As a scientist who has worked in the Amerasian Arctic for many years, I am in the best possible position to appreciate the effort expended by the authors of this manuscript. I am absolutely positive about the importance of this study on its own. I also think this study makes an important contribution towards reaching a plausible explanation of one of the most important issues observed in the Arctic, the degradation of sub-sea permafrost in the context of coastline retreat. Indeed, our ability to project future geofluid (including methane, carbon dioxide, hydrogen, water solutions…) leakages from the vast Arctic shelf into the water column largely depends upon the current state of sub-sea permafrost (Frederick and Buffet, 2014; Nicolsky et al., 2012-cited in ms).. Because the Arctic region lacks long-term and all-seasonal observations, reporting results of multiple geoelectric surveys taken in the same area along the same route during different times of year is invaluable. I would be more than happy if I could limit myself to only listing the advantages of this paper and recommend it for publication, but my role as a reviewer requires me to comment on downsides as well as positives. Unfortunately, I do see some downsides and I must report them.

First of all, I must say that the authors lack understanding of key sub-sea permafrost parameters which affect electrical resistivity. For example, assigning a resistivity of 15 Ohms to the frozen ground boundary is inconsistent with results that other authors have obtained in the Siberian Arctic. My Russian colleagues gave me the reference to a paper by Razumov and Dyatlov (1992) which shows that sub-sea taliks (thawed permafrost bodies) in Kolyma Bay of the East Siberian Sea are characterized by electrical resistivity from around 15 to 30 Ohms. Even changes made in response to the criticism of Reviewer #1 (responses shown on pages 1-2), assuming the range of transition between 10 and 100 Ohms, are still speculative; Ostercamp (2001) and Zykov (2007) demonstrated that in natural frozen sand, loam, clay sediments, and their combinations (in different proportions), electrical properties (electrical resistivity/electrical conductivity) can change by two-three orders of magnitude, from tens of Ohms in salty sand and sandy loam sediments to >1500 Ohms in sediments without salt. Apparently the authors do not realize that sub-sea permafrost is a highly heterogenous system which consists of a mixture of sediments (see details below), ice, and unfrozen pore fluids. Its overall physical and mechanical properties are determined by the various properties of its components and the relative proportions of those components. Because ice and unfrozen pore fluid and other physical and mechanical properties are strongly temperature dependent, any assumption remains arbitrary; only drilling can establish the rate of ice-bonded sub-sea permafrost degradation.

Second, the authors did not refer to recent drilling results obtained in the same area by Chuvilin et al. (2013, 2015), which show that thawed sub-sea permafrost is separated from ice-bonded permafrost by a transition layer of ice-bearing permafrost. In opposition to the authors' speculations (lines 170-188), Chuvilin et al. have shown that the sediment core composition includes sandyclay, fine sand, loamy sand, clay and silt, medium sand, silty sand, and/or fine-medium-coarse sand.

Third, I doubt that Figure 4b, which purports to show electrical resistivity for sediment pore water (based on eq. 1) from cores recovered from boreholes 101, 304, and 305, is correct. That figure shows that electrical resistivity in frozen core 101 ranges between near zero and 5 Ohms, while electrical resistivity in unfrozen core 304 (upper part) is >20 Ohms. This nonsense could be a consequence of using eq. 1; eq. 1 is useful for marine solutions, but the electrical resistivity of water extracts is calculated from sediments with a "terrestrial" signature (Kunitsky, 1989-cited in ms).

Fourth, in response to the criticism of Reviewer #1 (2nd page: "… no subsea permafrost drilling to validate the geoelectricaly estimated location of the IBP surface was performed. Without the actual validation, there are always speculations about the location of the IBP surface. No right or wrong, …. Nobody can give a definite answer without drilling and validating the geoelectrical survey"), the authors stated: "Drilling did in fact take place in the spring following our surveys (April 2012), and could provide data for comparison. Unfortunately, the data are under embargo until published by the study's lead author….". That response sounds strange to me, because I attended the invited report by Shakhova (Shakhova, 2015) at the AGU-2015, in which she demonstrated the actual rate of ice-bonded permafrost degradation based on 5 boreholes accomplished by other researchers in the same area in 2012-2014.

Fifth, the sentence on line 170: “No resistivities were measured on samples from this site, since no recent drilling took place” is misleading, because extensive multiyear geoelectric work (2012-2015) with validation by drilling was accomplished by Koshurnikov et al. (2015) in the same area: I saw his results presented at the AGU-2015.

The bottom line is: Everything, from interpretations of the observed data to conclusions, is rather hypothetical. (I refrain from using the unfavorable word speculative). Their hypothesis is very good, but unfortunately, the drilling work is lacking. I do not think this manuscript in its current state could be published in any scientific journal.

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PS. Note for authors: Having Russian co-authors makes it easy to obtain publications in Russian as cited above; I did so for many years.