We are grateful to the reviewers for their time and constructive comments to improve this manuscript. Here we address our reply point by point, in bold font. Reviewer's comments are in regular font. All changes are marked with red in the revised manuscript.

Reviewer 1:

This paper uses altimetry and bathymetry data to map changes in the grounded area of the Mertz ice tongue during the early-to-mid 2000s. They use the surface heights of lightly-grounded icebergs to estimate the firn air content for the ice tongue, and use this, geoid-corrected altimetry measurements, and a bathymetry map of the ice shelf, to map the difference between the bottom of the ice shelf, at hydrostatic equilibrium, and the sea bed for different time periods. Areas where these maps show the (hydrostatic) ice bottom below the seabed are treated as grounded. The authors find that the northwest flank of MIT (Mertz Ice Tongue) was grounded during 2002-08, and that the grounding increased between 11/2004 and 12/2006. They propose that the MIT would have calved because of increasing grounding extent even if the tongue had not been hit by an iceberg. The authors also examine the rate of change in the area of MIT, and estimate an interval between subsequent tongue calving events of 70 years.

A quick summary of this review is that this manuscript needs a great deal of editing and extensive revision before it is ready for publication. I have presented some of my thoughts about what needs to be fixed, but the writing is of uneven quality and my comments about the reliability of interpolated data should (in my opinion) lead to substantial changes in the text and figures. With this in mind, I have not gone to the effort of editing the paper in detail, and hope that the authors can do so themselves.

Reply: Thank you for your comments. We have made many revisions to the structure. Please find related changes highlighted in red in the text.

A major problem in this manuscript is the lack of bathymetry data under the MIT. Data are scattered, at varying density, seaward of the ice front, and the bathymetry maps appear to resolve the seaward extent of the Mertz Bank, but under the tongue the maps are entirely based on interpolated values. This makes the maps in figure 5 and the statistics in table 2 hard to believe except at the very edge of MIT where the altimetry and bathymetry more or less coincide. The conclusions of the paper are largely independent of the data everywhere except in the area where the data are credible, which makes me wonder why the authors chose to show the mapped elevation-difference values in the areas for which they have no data. The authors should make a clear distinction between results derived from measurements and results derived from interpolated values, and the relative expected accuracies of each.

Reply: Through further reading the references about GEBCO and ETOPO1 seafloor DEM, we do find there exists a bathymetry data gap under the MIT as the reviewer has pointed out. According to Beaman et al. (2011), the oldest bathymetry data collected along the margin of the MIT was at least from 2000. Thus, the boundary of the MIT in 2000 is used to identify bathymetry measurement gaps, as is indicated in Fig. 6. We want to use this boundary to identify the different quality of the seafloor DEM data.

As far as we know, there are no other new bathymetry measurements which can be used to verify the region of data gaps. Thus a further validation of the seafloor DEM is not conducted. Luckily, the ice tongue moved further into the ocean from 2000 to 2010 before calving, flowing over seafloor where bathymetry measurements density is good. Furthermore, the grounding detected is all located beyond the 2000 MIT boundary. Thus the analysis of grounding detection near the ice front in 2002, 2004, 2006 and 2008 is convincing. In this revision, the grounding detection result from Fig. 6 (Fig. 5, last version) is unchanged. However, the boundary of MIT in 2000 is now added to separate the seafloor area under the MIT where the seafloor was interpolated from the surrounding area where bathymetry measurements have been made. The statistical result of grounding detection in Table 2 is recalculated accordingly. Detailed discussion on the seafloor DEM is added in a newly added section 6.2.3.

A second problem is that the methods are difficult to interpret, in large part because the report cited as "Wang 2014" is not readily available online, and the paper "Wang et al 2014" appears to describe a method for estimating freeboard change, rather than the absolute freeboard used in the present study. A good deal of the material in this paper is based on a technique in that report, described briefly in section 3.1(126-135). This paper should include a full description of the technique. In particular, it is not clear from the description how the relocation step works or what it is supposed to accomplish, or how the surface slope relates to errors in this relocation (line 241). I would also have liked to see a justification for the kriging interpolation between ICESAT profiles; the grounding features appear to be small compared to the gaps between ICESAT tracks, which makes me suspect that the krigged freeboard values may not provide a good indication of grounding.

Reply: For freeboard production, we did not cite Wang 2014, but Wang et al. 2014, which did show how to use ICESat/GLAS data to produce a freeboard map in 2009 before MIT calving. In the revision, more details on the freeboard production method are given in Section 3.1. Uncertainty of kriging interpolation using ICESat/GLAS data is investigated as well which is about ±1.8 m on average in a new Section 4.1. The uncertainty of kriging interpolation is also considered when calculating the final grounding detection accuracy in Section 4.

The authors should also be clear about the tidal values used in the freeboard study. Are the altimetry values corrected for tides? What is the "lowest sea level" mentioned at 155, and elsewhere? Is it derived from a tide model, or is it the lowest observed sea level? Is the tide model on the ICESAT product used, or is a different tide model used? What are the errors involved in each part of this?

Reply: More details on ICESat/GLAS preprocessing and the method to produce the freeboard map has been given in the revised text. Tide correction of ICESat/GLAS GLA12 from TPX07.1 is removed to obtain the instantaneous sea surface condition. "lowest sea level" in line 155 may be confusing and has been changed to "lowest sea surface height among extracted sea surface height from different tracks and different campaigns, which is -3.35 m". It is derived by comparing all sea surface height derived from different tracks and campaigns from 2003 to 2009, not from a tide model. The lowest sea surface height stands for the lowest sea level around Mertz from 2003 to 2009 and is directly from ICESat/GLAS observation. Sea level lower than -3.35 m may in fact exist over the Mertz region since limited ICESat observation in any year may not catch the lowest one. The influence of sea level -3.35 m used in this study is discussed more in a new Section 6.2.1.

The English in the manuscript needs improvement. A few idioms are used throughout that are confusing or distracting. "Inversed" should be "inverted." "Area-changing rate" should just be "area rate." "Least-square" should be "least-squares." Activities in the current study should be in present tense, citations to the literature should be in past tense.

Reply: These grammatical issues have been resolved. The error or uncorrected expression pointed to by the reviewer has been corrected. More unclear descriptions or grammar misuses have been corrected as indicated in red in the revised text.

The FAC calculation (3.2) has some nice features, but needs to be described in more detail. How is the least-squares inversion carried out? What are the error sources?

Reply: More details about FAC calculation have been given. Please read Section 3.2 and 6.2.2.

160-177- is the extensive discussion of other methods of calculating the FAC germain to this study? This section would be clearer if much of this were omitted.

Reply: The introductory part of Section 3.2 has been revised. One paragraph not related to the FAC calculation much has been removed. Please read the revised Section 3.2.

222-229- this paragraph should be in the introductory part of section 3.2, not after the calculations have been presented.

Reply: In the introductory part of Section 3.2, the principle of FAC has been given. However, we want to use this text to discuss limitations of the FAC as calculated from these selected icebergs. In the revision, this paragraph in question is now moved to Section 6.2.2 as part of a deeper discussion on FAC extraction.

247: Where are the interpolation errors for freeboard and bathymetry?

Reply: The influence of kriging interpolation is discussed in Section 4.1. Also the uncertainty of kriging interpolation is derived and now considered in the final accuracy of grounding detection.

From Beaman et al. (2011), the poorest accuracy of single beam and multi-beam measurements was provided. Thus, we use it directly to evaluate the accuracy of the data in Section 4.1. Because the original bathymetry data product from Beaman et al. (2011) and Fretwell et al. (2013) was not collected and processed by us, it is impossible to evaluate the uncertainty of the products. As far as we know, there is no other new bathymetry measurements that can be used to evaluate the seafloor DEM. Thus, for seafloor DEM, we just use the poorest accuracy to reflect the uncertainty of seafloor DEM. The seafloor DEM is further discussed in a new Section 6.2.3.

254- 50 times the average slope is still a very small number (0.6 degrees). A better estimate of the error due to crevassing would be to directly incorporate the crevasse depth into the calculation- thus, instead of v*slope_error (12 m) the contribution would be closer to 50 m.

Reply: Crevasses are important features on the surface of the MIT. In the middle of the tongue, large crevasses can reach a depth of about 50 m. However, this is an extreme and rare occurrence. Around the ice front, the freeboard is about 30 m, as can be seen from Wang et al. (2014). It is therefore not proper to set the crevasse depth in that area to be as large as 50 m. The freeboard error caused by our approach is reasonable because we want to explore the average contribution to grounding detection from footprint relocation by considering ice velocity uncertainty and average surface slope. In this study, we have already magnified it by 50 times. As we feel this is a reasonable approach for the ice front treatment, we kept our original approach in the revised manuscript.

256-62: Why do we need to consider the freeboard stable (or not stable?) It appears that only static estimates of freeboard are used here (derived from single ICESAT campaigns)- so why does it matter that there would be a change (or not) in the freeboard?

Reply: Greater details about the method for freeboard extraction, relocation and mapping are added in Section 3.1. Because we use all ICESat/GLAS data from 2003 to 2009 to produce freeboard for different years, freeboard changes do matter if freeboard changed greatly. Thus,

the uncertainty of freeboard change rate does contribute to the final accuracy of grounding detection.

257 "annual changing rate of freeboard" should be "annual rate of freeboard change" or "freeboard rate"

Reply: Done

273: What is the significance of Edif < 34 m? Based on 263-268, this would indicate "not extremely confidently identified as ungrounded." Wouldn't a better statistic be Edif < -34, or "Extremely confidently identified as ungrounded?"

Reply: After considering the interpolation error, the accuracy for grounding detection is now ± 23 m (± 17 m in last version). We provide statistics for those elevation difference with E_dif less than 46 m (twice the standard deviation) so one can have a better estimate of grounding at the tongue. When using E_dif less than -46 m, the slightly grounded sections will be neglected. We use this value of 46 m to describe all possible grounding regions. Furthermore, the statistics in Table 2 do show results in different intervals from 46 m to less than 0.

280: Again: Do you mean "less than -17 m?"

Reply: We mean "less than 23 m" because in 2002, the minimum of 'E_dif' is larger than "-23 m". In this revision, '17' is changed to '23' because of a revised consideration of the kriging interpolation error.

291-293: Reporting Edif within the tongue is a problem, since the bathymetry is not known there. You might report changes along the margin, but the statistics reported here don't seem to mean anything.

Reply: We actually want to express the 'E_{dif}' for those regions listed in Table 2 only, not all regions under the tongue. These regions with 'E_{dif}' less than 46 m do fall beneath the ice front of the MIT. In this revision, we change it to "From 2002 to 2008, more regions under the MIT have 'E_{dif}' less than 46 m the area of which increased from 8 km^2 to 17 km^2. Additionally, the mean of 'E_{dif}' under the tongue for those having 'E_{dif}' less than 46 m gradually decreases from 28.8 m to 12.3 m, according to which we can conclude that the ice front was grounded more significantly with passing time. "

302 Combine the first two sentences, which form a joint conclusion: ". However," should be ", and that"

Reply: Done.

325 (and elsewhere) "Area-expanding trend" should be "area rate" or "rate of area change"

Reply: Done.

367-78: The significance of this paragraph is not clear. Ice-berg scouring is not discussed elsewhere in the paper, so the scientific question addressed by this paragraph needs more introduction.

Reply: This section is removed.

577- "is used in figure 6"– this appears not to be true.

Reply: Changed it to 'Fig. 4'

589: "closed' – should this be "closest?"

Reply: Done.

594: The legend here is not consistent with the caption.

Reply: Legend is changed.

606: It is hard to distinguish the outline from the "grounding part." The choice of colors (yellow on yellow) is not good.

Reply: This figure is redrawn and yellow lines are not used in this version.

Reviewer 2:

The authors use bathymetric, ICESat, and Landsat data products to estimate the firn air content, depth below sea level, re-grounding locations, and advance rate for the Mertz Ice Tongue from 2002-2008. They find that grounding along the Mertz Bank resulted in slight rotation and rifting of the Mertz Ice Tongue that would have resulted in the ice tongue's eventual collapse in the absence of any additional triggering mechanisms. Further, they suggest that the ice tongue collapse has a periodicity of ~70 years and that this periodicity results in periodic variations in local sea ice formation and bottom water formation. Although the topic of the manuscript is interesting, the limited presentation of the methods and irregular quality of the writing make it difficult to follow. In addition to the major revisions listed below, I recommend that the authors go through the text in detail to check the writing and to make sure that all figures are legible.

Reply: Thanks for your comments. We have thoroughly revised the manuscript. The changes are highlighted in red in the revised text.

1) In the data and methods sections, the authors frequently refer the readers to other publications rather than describe the data processing procedures in detail in the text. I find this to be particularly concerning for the freeboard inversions to estimate ice thickness because small errors in freeboard can lead to large variations in the estimated tongue depths. In order to have confidence in the provided tongue depths, I recommend including more detail on the relocation and interpolation procedures. Similarly, more information regarding the uncertainty of the bathymetry data used to identify grounded regions would be incredibly helpful.

Reply: More details about freeboard map production using all available ICESat/GLAS data from 2003 to 2009 is added in a revised Section 3.1. More discussion is added as well in a new Section 6.2.3.

From Beaman et al. (2011), the poorest accuracy of single beam and multi-beam measurements was provided. Thus, we use it directly to evaluate the accuracy of this data in Section 4. Because the original bathymetry data product from Beaman et al. (2011) and Fretwell et al. (2013) was not collected and processed by us, it is impossible to fully evaluate the uncertainty of the products. The seafloor DEM is further discussed in the new Section 6.2.3.

We do acknowledge that in regions with bathymetry gaps, the quality of seafloor topography is poorer compared to other regions. According to Beaman et al. (2011), the oldest bathymetry data used to produce the seafloor DEM that are already known was at least from 2000. Thus, the boundary of the MIT in 2000 is used to identify bathymetry measurement gaps, as is indicated in Fig. 6. We use this boundary to identify the different quality of the seafloor DEM data since as far as we know, there is no other new bathymetry measurements

that can be achieved to verify the region of data gaps. Luckily, the ice tongue moved further into the ocean from 2000 to 2010, before calving, into regions where bathymetry measurements are good. Furthermore, the grounding we detect is all located beyond the 2000 MIT boundary. Thus the analysis of grounding detection near ice front in 2002, 2004, 2006 and 2008 is convincing.

2) It's really difficult to follow the firn air content approximation. I assume the bed elevations are really well constrained under the targeted icebergs and you are simply iteratively estimating the iceberg depths for gradually decreasing values of the mean iceberg density. The units obtained for the firn air content estimated using this method require explanation. I assume that they represent the difference in iceberg depth assuming a constant ice density and the final ice density estimated through the comparison with the underlying bathymetry since the units are in meters, but this is not presented anywhere. It would be helpful to also present the final density inferred for the firn column so that it is easier to compare your estimates with other observations. The error estimates obtained for firn air content should also be presented in more detail. I am particularly concerned with the assumption that the ICESat tracks capture the thickest portion of each iceberg. I'd be more confident in the firn air content estimates if I was also shown that there are relatively small variations in iceberg freeboard along the ICESat tracks because that would increase confidence that the iceberg grounding location is captured by the ICESat data.

Reply: The method for FAC calculation has been revised in Section 3.2. The seafloor DEM is well controlled by the bathymetry measurements as can be seen from S-Fig. 1. A paragraph on why FAC is used and how it is obtained is provided in Section 3.2. Some text not related so much to FAC calculation has now been removed. Fig. 9 is added to show the spatial distribution of freeboard of icebergs. More details on freeboard measurements from ICESat/GLAS, and the limitation of our method for FAC calculation is discussed in Section 6.2.2. Our estimated FAC around Mertz is compared with published modeling results (from Ligtenberg, 2014) in Section 6.2.2.   Calculating the average density directly is beyond the scope of the current manuscript.

3) The addition of the iceberg scour section at the end of the discussion is somewhat out of place with the rest of the manuscript. I suggest removing it entirely.

Reply: This section is removed.