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Interactive comment on "Mapping and Assessing Variability in the Antarctic Marginal Ice Zone, the Pack Ice and Coastal Polynyas" *by* Julienne C. Stroeve et al.

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Response to reviewers' comments We are grateful to all reviewers for their thoughtful comments, which have strengthened our manuscript. Below we detail our responses to the reviewers' comments and changes made to the manuscript. Our responses are italicized and highlighted.

Reviewer #1 Interactive comment on "Mapping and Assessing Variability in the Antarctic Marginal Ice Zone, the Pack Ice and Coastal Polynyas" by Julienne C. Stroeve et al. H. Flores (Referee) Hauke.Flores@awi.de Received and published: 29 March 2016 General comments Julienne Stroeve and her co-authors compare the results of two popular algorithms using passive microwave satellite data to classify ice types in the

Antarctic sea ice zone, the NASA Team and the Bootsrap. They show that sea ice extent estimates are largely consistent between the two algorithms. They differ, however, in the proportion of consolidated pack-ice versus marginal ice zone (MIZ) and polynias, with greatest discrepancies in the contribution of the MIZ. When applied to biological datasets, in this case the breeding success of snow petrels Pagodroma nivea, however, these discrepancies can lead to opposite conclusions. This manuscript presents a highly desirable critical cross-validation of satellite-derived data. In particular, the inclusion of a biological dataset adds high value to both physical and biological communities, highlighting the importance of such exercises in this under-studied cross section of disciplines. Furthermore, it was a pleasure reading this manuscript, since it manages to present a complex topic in an easily understandable language, even to non-physicists.

We thank the reviewer for their positive comments.

Having said this, there are a few things that should be improved: 1) The results and discussion section are not well separated. To preserve the excellent flow of the manuscript, I recommend to merge them into a "Results and Discussion" section. If this is not possible, speculations and literature references should be consequently moved from the results section to the discussion.

Given the length of the results and discussion it seems best to keep the Results and Discussion sections separated. Thus, we moved most of the speculations and literature references into the discussion section.

2) In my view, the greatest weakness of the approach chosen is the definition of the 'polynia' ice type. Just using a proportional ice coverage of 0.8 near the coast as a criterion may incur confusion with other more open ice situations. It may well be that the two algorithms 'see' completely different things, and therefore result in a large difference in the seasonal occurrence of 'polynas'.

Indeed, the notion of an 80% SIC threshold is somewhat arbitrary. In Li et al. (2016) we found that to match NASA Team and Bootstrap for polynya area, we needed to use a 75% SIC threshold for NASA Team and 85% for Bootstrap. This highlights the sensitivity of the algorithms. While we find good spatial distribution with polynyas reported in previous studies (e.g. Massom et al., 1998, Arrigo and van Dijken, 2003, Kern 2009, Arrigo et al., 2015) we are really focused on the initial stage of polynya development (before adjacent polynyas coalesce into a larger one). We have now specifically stated in the methods section:

We have previously tested mapping polynyas using a SIC threshold of 0.75 and 0.85 for the NASA Team and Bootstrap algorithms, respectively, and found that these thresholds provided consistent polynya areas between the two algorithms and matched other estimates of the spatial distribution of polynyas [see Li et al., 2016]. However, for this study we chose just one threshold, a compromise between the two algorithms, so that we can better determine the sensitivity of using the same threshold on polynya area and timing of formation.

3) There is little information provided on the statistics of GLMs looking at the breeding success of snow petrels. A convincing statistical approach, including model selection, is the fundament of any conclusions concerning the seemingly opposite outcomes of the two algorithms. We have added more to this section. We appreciate the reviewer finding this an important component of the paper. The concept of strength of evidence seems almost “new” in the life and social sciences. Traditional methods have focused on “testing” null hypotheses based on test statistics and their associated P values. From the P value comes an arbitrary judgment concerning “statistical significance” and dichotomous ruling about the rejection of, or failure to reject, the null hypothesis. For several reasons, P values do not constitute a basis for formal evidence (see Royall 1997). The new I-T methods are not a test in any sense; rather they represent a very different methodology for empirical science. The I-T methods provide a formal, fundamentally sound, approach of developing an a priori set of hypotheses and then a quantification of the data-based evidence for, and ranking of, each hypothesis. This is followed by interpretation of the results in the face of model selection uncertainty and

this is one aspect of multimodel inference."

Specific comments Introduction Ll 44-66: When addressing regional variability, the strong decline in the WAP should be mentioned with respect to the final objective to discuss ecological implications. Done and we now reference Ducklow et al. 2012 and Smith and Stammerjohn 2001 about sea ice declines in the WAP.

Also, previous declines in Antarctic SIE should be mentioned. For example, Flores et al. (2012) note that "This growth, however, has so far not compensated for a decline of the average sea ice coverage between 1973 and 1977, which accounted for âĹij2 × 106 km2 (Cavalieri et al. 2003, Parkinson 2004). Reconstruct ions of the position of the ice edge in the pre-satellite era give strong evidence that the overall areal sea ice coverage in the Southern Ocean declined considerably during the second half of the 20th century (Turner et al. 2009a)". Done

M&M Ll151-152: Could be problematic with respect to ecological interpretations, because it also includes areas with loser pack ice and wakes behind icebergs that are not polynias and thus do not necessarily feature the same biological dynamics. Also: What is "near" the coast precisely? We agree that we are not necessarily resolving polynyas here with the coarse resolution satellite data. However, since our interest is open water areas near the coast that can be used by bird species for foraging, we feel that this metric is still important. We now make sure to say potential coastal polynya.

Results Ll 413-526: More details on the model statistics are needed. Were the slope terms and intercepts significant? Using differences in AIC alone for model selection can be tricky. I recommend testing the ’better’ model against the ’next worse’ using ANOVA or F statistics, depending on the model applied. It would be useful to see the model validation plots in the supplementary material. To me it is not clear why the model using pack ice and BT is preferred. For the reviewer concern, we think it is a statistical debate. We applied a state of the art approach that is commonly used in ecology. We try to make that point in the revisision. However, the reviewer’s comments seem to somewhat contradict each other. In the main comment it's stated " A convincing statistical approach, including model selection, is the fundament of any conclusions concerning the seemingly opposite outcomes of the two algorithms “which is what we did. But then "I recommend testing the ’better’ model against the ’next worse’ using ANOVA or F statistics, depending on the model applied”, which is not the best approach for model selection.

We have added more details on the statistical model used. We applied the information-theoretic approach to valid inference that "replace the usual t tests and ANOVA tables that are so inferentially limited" (quote from the statistician Burnham et al. 2011 Behav Ecol Sociobiol (2011) 65:23–35 DOI 10.1007/s00265-010-1029-6). This is based on quantitative measures of the strength of evidence for each hypothesis (Hi) rather than on “testing” null hypotheses based on test statistics and their associated P values. Here, to quantify the strength of evidence for each hypothesis (Hi) , we used the common criteria AIC= - _2 log(L))+2K. The term -_2 log(L)) is the “deviance” of the model and log(L) the maximized log-likelihood. We select the model with the smallest AIC value as “best” in the sense of minimizing Kullback–Leibler (K-L) information loss, i.e. provide the highest strength of evidence for the given hypothesis. The section now reads as:

Effects of MIZ and pack ice area were analyzed using Generalized Linear Models (GLM) with logit-link functions and binomial errors fitted in R using the package glm. Specifically, the response variable is the number of chicks Ct in a breeding season t, from 1979 to 2014 collected at Terre Adelie, Dumont D'Urville [Barbraud and Weimerskirch, 2001, Jenouvrier et al., 2005]. It follows a binomial distribution, such that Ct $\sim$ Bin(ïĄ■t,Nt), where Nt is the number of breeding pairs and ïĄ■t is the breeding success in year t. The breeding success is a function of the MIZ and pack ice covariates at time t (COV) such as: ïĄ■t = ïĄć0 + ïĄć1 COV(t)

To select the covariate that most impacts the breeding success of snow petrels, we applied the information-theoretic (I-T) approaches [Burnham et al., 2011]. This is based

on quantitative measures of the strength of evidence for each hypothesis (Hi) rather than on “testing” null hypotheses based on test statistics and their associated P values. To quantify the strength of evidence for each hypothesis (Hi) – here the effect of each covariate on the breeding success- we used the common criteria AIC (the Akaike's Information Criteria), where AIC = - 2 log(L) + 2K [Akaike, 1973]. The term, -2 log(L), is the “deviance” of the model, with log(L) the maximized log-likelihood and K the total number of estimable parameters in the model. The chosen model being the one that minimizes the AIC, and the ability of two models to describe the data was assumed to be “not different” if the difference in their AIC was < 2 [Burnham and Anderson, 2002]. While non-linear models may be more appropriate as ecological system relationships are likely more complex than linear relationships, without a priori knowledge of the mechanisms that could lead to such non-linear relationships, it is extremely difficult to set meaningful hypothesis to be included in the model selection. Discussion L619: Another way to validate algorithms would be the ASPEcT ship observation data. Yes that could be useful and something to consider for future work.

Conclusions Ll643-646: I have the feeling that the results of the polynia estimation are blurry in both algorithms, resulting in this large variability of timing of polynya maxima. This may be due to inaccurate definition of polynyas in the analysis. We agree that precise polynya delineations are problematic at this resolution, but as we previously state, our interest is really in potential open water near the coast. We added another sentence in the conclusions to highlight this: While we do not precisely resolve polynyas, these potential coastal polynyas (i.e. open water areas near the coast) are important foraging sites for sea birds.

Technical corrections Abstract L14: replace “biological” with “biologically” Done

Introduction Ll 40-43: Split this sentence in two. Done

L59: I believe it should say “maxima” Corrected

L94: Did you mean “continuously”? Corrected

L114: replace “mattes” by “matters” Done

Results L197: Better say “Results and discussion”? We felt it best to keep them separate

L215: delete “(e.g. the shading)” Done

L373: replace “significantly” by “significant Done

L375; delete “(e.g. spring)” Done

Figure 5: What is the variability measure here indicated by the shaded areas around the curves? This is the standard deviation from the long-term mean. We now state that in the figure caption.

Reviewer #2: Stefan Kern Summary: Sea ice concentration data sets obtained for 1979-2014 with two different al- gorithms (NASA Team and Comiso Bootstrap) from satellite microwave radiometry are used to investigate the spatio temporal evolution of a number of different ice regimes. These regimes are defined by sea ice concentration thresholds and by their position relative to the coast and/or open water along radial transects through the ice cover. Classified maps are subsequently used to compute the circum-Antarctic and regional extent of the ice regimes selected. The focus is laid onto ice regimes "marginal ice zone (MIZ)", "pack ice" and "possibly coastal polynya". The spatio-temporal distribu- tion of these ice regimes is investigated and discussed with focus on the difference between both algorithms and on trends (and their significance) over the period investigated (1979-2014). At the end the paper tries to link the satellite observations with biological observations.

General comments: The reviewer is allover in line with the general content and also the main message of the paper. There are several issues, however, which the authors could improve and/or decide upon to substantially improve the paper. Points 1), 2) and 5) are those which made the reviewer to decide to rate rigour and impact as "fair" 1) One of the two algorithms used is - to the opinion of the reviewer - well known to provide

more reliable results for Antarctic sea ice. The authors tried to avoid to state this upfront in their paper. Also in the discussion of their results the reviewer feels that the authors could have elaborated more on the known differences between the two algorithms and on their, partly known, reasons. The reviewer feels that this would come closer to the "Assessing Variability" part stated in the title of the paper. We thank the reviewer for their very in depth review. We agree with the reviewer that the Bootstrap algorithm has been more systematically used for Antarctic studies. However, we find in the literature that the NASA Team algorithm is also used in many studies, even in a recent study by myself and colleagues, as we wanted to use the most up-to-date data set and the NASA Team algorithm is produced in near-real-time. We have purposefully steered away of recommending one algorithm over another as we feel that in order to do so requires more validation studies, which we plan to do using visible imagery this year. We already spend three paragraphs on the known differences between the algorithms and impacts on the ice concentrations. We feel that this is sufficient given the lack of validation of each data set in the Antarctic. However, we add yet another reference from observations in the Weddell Sea that suggest sea ice concentrations are likely closer to the Bootstrap algorithm than the NASA team, though these were made in the interior of the ice pack. An in depth validation effort is outside the scope of this paper but will be performed this summer and submitted to a remote sensing journal.

2) The reviewer feels that the explanation of the methodology, its sensitivity to chosen parameter values, its limitations, and the relevance of the results could be improved: a) Some specific information about the methodology is missing (as is mentioned in the specific comments. b) An investigation about the sensitivity of the chosen threshold to separate MIZ from pack ice is missing. The authors might ask themselves how their results would look like if they would have used different sea ice concentration thresholds. c) The inclusion of ice regime "possibly coastal polynya" into the analysis seems not to be straightforward. The main reason for this is the fact that many polynyas are sub-grid scale features and therefore produced somewhat noisy signals in the analysis. Instead, as is suggested by Figure 5, the ice regime "broken ice in the pack ice"

might deserve more attention because this is on average the third-largest fraction the ice regimes chosen occupy in the NT data - at least with the chosen threshold of 80% sea ice concentration. In short: I would suggest to skip "possibly coastal polynya" and include "broken ice". d) As with regard to the relevance it would have been good to see how relevant results are. A trend in a regime of 10 km / decade manifests itself in 1-1.5 grid cells over the entire time period looked at. How sure are the authors that their results are solid? The choice of the MIZ threshold was based on the paper by Strong and Rigor for the Arctic, and this was validated with National Ice Center Charts. Thus, we chose to remain consistent with the definition of the MIZ used in that study. As mentioned above, we do plan a validation this summer with visible imagery. We agree with the reviewer that the trends in the MIZ are rather small (1-1.5 grid cells) when averaged over the entire SH sea ice, but regionally there are larger trends. The reason we used possible coastal polynya as we have already published a paper in GRL this year using the NASA Team and Bootstrap data for mapping polynyas and comparing with the timing of phytoplankton blooms. We agree with the reviewer that polynyas are generally sub-grid scale features and not straightforward to detect with the coarse resolution passive microwave data. However, it is not our intent to accurately map polynyas at this resolution but rather areas where they may exist and therefore areas that are biologically active regions. In this use, and the reason we use the word, ‘possible coastal polynyas’is for the biological relevance and the fact that birds can forage in these open water areas. We make this clear now in the methods section and state: We have previously tested mapping polynyas using a SIC threshold of 0.75 and 0.85 for the NASA Team and Bootstrap algorithms, respectively, and found that these thresholds provided consistent polynya areas between the two algorithms and matched other estimates of the spatial distribution of polynyas [see Li et al., 2016]. However, for this study we chose just one threshold, a compromise between the two algorithms, so that we can better determine the sensitivity of using the same threshold on polynya area and timing of formation.

3) The reviewer was confused a bit by the 3 or 4 different over-arching names the

authors used for MIZ, pack ice, etc. Harmonizing these and not using "ice type" would certainly improve readability of the paper. We are not entirely sure what the reviewer means by different names for the MIZ and pack ice, but we have removed the use of ice types to address the reviewers concern. When instead say ice category.

4) The text with regard to interpreting Figure 9 seemed a bit long and confused sometimes. Here the reviewer feels that focussing more on the easily to identify features would help the reader. Also, as detailed in the specific comments, a shift of the starting point from 0degW to 60degW would enhance interpretation of the figure. The authors might also want to think about difference maps between trends obtained for NT and BT. We simplified the discussion but chose not to show difference maps as they are harder to interpret.

5) While the reviewer applauds the inclusion of the biology at this stage as an external means to assess the satellite observations, the reviewer feels that a more detailed description is required to fully understand and evaluate the results presented in Figure 11 and Table 5. In the discussion the paper focusses more on climatic issues and potential linkages instead of discussing the potential limitations of the approach (see 2) and the added value included by the inter-disciplinarity. If this paper aims to advertise that through interdisciplinarity value is added to such kind of an analysis of a geophysical data set, then it has failed because it is neither visible from the title nor is this topic given enough weight, in comparison to the climate issues, in the discussion, and the future potential of using such inter-disciplinary approaches is also not discussed further. This part does not really seem to be integrated into the paper. We have attempted to better integrate the biology aspect of the paper. We have also changed the title to: Mapping and Assessing Variability in the Antarctic Marginal Ice Zone, the Pack Ice and Coastal Polynyas in two Sea Ice Algorithms with implications on Breeding Success of Snow Petrels. We have additionally included more information on the biological method that we hope allows readers to fully understand the results presented in Figure 11 and Table 5.

Specific comments: Abstract: First impression is that > 50% of the abstract do not reflect results of this study. Perhaps the sentence "Knowledge of the ... contraction in others." could be replaced by a sentence describing the methodology used in the paper - which is currently missing in the abstract. We slightly tweaked a sentence in the abstract to include the methods used: This study uses two popular passive microwave sea ice algorithms, the NASA Team and Bootstrap, and applies thresholds to the sea ice concentrations to evaluate the distribution and variability in the MIZ, the consolidated pack ice and coastal polynyas. We also added a bit more of the results into the abstract: Results reveal that the seasonal cycle in the MIZ and pack ice is generally similar between both algorithms, yet the NASA Team algorithm has on average twice the MIZ and half the consolidated pack ice area as the Bootstrap algorithm. Trends also differ, with the Bootstrap algorithm suggesting statistically significant trends towards increased pack ice area and no significant trends in the MIZ. The NASA Team algorithm on the other hand indicates significant positive trends in the MIZ during spring (September, October and November). Potential coastal polynya area is also larger in the NASA Team algorithm, and the timing of maximum polynya area may differ by as much as 5 months between algorithms. These differences lead to different relationships between sea ice characteristics and biological processes, as illustrated here with the breeding success of an Antarctic seabird.

L49-51: In this context, the authors might want to refer to the work of Reid et al., and Simmonds, both in Annals of Glaciology, 56(69), 2015. Done

L67-75: The reviewer is wondering whether the authors might want to include also the work of Holland and Kwok, Nature Geoscience, 5, 2012, which nicely suggests the different effects and regions of predominantly dynamic control and thermo-dynamic control. Holland, The seasonality of Antarctic sea ice trends, Geophys. Res. Lett., 41, 2014, might also be a paper to take a look at and to include here. Done

L81: The reviewer suggests to refer also to the book edited by M.O. Jeffries: Antarctic Sea Ice, Physical processes, interaction and variability, Antarctic Research Series, 74,

AGU, 1998. Thank you for pointing out this reference, we were unaware of this book.

L82: The authors write of a "dynamic MIZ". Is there also an "non-dynamic MIZ"? Comment: The authors define the MIZ basically via the dynamic processes and in a way refer to the action of waves to fracture the sea ice and generate smaller floes. While on can imagine that the width of the MIZ can be defined during on-ice wind or swell events by the penetration of swell into the sea ice The reviewer is wondering how one defines the MIZ during the other times, i.e. when there is no on-ice wind of swell, the pressure is released and the sea ice is following perhaps a divergent motion with a lot of openings which are refreezing. How does on define the MIZ here? This is discussed further in the methods section. As we state we chose the threshold based on previous studies of mapping the MIZ in the Arctic.

L83: "longer and larger ice-free summer". What is meant by "larger ice free"? Longer refers to the time, and larger referred to spatial area. We found the sentence was not really needed so we removed it.

L84-94: What the reviewer is missing here is the special character of the MIZ in the Antarctic during sea ice growth / ice edge advance; the suggestion is to get back to the "good old pancake ice cycle" notation of Lange et al. 1989, Annals of Glaciology, 12, 92-96 and Lange and Eicken, 1991, Annals of Glaciology, 15, 210-215. Even though these are old papers there is recent evidence that this is still valid: e.g. Ozsoy-Cicek et al., Deep Sea Research part II, 58(9-10). We are not entirely sure what the reviewer is looking for. The aim of the paper is not to go into detailed processes of the formation of the MIZ (e.g. pancake ice formation) but rather give a brief description of the MIZ and its importance for biological activities. Since the paper is already rather long, to go into the detail requested by the reviewer will only add to the length to the paper but also likely confuse readers as to what the study is really about, and that is to compare two different sea ice algorithms and interpret how the use of the different algorithms would impact uses of the data for biological applications.

L89: "ocean waves define ..." Is a repetition of L82. Fixed

L92-96: The polynya part comes a "bit naked" with regard to citations. Suggestion: Morales-Maqueda et al., 2004, Review of Geophysics, 42, and later on, when it comes to the role of ice production perhaps: Drucker et al., Geophys. Res. Lett., 38, 2011. Done

L119: At the end of this line one could add the reference of Comiso et al., 1997, Remote Sensing of Environment, 12 and of Comiso and Steffen, J. Geophys. Res. 106(C12), which both nicely illustrate to pros and cons of the two algorithms the authors are using in their study. We feel those references are a bit out of place in that sentence but we refer to them in the discussion.

L126: The reviewer is not too happy with the term "ice type" as the authors use it here. Ice types are given in the WMO nomenclature of sea ice and are terms such as thin first-yer ice, shuga, nilas, grey ice, pancake ice, etc. What the authors define here in their work can perhaps be termed better by "sea ice regime" or "ice category" - a name the authors use for instance in L180 anyways, while in L187/188 the authors name it "ice classes". Perhaps sticking with one term would be a good idea. In case the authors decide to stay with "ice type" then the suggestion is to state clearly here that they are not referring to ice types in the classical sense but use the term "ice type" to differentiate between MIZ, pack ice, and polynya - and even more "broken ice in the pack". We now say ice category if we do not specifically refer to the MIZ, pack ice or polynyas.

L138/139: Here it might help to mention that the NSIDC has combined these two algorithm to build the first NSIDC sea ice concentration CDR (Peng et al., 2013, Earth System Science Data, 5) Good idea, now mentioned.

L145: "heavily influenced" Could this statement be precised? We feel this is not necessary

L148: The authors could note here that presumably this definition of the border between MIZ and pack ice is not connected to the other, more dynamic definition of the MIZ via the penetration depth of waves into the sea ice. Is that correct? It is difficult to determine precisely how far waves penetrate without doing wave analysis as well, which is outside the scope of this paper. Our definition of the MIZ follows that from Arctic studies and should reflect most of the wave/ice interactions.

L152: Work carried out previously used different sea ice concentration tresholds: e.g. C5 70%, e.g. Parmiggiani et al., 2006, International J. Rem. Sens. 27(12) or Massom et al. 1998, Annals of Glaciology, 27, 420-426. The authors could comment on their choice of using 80%. As we stated in the manuscript, we followed the analysis by Strong and Rigor, thresholds that were previously validated in the Arctic. We feel this is justified for this paper without additional validation with other satellite data.

L153: What is the longitudinal sampling of the radial transects? Or in other words: How many transects did the authors use? The authors could perhaps also shed more light on how the transects were placed. The data are on a polar-stereographic grid. Therefore radial transects which have, e.g. a 2-pixel spacing at 55 deg South may overlap the same pixel further pole-ward. How is this treated in the algorithm? Is it correct to say that the automatic classification scheme is simply kind of convering a daily sea ice concentration map into a binary map where specific (which?) values are assigned to pixels of the respective ice category. In other words, the range of 0 ... 100 is condensed to 5 values (according to Figure 1 and Table 1). For the classification of the daily 25 km grid cells, transects (stepping north to south) was performed at 1/2 degree steps. Yes, there is some noise in the edge on a daily basis. Some missing data will filled in by values from adjacent days. The classification scheme converts the raw concentration data into 5 classes: MIZ, PACK, Polynya, Ocean and Interior broken sea ice within the pack ice region. But it is using the relative positions of open water, MIZ and Pack ice to lead to some understanding of the time variation of sea ice. Ultimately one has a 3 dimensional array of sea ice concentrations (lat, lon, day).

We are classifying it to better understand what is happening as the seasons and years change. Other classifications (for instance different thresholds or different geometry) might tease out other general features of the changes. We can only say that this classification improved our understanding of sea ice changes. In this paper we are trying to communicate the general features we found.

For the area average seasonal cycles and trends the actual area of each grid cell was used. This smoothed out the rough edges. If one looks at figure 8, the daily annual cycle of trends, some noise is leaking through. Similarly some of the small scale bumps between different longitudes in figure 7 are not important.

Hence there is no computation involved - in contrast to what is stated further down in L189. Is Table 1 really needed. Will the information given therein used later in the paper? We prefer to keep Table 1 in as it also shows the SIC thresholds for broken ice and open ice in the pack ice.

L172: Polynyas may also form offshore of a fast ice region. This occurs for instance in the Eastern Weddell Sea or the Indian Ocean sector. Are such areas counted under category "broken ice inside pack ice" or "inner open water"? What about the Cosmonaut polynya developing in the Indian Ocean sector? We are really focused on the polynyas near the coast, and depending on how far these polynyas are from the coast they may or may not be captured by this method. Depends on whether or not they are captured in the coarse resolution of the satellite pixel. However, given the coarse resolution and the SIC thresholds we use, they are likely captured.

There are regions like closely east of the Antarctic Peninsula and in the Western Ross Sea where the radial transects do, when coming from the north, first transect sea ice / open water, then land, and then again sea ice / open water. How are these transects treated? For the limited range of longitudes that experience this problem, we stepped from the north to the first ice edge. If you look carefully at all the data, there are a few anomalous results near the peninsula and some areas with islands. There are 1%

errors in the classification. That is in keeping with the SSMI concentration accuracy.

L173/Figure 2: Is there a change to instead list the colors in the caption annotate these in form of small boxes in or underneath the block of images? This way the authors dould avoid potential misinterpretation of the Figure by the individual way people allocate actual colors to the name given plus by the way printers interprete colors. Done

L174: Suggest to give the actual dates instead of the day of the year. Done

L189: How did the authors compute gridded fields and regional averages from binary, i.e. classified maps? The reviewer's guess is, the gridded field is not computed but simply obtained by the classification process (see comment to L153). Subsequently, the classified gridded fields are used to compute the areas covered by the different ice regimes - for the entire southern ocean and the 5 regions - on daily temporal scale. The resulting time series of ice regime areas is then used to compute monthly mean ice regime areas. Is this correct? The reviewer had diffulties to understand this the way it is currently written and found myself thinking about how - in one grid cell - a temporal change between different ice regimes is treated. Which grid cell area file did the authors use to compute the ice regime areas? Since the authors did not use data gridded on the EASE grid, the grid-cell area at latitudes different from 70 deg South differ from 625 kmËĘ2. You are correct that we provide binary gridded fields at 25 km spatial resolution with a flag for different ice categories (as seen in Figure 2). We use the area per pixel to compute the true extent of each ice category. Since the reviewer found our description confusing we made the following changes: Using the binary classification scheme, daily gridded fields at each 25 km pixel are obtained. Using this gridded data set we then obtain regional averages for five different regions as defined previously by Parkinson and Cavalieri [2012]. These regions are shown in Figure 3 for reference. Climatological mean daily and monthly time-series spanning 1981 to 2010 are computed for each of the five sub-regions, as well as the entire circumpolar region, and for each ice classification together with the +/- one standard deviation (1ïĄş). Monthly trends over the entire time-series are computed by first averaging the daily fields into monthly values and then using a standard linear least squares, with statistical significance evaluated at the 90th, 95th and 99th percentiles using a student t-test.

L190: Figure 3 shows five instead of six regions. A typo? Corrected

L191 / Figure 3: Does figure 3 on purpose omit showing the borders between the regions? At least in my print out there are no border lines. We have added the border between regions

L192: Can the authors comment on the time period chosen? Why, if the sea ice concentration data sets are available from October 1978 until today they choose a shorter period? Sea ice concentrations based on SMMR, i.e. 1981-07/1987, are only observed every other day. Has this been taken into account in the averaging process? At the time we did the study, the Bootstrap data was only available through 2013 and yes we take into consideration that SMMR is only observed every other day. We updated results through 2014 as the Bootstrap data set was updated.

Figure 4 and its discussion: Figure 4 seems not to using the space allocated for it efficiently. Suggestions: i) Start at a southern latitude of 80 degS or even 75 degS; ii) include the variability of the ice edge location either by using bars showing +/- on standard deviation or by a shading similar to Figure 5. The reader might be interested to see whether one algorithm varies more than the other one. Even though both algorithms seem very similar one can see a southward displacement of the Bootstrap ice egde during summer and a northward displacement of the Bootstrap ice edge during winter. The authors could detail a bit more how large this displacement is ... and perhaps write that this is of the order of one 25 km grid cell (which is how it looks like). The errors are small for Figure 4 because this is an average over 35 years (we have now added error bars on the figure). The idea was to show that NASA Team SIE = Bootstrap SIE. Details like the MIZ and polynya are different because the concentrations are different, especially around the 80% SIC threshold. We do now mention the small southward displacement of the Bootstrap ice edge in summer but the northward

displacement overlaps with the error bars. The authors state that the large emissivity difference between open water and sea ice drives the location of the ice edge. This is partly true only. Another reason why both algorithms show such close agreement in the ice edge location could be the application of a (the same?) weather filter which is known to cut off low ice concentrations [Ivanova et al., 2015]. The weather filters are different between the NASA Team and Bootstrap algorithms and thus this is not a factor here.

L206: The authors speak about an extent, i.e. the sum of all grid cell areas within one ice categorie without any weighing carried out with the actual sea ice concentration, while the y-axis in Figure 5 denotes the quantity given as "Area". Would it make sense to, in order to avoid misunderstandings with the classical sea ice extent and sea ice area, also annotate the y-axes with "Extent"? We can see the reviewers point about a confusion between the extent vs. area used with sea ice concentrations. However, extent in the context of sea ice is the outer boundary of the ice cover. The area of the holes are not subtracted out. In Figure 5 for example, the areas account for different interior ice types.

Another comment to Figure 5 - later in the paper it becomes even more clear that NT and BT are different particularly in terms of ice categories MIZ and pack ice. Not too much could be said about the ice category "possibly polynya" which results seem to be quite noise and also difficult to interpret given that these tend to be a sub-grid scale phenomenon during winter and only seem to become important during November/December. Here, in Figure 5 the 3rd largest difference between NT and BT is not observed for ice category "possibly polynya" but for "broken ice inside pack". Did the authors carried out similar analysis with this ice category like they did with "possibly polynya"? I could imagine that considering and looking at ice category "broken ice inside pack" could be more enlightening than "possible polynyas". We are unclear what the review is after here as we do discuss the broken pack ice inside the pack ice in the final paragraph in this subsection.

L214: "ice types" –> "ice categories" Done

L223: Seems the peak extent for NT is in July. While it does appear so in the Figure, it is actually quite flat from July to October, but as you can see in Table 2 the peak does indeed occur in September.

L224/225: This statement for BT is also for September? Yes

L271: The Bellingshausen/Amundsen Sea is the only region where the maximum MIZ extent does NOT occur after the maximum pack ice extent during spring. Good point, this has now been noted

Figure 6: - Please correct "Bellinghausen" to "Bellingshausen". Done

The lines displaying other ice categories than MIZ and pack ice are very difficult to delineate in the images; one could try to increase the size of the Figure and make it to extend over two pages or to leave these other 3 categories out. We agree it is a bit hard to view the other ice categories and we tried different sizes, which during publication should resolve these features better than how we displayed the data in the word doc.

L272-282: The authors could also mention the large interannual variability in the MIZ extent for NT compared to BT. Done

L283: The authors could add "and bottom" behind "4th row" because they are referring to both regions before "[Figure ...]". Done

L291-303: While the maximum polynya extents can be identified in Figure 6 while they occur during late spring or summer they cannot be identified when they occur during winter. The authors therefore indeed might want to increase the size of the images in Figure 6 and let it extend over two pages. We can request the figure extends two pages during publication.

L306-317, Figure 7: Could the authors mention what the reference location / latitude is against which they define whether the ice edge expands or contracts over time?

One could assume that this is the latitude given in Figure 4 but one cannot find an obvious hint towards this. Perhaps it is arbitrarily chosen? It is 60S, note this figure is for illustrative purposes, Figure 9 shows the trends in all months/longitudes

And an issue coming from personal taste: An expansion of the ice edge is usually associated with cold / colder conditions. And usually cold is linked to bluish colors while warm is linked to reddish colors. It is the other way round in Figure 7. We are keeping it the way we have it as it is consistent with how NSIDC presents ice concentration anomalies (blue for negative, red for positive).

The authors could include into the caption that the bar in the range of the two upper maps give a reference for the trend in latitudinal movement of the extent of the respective ice category. This scale says 10 km / year which means that, e.g., in the Ross Sea there are regions where over the 30 years considered here the ice edge expanded by 300 km. Is that true? - The reviewer is surprized to see that even though we look at a 35-year long time series of data the trends in ice category expansion or contraction can be quite noisy - particularly for MIZ NT in the Indian Ocean sector north of Amery ice shelf or in the transition between Ross and Amundsen Sea. The same is true in a way to Pack ice NT. Can the authors comment on that? - Please increase the font size of the algorithm names in the two top maps of Figure 7. Figure 7 is primarily for illustrative purposes only as we found this a nice way to highlight differences between the two algorithms. We are not entirely sure what the reviewer means by noisy? Perhaps the alternating locations of negative trends rather than spatial consistency? It is true that there is an expansion on the order of $\sim$200-300km. For example, we see at longitude 215, the expansion is 200km, at 219.5 degrees it is 232 km. The physical processes for that are outside the scope of the present paper but would make a good follow on study.

L318 ff and the middle and bottom row of Figure 7: Could the authors also here note what the reference extent is against which they plot the trend in the expansion / contraction? To make this figure, first a sequence of monthly north longitudes for each 2 degree latitude bin was prepared. Then trends in that longitude were calculated. This is listed in km/yr, implying that the ice edge is moving north or south with time. The pack ice latitude is the monthly mean first occurrence of pack ice stepping north to south. Note however this is really for visual illustration to guide the discussion of where expansion and contraction occur in longitude.

L342-359 / Figure 8: - Please increase the font size and only show every second y-axis label to increase readability. - Following up with one of my early comments: The y-axis is again giving "Area" even though you mean "Extent". - Is there a reason why the authors switch between p-values and confidence level in percent? L356: Actually NT does not show positive MIZ extent trends anymore in November. Done

Figure 9: This is a very condense figure with regard to the annotation - particular of the y- and x-axes. The reviewer is wondering whether the authors would consider to either increase the size of the images themselves and therefore of the figure by itself or whether they find a different solution for this problem. The images themself are large enough to identify their message. - Is there a chance to highlight in the images which of the trends are significant? - The "possible polynya" cases so far have not been discussed that much and admittedly are also more noisy and difficult to interprete. How about the authors consider to skip that ice category completely in Figure 9 for the sake of presenting the other two more clearly? - The longitude 60W is a natural break point in the sea ice cover. How about the authors re-organize the images such that they start at 60W and end at 61W, i.e. in the Bellingshausen Sea? This way the authors could better visualize the eastward progression of the areas of positive pack ice trends with season in the Weddell Sea seen by both algorithms. This way the Weddell Sea would also not be split up. - Did the authors think to take a look at difference plots between the trends of the two algorithms? Such a difference plot might highlight the differences between the two algorithms even better. We have changed the figure to increase both the size of the figure and also note which trends are statistically significant by using small circles to denote the areas with trends not statistically significant. We kept the

longitudinal range however from 0-360. We have also changed our discussion of the figure to be more succinct.

L370/371: It would fit to add a sentence here which underlines that in the Ross Sea also in this view (i.e. the way presented in Figure 9) the trends in BT MIZ extent are small (and not significant?). Done

L373: add "significant" after "statistically". Done

L379-382: The reviewer has difficulties to understand this argument. Southerly winds, i.e. winds blowing from the South to the North, cause the sea ice to be advected north - presumably with a substantial amount of lead formation. Openings generated by this more divergent type of sea ice motion should be seen by both algorithms in the same manner. Instead of voting for an additional oceanic influence (because this should again affect both algorithms the same way) the reviewer would vote for a smaller sensitivity to thin ice growing in openings and leads for BT than for NT. The authors could have a look into papers comparing these two algorithms. Also the paper by Holland and Kwok, 2012, mentioned further up, could shed some light on this issue. Ok

L383-385: This statement holds up until May, yes, for both BT and NT and the pattern re-emerges in October in NT and December in BT Ok

L390: Not even the large negative ones in the Weddell Sea in NT? Corrected with regards to Figure 9. When averaged over the entire region then it is true that the trends are not statistically significant (Table 3).

L396-398: Having read this one gets the impression that there is more broken sea ice left after summer melt in the region 30W to 0E. Also very interesting to see that the major positve trend in MAM (NT) or MAMJ (BT) is confined to a very narrow longitude band which moves to the East with progressing season. And it is this narrow longitude band where already in June negative trends pop up. Interesting. We agree this is interesting and added a comment following the reviewer.

L402/403: Suggestion to not comment on the intital retreat in September / October here; for ice category "pack ice" BT trends are almost zero and NT trends as well, they become larger in November / December. Agreed

L404: Which is an interesting notion given the fact that the eastern B.A. Seas have this finger-like structure of substantially negative trends extending from January well into July; NT pack ice trends are weaker here. We realize this sentence was confusing as it was pertaining to the Trends in Table 3 rather than Figure 9. We have rewritten to focus the discussion on Figure 9 and following reviewer 1, much of the discussion is now left for the conclusions.

L405ff: The authors mention both algorithms for MIZ trends but in BT they are very small except perhaps far east in May/June; in contrast there is a large blobb of negative MIZ extent trends visible for NT. The authors could re-write these sentences about what happens in the Bellingshausen/Amundsen Sea more clearly and perhaps also include in the advance-retreat cycling which has been mentioned and investigated by various authors for this region in particular (Stammerjohn is one candidate here, Harangozo another one). The entire section has been rewritten.

L415-427: Of course, for completeness, these two sectors need to be discusses as well. However, the reviewer feels that what is written for these two regions could be condensed even more because i) the patterns are really very similar between NT and BT and ii) pack ice trends are generally positive, more in BT than NT, right, and iii) trends in MIZ extent basically vary around zero with exceptions during September through December in both regions and both algorithms. The reviewer found it hard to get into discussion of significance and the relation to SIE change here. Done

L426-427: This notion is not reflected by Figure 9. The reviewer again suggests to skip the ice category "polynya" in this figure. As requested by the reviewer we have removed polynyas and the discussion from Figure 9.

L440-470 / Figure 10: - The images in Figure 10 could be re-ordered such that the topmost is MAM, then JJA and then SON - motivated by how the season progresses and motivated by the authors' order of describing the figure. Done

In JJA it is hard to see these trends and the fact that they oppose each other in Figure 10; maybe these are not that relevant? We felt it was important to keep the y-axis the same for each seasonal mean, which is why it is hard to see the trend in the MIZ in JJA since it is so small. Since it is not statistically significant in regards to the MIZ it is not all the relevant anyway

in L451: The largest MIZ regime increase is 10 km / decade for NT ... that's not even half a grid cell ... and just between 1 and 1.5 grid cells in total over the 35-year period. Point noted

in L453: Perhaps referring to Figure 7 would be a good idea to underline this notion? Done

in L455: The authors could add that they are still referring to NT here. – We did not find that necessary

in L467: This is an interesting observation and the reviewer is curious to see whether this weaker correlation has found its way into the discussion - it might be an effect of the larger sensitivity of NT to snow property variations particularly during late-winter / spring. We are not entirely clear at this point why there is a difference in correlation strength for SON. It may indeed be a result of snow properties as the reviewer suggests but without data to assess that (i.e. snow data), it is difficult to speculate with any certainty.

L472-481: This introduction into the seabird topic underlines my comment given with respect to Figure 5 that ice category "possible polynyas" is perhaps not that relevant for this study. While it is not entirely relevant for this study as we are focusing on a different sea bird, others who do studies on other bird species may find this discussion relevant. Thus, we have decided to keep this in.

L502-504/ Table 4: The reviewer is a bit surprized that the number of chicks counted in Dumont D'Urville, which is situated at longitude 140degE can serve as a response variable for a region which in 5 of the 6 months taken starts at least 20 deg further West and which extents up to a quarter way around the continent towards the West. Is that site (Dumont D'Urville) really representative for the geographical region chosen? Why didn't the authors chose a region which is kind of centered at the chick collection site? Perhaps the authors could comment on that. We used the distribution at sea of snow petrels recorded from miniaturized saltwater immersion geolocators during winter to define our area. Please refer to Delord et al. (2016) and their Figure 2 with the mean latitudes and longitudes of snow petrel during the non-breeding period recorded (see also our Table 4). We have now added more information on legend of Table 4 to clarify that point. "These areas were defined from the distribution at sea of snow petrels recorded from miniaturized saltwater immersion geolocators during winter (Delord et al. 2016)." Then, we study the carry over effect of winter conditions on the breeding success of snow petrel. Indeed, breeding success of snow petrels depends on sufficient body condition of the females, which in part reflects favorable environmental and foraging conditions prior to the breeding season [Barbraud and Chastel, 1999]. This is the first time that appropriate areas of the observed foraging range are used to study the carry over effect of winter conditions on the breeding performance of snow petrel, as this information did not existed previously.

L507: What is "AIC"? the Akaike's Information Criteri, this is now noted in the text

L513-526: - What is given at the x-axis of Figure 11. What is "pack ice bootstrap"? Are we looking at an anomaly? - The text (here and in the previous paragraph) speaks about AIC difference < 2 or > 2. Difference to what? What do the AIC values in Table 5 tell me? - The NT MIZ AIC value isn't that far away from BT pack ice AIC; why NT MIZ is not supported then? - The authors could use the same number of digits for all AIC values ... but could ask themselves how accurately AIC can be determined ... 1/100

or 1/10? - So what the authors wish to tell the reader here, in short, is that using BT pack ice fits best in relation to breeding success while using NT MIZ would have led to a wrong conclusion. NT pack ice and BT MIZ would have given in-different results. While the reviewer kind of likes the result (because it is known that BT provides more realistic sea ice concentration in the Southern Ocean than NT) the reviewer is wondering about i) how sensitive this results is with regard to application of a different model and ii) how the results would look like if the threshold sea ice concentration used to delineate MIZ and pack ice would have been 70% or even 60%? Yes the x - axis on figure 11 is the area of pack ice calculated with the bootstrap algorithm, and expressed as proportional anomalies relative to the mean over the observation period (1979- 2013). We have now specified that in the legend of Figure 11 and added line 506 that " sea ice covariates (MIZ and pack ice areas) were expressed as proportional anomalies to the mean, with xa(t)=(x(t)-xm)/xm; with xm the average value from 1979 to 2013. "

The Akaike Information Criterion (AIC) is a way of selecting a model from a set of models based on information theory [Burnham and Anderson, 2002]. AIC is largely used in the biological sciences but it has remained largely unused outside of this fields. The few examples that can be found in other fields, include the pharmacological sciences and social sciences (see the application to wines and policy by Snipes and Taylor, 2014). The chosen model with the lowest AIC is the one that minimizes the Kullback-Leibler distance between the model and the truth. It is defined as: AIC = -2 (ln (likelihood)) + 2 K where likelihood is the probability of the data given a model and K is the number of parameters in the model. AIC scores are often shown as ΔAIC scores, or difference between the best model (smallest AIC) and each model (so the best model has a ΔAIC of zero). We have now added in the legend of Table 5 the following sentence to clarify the model selection: "AIC scores are often interpreted as difference between the best model (smallest AIC) and each model referred as ΔAIC. According to information theory, models with ΔAIC<2 are both likely [Burnham and Anderson, 2002] but if a model shows a ΔAIC>4 it is unlikely in comparison with the best model (smallest AIC). In our following model section, the model with the lowest AIC (highligthed in gray) includes Pack ice calculated with the Bootstrap algorithm as a sea ice covariate. If AIC are sorted from lowest to highest value, the next model includes the sea ice covariate MIZ calculated with the NASA algorithm. It shows a ΔAIC $\sim$8 from the best model, and thus is not well supported by the data in comparison to the best model. "

We have explored the various combinations of sea ice covariates, so our model selection is complete and robust. We could have explored non linear relationship, but we do not have a priori knowledge of the mechanisms that could lead to such non linear relationship. We could have implemented several covariates in the same models. However, it is not statistically appropriate in our case because sea ice covariates are strongly correlated. We have not evaluated how sensitive the results would be if the SIC for the MIZ changes.

L528-551: While what the authors write in these two paragraphs is for sure right the reviewer has difficulties to relate this to the main topic of the present paper - and in particular to the link with biology. We have edited to make more relevant but we wanted the discussion to also focus on other aspects that may be of broader interest.

L552-566: Here the authors try to establish the link to their paper. Suggestions are: - to clearly differentiate between sea ice extent and sea ice area. The latter includes sea ice concentration information and therefore is much better suited than sea ice extent to investigate changes and variability of the nature of a sea ice cover. Our values for the different ice categories are area as open water is accounted for when computing the area averages.

L557: There are other studies which could be mentioned here, which focus much better on polynyas and their temporal development and/or associated ice production, e.g. Tamura et al., 2008; Kern, 2009; Nihashi and Ohshima, 2015 - in L564-566: Holland and Kwok, 2012, would give enlightening information. We prefer not to go into that line of detail as it is outside the scope of the present paper.

L581-588: The reviewer suggests the authors make the notion that the BT used in the

Arctic is not the same as is used in the Antarctic - nut just in terms of the tie points but in terms of the used frequency combination. The results are therefore not compatible 1-to-1. I do not believe that is the case as tie points are computed from the BT algorithm on a daily basis using scatter plots of Tbs.

L594-595: Such a statement might need a reference. Isn't also most of the Antarctic sea ice snow covered? Yes most of the Antarctic sea ice is snow covered, but it is also prone to flooding, which makes the emissivity different than for dry snow on sea ice. The NT algorithm selects different tie points for each hemisphere which assumes a specific type of sea ice (generally snow covered). However work we have done shows the SIC is underestimated for thin ice, or bare ice.

L595-600: As with regard to tie point selection the reviewer recommends to perhaps take another look into relevant literature. While it may be correct that NT uses predefined tie points it is also correct that the BT needs to define tie points which is done based on the actual observations, yes, but except in a very few cases or when applying the BT regionally as e.g. in the Southern Ross Sea where thin ice exported from the Ross Ice Shelf dominates the scene, it is likely that the sea ice is snow covered as well. The BT algorithm selects tie points each day based on the distribution of the brightness temperatures, so they change day to day, whereas the NT tie points are fixed.

In particular the sentence in L597-600 is then perhaps a sub-optimal statement. - The reviewer is wondering whether the authors can give a reference for the "seasonal variations in emissivity can be very large"? We added 3 references

L601-619: Finally the authors come up with at least one of the key papers of results from intercomparison of NT to BT sea ice concentrations in the Antarctic (e.g. Comiso et al., 1997). One could also take a glimpse into Comiso and Steffen, JGR-C, 2001. Done One uncertainty factor for sea ice concentration retrieval is indeed the snow cover influence for which it is stated in the literature that the vertically stratified snow cover complicates sea ice concentration retrieval by the NT due to the stronger sensitivity of the 19 GHz channels to these effects - which is the main reason for the occasionally observed strong underestimation of the sea ice concentration by NT over pack ice compared to BT (Comiso and Steffen, 2001) and which is the main reason why the enhanced NT algorithm was developed (Markus and Cavalieri, 2000). The second factor is the different influence of thin ice on the sea ice concentration. Thin ice creates a negative bias in the sea ice concentration obtained as is illustrated, e.g. in Ivanova et al., 2015, and in Shokr and Kaleschke, 2012. On top of these the authors could mention other issues like gap layers, ice-snow interface flooding, formation of meteoric ice, snow metamorphism which all may or may not have an influence on the sea ice concentration which - to the reviewers' knowledge - nobody as quantified yet for Antarctic sea ice. We now mention these factors L606-610: This excursion to the Arctic seems confusing and could be deleted. We were trying to relate this to the reason why the MIZ differences between algorithms is larger in the Arctic than in the Antarctic. We made this statement clearer..

-L613-614: While one could agree to this notion one could also make the point that the definition of the MIZ via the sea ice concentration can be a very vague estimate. It can well be that even with 100reds of contemporary scenes with high-resolution optical imagery or SAR images one will not be able to better "validate" where the MIZ stops and pack ice starts. The reviewer has rather the feeling it is a matter of definition - and sea ice concentration might not be the ideal means for this.

References: Parkinson et al., 2012 in L53 not in refs. Should be Parkinson and Cavelieri, corrected.

Bintanja et al., in L72 has year 2012 in text but 2013 in refs. Should be 2013

Kohout et al. 2014, in L81 not in refs. Added

Ferrari 2014 in L107 is Ferrari et al. 2014 in refs. Changed

Ivanova et al., 2015 in L113 is now in The Cryosphere We could not find the final pub-

lished version doing a google search, all results come as as Cryosphere Discussion.

Loeb et al. in L488 is 1993 in text but 1997 in refs. Should be 1997

Polvani and Smith, 2013 in L534 not in refs. Added

Hobbs et al., 2015, in L546 not in refs. Added

Ivanova et al. appears as Ivanova and others in the refs.

Steig et al. appears as Steig et al. in the refs. It is because there are more than 20 authors, if TC needs them all listed I can do that, but generally with that many authors, et al., is ok in the references. Kohout and Meylan, 2008 not used in text. Louzao et al. 2011 not used in text. Typos: L114: "mattes" –> "matters" Thanks for catching these, they have been corrected.

Anonymous Referee #3 Received and published: 19 April 2016 The manuscript examines the variability and trends of Antarctic sea ice in three cat- egories: pack ice, marginal ice zone and coastal polynya derived by Bootstrap and NASA Team algorithms. Authors show that the differences in trends and variability be- tween these two datasets are quite large for pack ice and marginal ice, even though the differences in the total extents are relative small. The details within the ice pack is quite essential for the atmosphere-ocean-sea ice interaction, as well as biological studies, such as the study on snow petrel presented here. The manuscript reveals an important fact that satellite observed sea ice concentration can contain large errors and biases, and should be used with caution. I think that the study contains valuable information for polar community. However, I see some places need to be clarified. In general, the manuscript is rather tedious. Authors should reduce the discussions on insignificant results and focus on important and significant results. It is worth to be published after a minor revision. We thank the reviewer for their helpful comments. We agree at times the manuscript is tedious and it has been a difficult balance to highlight the importance of the results while also quantifying them. While Reviewer 1 found the paper quite easy to read for a non-specialist we also see this Reviewers point. We have tried to streamline the discussion as much as possible in our revision. The research represents an original effort to evaluate SIC quality by comparing two datasets in marginal ice zone and pack ice. It fits well the scope of TC. The methodology is sound and conclusions are sufficiently supported by analyses. However, the title is not quite accurate. The study cannot conclude on the trend and variability of MIZ since the discrepancies between two data products are quite large. The manuscript focuses on the discrepancies in MIZ, pack ice and coastal polynyas between data products. The title should reflect on that. We thank the reviewer for their comment. While we see the reviewers point, we feel the title remains appropriate given that we are assessing their variability. We added to the title the mention of looking at two sea ice algorithms. The title now reads: Mapping and Assessing Variability in the Antarctic Marginal Ice Zone, the Pack Ice and Coastal Polynyas in two Sea Ice Algorithms with implications on Breeding Success of Snow Petrels Technic/editorial issues. Figure 3. It would be helpful to add longitude lines to separate the different regions. In addition, bird study areas also need to be marked in this figure. Done

Line 306-317. The figure 7 needs some clarification. Is the expansion and contracting of outer ice edge relative to a zonal mean? If it is the case, what is the zonal mean of MIZ, in consideration of that the zonal mean pack ice is the mean of 85% and outer edge is the mean of 15%? It is relative to 60S. We have added that to the caption. Line342. Insert “area of the” or “extent of the” before pack ice. done Line 371, “While the sign of the Ross Sea sector trends from” done Figure 9 need some work. It would be very helpful if authors add contours of a confidence level in figure 9 so readers can see where and when significant trends occur. All x-axis and y-axis labels need to be enlarged, so they are visible. We have updated Figure 9 to have enlarged x and y axis and included the confidence level Page 10, There are many detailed discussions and the key results don’t stand out. Authors should focus on the significant trends and their implications. We thank the reviewer for their comment but not entirely sure what the reviewer is after from this comment. Nevertheless, we have made some modifications

that we hope clarify and add some more quantitative analysis to this section and add some significance of the results. Page 11, section 3.2.3. Authors need to explain how figure 10 is calculated. It should

be seasonal mean and zonal mean, right? In addition, it would be nice to comment on whether the results derived from the width are consistent with or different from the results from ice extent/area presented in the early section. Figure 10 was calculated as follows: Step 1: We classify each 25 km pixel according to its class: MIZ, Pack Ice, Interior broken, Polynya. Step 2: Scan from north to south until the transitions between ocean to MIZ, MIZ to Pack Ice, Pack to Polynya or coast. Step 3: Record the latitude of the transition for each day. Step 4: Compute average latitude for each month at particular longitudes. Step 5: Convert monthly mean latitude boundaries to width in km. Step 6: Sum up for all longitudes. Step 7: Compute seasonal means.

We updated the text to read: Finally, we compute the overall width of the MIZ and pack ice following Strong and Rigor [2013] and produce seasonal means. Briefly, following the classification of each ice type, latitude boundaries are computed for each longitude and each day. These are averaged for each month to provide monthly mean latitude boundaries at each longitude. The boundaries are subsequently converted to width in km, and averaged for all longitudes. Finally, seasonal means are derived. Line 507. Please define “AIC”. It is Akaike’s Information Criterion and a common metric used in ecology for model selection. We have added this information to the paper. Page 13, Discussion. It is very clear that SIC derived from these two algorithms has large differences within the ice edge. To readers who use SIC for various studies, it is very important to know which product is more suitable for their need. It would be useful if authors could comment on whether there are other independent verifications? The snow petrel study is an excellent example. However, it is only related to the packed ice. We agree that it is important to know which product is more suitable for their need and we are not aware of any validation studies addressing these different ice types. However, we plan to do a validation exercise with visible imagery this year but it is outside the scope of this current paper and will likely be submitted to a remote sensing journal. Instead in this paper we focus on the fact that different results are obtained based on which algorithm is used, which is important to keep in mind when doing biological studies like this.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/tc-2016-26/tc-2016-26-AC1-supplement.pdf

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Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-26, 2016.