This manuscript reports the new bed topography and associated predicted locations of the oldest ice in the Dome Fuji region. The data presented here have a high value; it is the first modern regional airborne surveys that are necessary to characterize this region. In this regard, this paper will be a main reference point for future work in this region and is worth to be published. However, in my opinion, this manuscript doesn't present full information package and individual conclusions are weakly supported by the analysis. I would like to see more comprehensive descriptions of the methods and data so that one can evaluate the work and utilize the results in an appropriate way. Therefore, I would recommend a major revision and I hope that my comments below are helpful.

1. The main shortcoming of this paper is incomplete description of the survey, data processing, and data properties. Main lacking information is:
    a. Survey: line spacing (mean, or range/max/min, it's hard to read from the figure), flight mode (constant elevation or constant clearance to the ice), nominal data-collection spacing along survey profiles.
    b. Data gridding: see #2 below.
    c. Lake detection: see #3 below.
    d. Continuity index: depth range analyzed for the index, see #5 below too.
2. The presented ice thickness map could include many artifacts due to unrealistically small grid size and smoothing to a large degree. Is the grid size of 0.5 km justifiable? Radar profiles are separated by > 5-10 km, and the authors admit that the large standard deviation (142 m) between gridded values and previous German/Japanese radar data is inevitable result of the smoothing (P4L11). I am afraid that this grid size gives a false impression that the bed topography is known better than we actually know. No details are given about smoothing or data interpolation. This is a key aspect of the final product presented here, however.
3. Very little information is given about lake identification. Nobody can reproduce or assess author's arguments in a meaningful way. Radargrams of at least several lakes should be presented. Do they look like definite lakes, or more like dim lakes? Are all known lakes along the new radar profiles found in this study?
4. Sections 2.2 and 2.3 should be better organized. Section 2.2 are written in a way to examine uncertainty of the new data but also to examine old data, which is confusing for me. Section 2.3 presents spatial patterns of the difference between OIR and BEDMAP2, which can be better presented if Sections 2.2 and 2.3 are more efficiently organized/integrated.
5. Continuity index presented in Fig. 5 should be shown only along the radar profiles and should not interpolate between profiles.
6. It is necessary to show radargrams that represent several typical features in this region (e.g., region with high continuity index, lake, transition from the cold base to thawed base) together with continuity index, lake appearance, Delta G, etc. The manuscript shows only limited aspects of the data properties and it is hard for readers to make a comprehensive assessment.
7. One of main conclusions is that the most promising sites for the oldest ice is in the box I and II. I agree with the authors about this conclusion in general but data should be better presented to fully support this argument. First, why do the authors recommend two separate regions Box I and Box II, instead of one large box that includes both I and II? Second, what defines the

boundaries of these boxes? These two are just examples showing the lack of full argument of the main conclusion.

8. Please enlarge figures, and consider grouping some figures into a single figure (panel a, b, c, ….). it's hard to compare several data properties together in the current form.
9. Mention data availability.

More detail comments:

Figure 1: include surface topography (with contours or background color) in both panels. I recommend enlarge both panels for clarity.

Figure 2: it's hard to distinguish fully saturated colors and half-transparent. Please modify the figure.

Figure 3: The candidate sites should have Delta G > 5 mW/m2. Is it better if the colorbar shows Delta G between 5 and 18 (instead of 0 and 18)?

Figure 4: Consider larger intervals of the contours. Many features are smaller than the radar-profile separations and thus could be artifacts. It's hard for me to see water paths.

P3L22: "Echo-free zone" is defined as the deep ice on which radar returned power decreases in a short vertical distance much more than expected with geometric spreading and englacial attenuation (Fujita et al., 1999). The echo-free zone is not identical with a zone that shows no return signals, which can be caused by limited radar performance.

P3L25: Is this true? I think that it is the case, but there is no evidence. Probably better to say something like "because the data were collected in late 1980s without modern GPS positioning system."

P3L29: Pulse widths determine a vertical resolution (capability to distinguish two objects separately) but irrelevant to the precision of the range measurement. The observed elevation change is about 0.01% of a typical ice thickness in the study area (2 km), which is below the known accuracy of the radio-wave propagation speed.

P4L5: show the number of cross-over points analyzed here.

P4L3: please mention clearly at the beginning of this paragraph "the uncertainties are analyzed by cross-over analysis of the new datasets and by the comparison of the gridded map product with previous data that were not used in the map product." Many uncertainty analyses are presented here and it's hard to see the overall analytical setup.

P4L8-9: consider showing a histogram of cross-over errors. Also, it makes more sense to show first and third quartile, instead of the standard deviation.

P4L10: unclear what negative number means here (which data were subtracted from which).

P4L11: flightlines? Confusing because JARE data are ground based.

P4L13-19: Comparison with the BEDMAP2 data shows large discrepancy, and the authors argue that it mainly caused by poor positioning of the earlier Soviet data (again: no evidence of the poor positioning). So, these statements are not for determining uncertainties of the newly collected data.

P4L24: characteristics of what?

P5L15: include the mean and range of geothermal flux in the study area taken from these datasets. I believe that the geothermal flux is a predicted field of this model (rather than an input field) and these geothermal flux datasets are used to derive delta G. So, I think that geothermal flux is mentioned at a wrong place.

P6 Equation: it's confusing to see G_min and G_mean, as G_min is a model prediction and G_mean is the mean of three datasets. How about G_min(predicted) and G_mean(datasets) or such?

P6L2: Why is Sigma_G < 25 mW/m2 considered as low? Geothernal flux in this region is roughly 50 mW/m2, so I understand that the standard deviation of 25 mW/m2 means that the geothermal flux has +/- 50% uncertainty. In other words, the margin of geothermal flux with which the bed is kept frozen is much less than the geothermal flux dataset uncertainty (i.e. Delta G of 5 mW/m2 is much smaller than the range of the dataset, 25 mW/m2). I understand that the full description of this modeling is given in Van Liefferinge and Pattyn (2013) but still key information should be given here.

P7L10: don't use several words in interchangeable ways; Lakes, wet areas, water-filled areas, ….