Response to comments from Anonymous Referee #1

[Comment] *1. I would like to see a separate Discussion section following the results. The manuscript as it is now appears to not have such a section. Adding this section would allow for deeper discussion of the results, placing them in the larger scientific context and relating them to other studies. I'd recommend placing your answers to many of my remaining comments in a Discussion section. I think this will also make it easier for other readers to find this information, and will ultimately lead to more citations for your paper.*
[Response] Following the reviewer's suggestion, a separate Discussion section has been added in the revised manuscript.

[Comment] *2. Page 18461, line 8: The "m" parameter determining the width of the WTD distribution is critical to the amount of bias in estimated CH_4 emissions (Bohn and Lettenmaier, 2010). The authors have estimated this via a parameterization from Fan and Miguez-Macho (2011) that depends on climate and topography (from the GTOPO30 30-arc-second DEM) and has a calibration parameter "α". However, calibrating so that simulated saturated area matches observed saturated area does not uniquely constrain the WTD depth distribution – this all hinges on the grid-cell-spatial-mean WTD being correct, and any errors in spatial mean WTD could be compensated for by the calibration of α. The grid-cell-mean WTD is generated by VIC running with the parameters of Su et al (2006). However, Su et al (2006) did not explicitly consider peat soils – they used the same grid-cell-average soil properties over the entire grid cell, so that a cell with partial peatland coverage would at best have soil properties intermediate between mineral and peat. Considering the substantial differences in porosity and permeability between mineral and peat soils (e.g., Letts et al., 2000), we would expect that WTD using Su et al's parameters would yield WTD's having a) annual bias (do you think this would this be too deep or too shallow?) and b) greater seasonal fluctuation compared to peat soil in the wetlands (and opposite biases in the uplands, but methane emissions from uplands are much smaller than from wetlands). So, do you think there is a possibility that your empirically-derived values could be compensating for biased WTD's? If the WTD's are biased, what would you expect the bias in methane emissions to be* *(from this source of error)? Your thoughts on this topic could go into the Discussion section.*

[Response] We agree that the calibration of parameter α does not uniquely constrain the 5-km WTD distribution and it is true that any possible biases in VIC-simulated 100-km WTD could be compensated in the calibration processes. A paragraph (the first paragraph in Discussion section) has been added to explain and discuss possible error sources and their implications on the simulated WTD and methane emissions.

[Comment] *3. Page 18463, line 5: Did you compare your simulated WTDs with observations? How well did they match? I.e., given that you know the topographic wetness index of each point on the DEM that you used to generate the WTD distribution, you could predict the average WTD for the 5-km pixel containing each observation site. If you could provide some measure of the goodness-of-fit of simulated and observed WTDs, it would be very helpful. This would be appropriate in your Results section.*

[Response] Following the reviewer's suggestion, we extracted WTD for the 5-km pixel that contains a well. We then compared the simulated WTD with WTD observations at those wells. The comparison is described in the first paragraph of Results section. A new figure (Fig. 3 in the revised manuscript) was added in this revision.

[Comment] *4. How valid is the topographic wetness index (TOPMODEL) method in flat areas like the West Siberian Lowland or the Hudson Bay Lowland? Isn't this approach based on the assumption that WTD depends on topographic slope, which in turn depends on the assumption that gravity plays a major role in determining the water table depth? How true is that in an extremely flat area such as a large wetland (for example, the Vasyugan Wetland complex in West Siberia stretches uninterrupted across 15 degrees of longitude)? Your figures indicate that the method does a good job of differentiating between uplands and wetlands, but without the metric I'm asking for in point #3, we don't know for sure if you have accurately represented WTDs within a wetland. This should be discussed in the Discussion section.*

5. Continuing from the previous question, what about the role of microtopography? Field studies (e.g., Saarnio et al.,1997; Eppinga et al., 2008 – esp. figure 1) found very large *differences between WTD of hummocks and hollows (on the order of 50-70cm), with the differences almost equivalent to differences in the local elevation of the surface, rather than local slope. In my own experience, observed WTDs of Glagolev et al. (2011) showed almost no correlation with topographic wetness index derived from the ASTER DEM; they were far more highly correlated with the local landform (hummock, ridge, hollow, pool). These hummocks and hollows are about 1 meter across; i.e. this type of microtopography is not captured in large-scale DEMs. It may be worth noting that Bohn et al. (2013), who also used the VIC model, changed their approach from a TOPMODEL-based one to a microtopography-based one (Bohn et al., 2013). (Note: Bohn et al (2013) was published after you submitted your manuscript; therefore I leave it to you to decide whether you should cite it in your final manuscript. Also, disclaimer: I was an author on that manuscript. I will not be offended if you do not cite it.) I am not suggesting that your method was incorrect or that you should re-do any simulations. But I would like you to address how similar you think your WTD distributions might be to the "true" distributions. Given that the DEM you used was at 5-km resolution, what exactly does the topographic wetness index derived at that resolution measure? A larger, regional trend in WTD, perhaps? Is there evidence for such trends? (look at Eppinga et al, 2008, figures 4 and 5, for example). Surely the 5-km topographic wetness index has little relationship with microtopography on the scale of meters. Would an optimal WTD scheme perhaps be a combination of both approaches? Once again, your thoughts could go in the Discussion section.*

[Response] We agree with the reviewer that our TOPMODEL-based method could be not valid in extremely flat regions where microtopography might play a more important role in controlling WTD and therefore CH_4 emissions. Thus, for those flat regions like large wetlands, a microtopography-based model framework could be a better option if detailed microtopography information is available. In current study, we apply a TOPMODEL-based model framework since only large-scale DEM data is available for the whole pan-Arctic and the detailed microtopography information is unavailable for us. However, in future studies, a combination of TOPMODEL-based and microtopograhy-based model framework could be a better choice with more and more microtopography information is available from field investigations or high-resolution satellite data. See the third paragraph in Discussion section for more explanation and discussion on this comment.

[Comment] *6. What consequences might the 42% difference that you have found have for our greater understanding of high-latitude methane emissions? How would this finding be important for other researchers? For example, the fate of permafrost carbon is getting a lot of attention right now (e.g., Koven et al., 2011). How might your results impact projected future emissions due to climate alone (with methane produced from contemporary carbon as it is today) and/or due to permafrost thaw (liberating ancient carbon)? Would it be a simple 42% increase over any projections that other studies have made, or would this imply greater/lesser sensitivity to various climate factors, and if so, which ones (e.g., can you relate your findings from Table 1 to other studies' projections)? Again, please discuss this in the Discussion section.*

[Response] The implications of our findings for the projection studies are now presented and discussed in the last paragraph in Discussion section.

[Comment] *Page 18457, line 27: Just FYI, a study was recently published examining the effects of water table depth heterogeneity at large scales: Bohn et al. (2013). However, Bohn et al. (2013) focused on West Siberia. Bohn et al (2013) was published after you submitted your manuscript. I leave it to you to decide whether to cite that paper; this is more for your own information.*

[Response] To promote the research in addressing the effects of spatial scales on methane emissions in northern high latitudes, we cited Bohn et al. (2013) in our Discussion.