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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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Review of

Mapping and assessing variability in the Antarctic marginal ice zone, the pack ice and coastal polynyas

by

Stroeve et al.

Summary: Sea ice concentration data sets obtained for 1979-2014 with two different algorithms (NASA Team and Comiso Bootstrap) from satellite microwave radiometry are

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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 distribution 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. 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? 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. 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. 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.

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

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paper - which is currently missing in the abstract.

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.

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.

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.

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?

L83: "longer and larger ice-free summer". What is meant by "larger ice free"?

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. Evven 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).

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

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.

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.

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".

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)

L145: "heavily influenced" Could this statement be precised?

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?

L152: Work carried out previously used different sea ice concentration tresholds: e.g.

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%.

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). 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?

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? 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?

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.

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

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 kmˆ2.

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

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.

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?

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).

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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].

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"? - 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".

L214: "ice types" –> "ice categories"

L223: Seems the peak extent for NT is in July.

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

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.

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

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

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

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.

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? 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. - 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.

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?

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.

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.

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?).

L373: add "significant" after "statistically".

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.

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

L390: Not even the large negative ones in the Weddell Sea in NT?

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.

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.

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.

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).

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.

L426-427: This notion is not reflected by Figure 9. The reviewer again suggests to skip the ice category "polynya" in this figure.

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. - 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? - 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. - in L453: Perhaps referring to Figure 7 would be a good idea to underline this notion? - in L455: The authors could add that they are still referring to NT here. - 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.

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.

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.

L507: What is "AIC"?

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%?

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.

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. - in 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.

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.

L594-595: Such a statement might need a reference. Isn't also most of the Antarctic sea ice snow covered?

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. 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"?

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.

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. - L606-610: This excursion to the Arctic seems confusing and could be deleted.

-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. Bintanja et al., in L72 has year 2012 in text but 2013 in refs. Kohout et al. 2014, in L81 not in refs. Ferrari 2014 in L107 is Ferrari et al. 2014 in refs. Ivanova et al., 2015 in L113 is now in The Cryosphere Loeb et al. in L488 is 1993 in text but 1997 in refs. Polvani and Smith, 2013 in L534 not in refs. Hobbs et al., 2015, in L546 not in refs.

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

Kohout and Meylan, 2008 not used in text. Louzao et al. 2011 not used in text.

Typos: L114: "mattes" –> "matters"

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