years ago. But that is not the case for more recent years. The similarity between PIOMAS and FDD sea ice volume estimates for the past 10 years is remarkable in particular for matching the large amplitude interannual variability so well. This is not a “rude” correlation. Let us now answer other questions and comments from the reviewer. General comment 3 and specific comments 1 and 2 We will improve the section concerning the way we calculated sea ice volume and we will provide all the necessary “crucial information about the applied methods”. We will also provide basic information about the “consistency” for the 3 different methods (Cryosat-2, PIOMAS and FDD) used for estimating sea ice volume without repeating and duplicating what has already been published extensively regarding Cryosat-2 and PIOMAS. Regarding lines 70-71 (this is also specific comment 1) the results are based on PIOMAS and we will reformulate this sentence in a clearer way. Lines 306 -312. We agreed with the anonymous reviewer that lines 306-312 are not necessary (as also mentioned in the text). In order to avoid any confusion and/or misinterpretation we will delete lines 306-312 in a new version of the text. Lines 465-468. The prediction of Wang and Overland (2009) is based on a paper published in GRL entitled “A sea ice free summer Arctic within 30 years”. In the abstract of this paper it is indicated “We predict an expected value for a nearly sea ice free Arctic in September by the year 2037”. In addition it is also indicated in the same paper for presenting Figure 2 “this provides an expected value for a September nearly sea ice free Arctic in the year 2037”. Consequently for

lines 465-468 we suggest to add “nearly” to “sea ice free” and we will delete “or even earlier (2030-2035)”. Detailed comment 5 (line 416 – 417). Neither FDD, nor PIOMAS and Cryosat-2 can make real prediction. Consequently we will delete “predict” and will use “envision” instead. General comment 4 and detailed comment 6. We apologize for the bad quality of the figures and we will improve all the figures to make them fully readable. Detailed comment 3 (line 273). We will argue more precisely about the 1% uncertainty for sea ice extent in a new version of the text. In conclusion and responding to the reviewer’s comments regarding FDD being “problematic, too simplistic and not state of the art”, we believe that simplicity does not necessarily mean simplistic or problematic. The main simplifications we used for the FDD approach concern ocean heat flux and ice dynamics that we considered legitimately less important in winter than conductive heat flux and other influential ice thermodynamic processes such as the snow cover. Simplification is always the case even with the most sophisticated approach. We just tried to demonstrate in this paper that simplifications we used for the FDD approach lead to realistic results. Simplicity might also contribute to the state of the art in science. Many thanks to the anonymous reviewer for spending time and for important and relevant comments concerning our paper “Rapid decline of Arctic sea ice volume” that would help us for improving the quality of the paper. We will certainly be able to improve scientific rigor and the quality of the figures quite significantly.

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