Dear Referee #3,

First we would like to sincerely thank you for your effort in reading our relatively long manuscript. We thank you for your careful and valuable review. Following your comments, we have modified and enhanced the structure of the manuscript. We rearranged some paragraphs in Introduction and Discussions, refined consistency, and added further analysis of methane fluxes regarding the sensitivity of CH_4 emissions to TOPMODEL parameterizations. We have also refined our statements in Discussions for better flow and improved understanding of our approach. The new version of the manuscript is attached to this document, with major modifications highlighted in blue. Every single comment is answered here. Please find your comments below followed by our responses in blue including line numbers in the revised manuscript where appropriate:

General comments: Previous works have also fitted the CTI products to functions that represent a grid cell CTI value, such as in Kleinen et al., 2012 and Ringeval et al., 2012. Although this approach sounds reasonable, I am not convinced that by providing the inundated fraction in the grid cell the computational cost is considerably reduced. This might be true for some models but not in all cases and not in all resolutions. Furthermore, if this is true, an extra preprocessing of after the CTI grid cell fitting to obtain the inundated fraction implies an extra step beforehand that certainly adds more errors in the model input. The authors give a step towards this by reducing the uncertainties in the calculation of the maximum soil saturated fraction obtained from the CDFs, by introducing a parameterization to calibrate the maximum wetland fraction (F^{wet}_{max}) with "original" values (F_{max}) obtained from the CDF when the mean CTI is zero.
An interesting contribution from this manuscript is the comparison of the three DEM's (HYDRO1k, GMTED and HydroSHEDS) for wetland simulation in DGVMs, and arise the need of hydrological corrections before its use.
My major concern regarding this manuscript is that I find it still too descriptive for the model setup and I believe is still out of the scope of Biogeosciences. Despite the authors made an effort by adding few sentences regarding the analysis of modeled methane fluxes to test the wetland representation from the model, the authors rarely refer to the CH_4 fluxes application throughout the manuscript. The focus of the manuscript is still to simply compare the three DEM products in their model setup and improve the F_{max} parameter in TOPMODEL, but they do not make any strong reference to the evaluation of methane fluxes or discuss further other papers that make this analysis. A clear example of this, are in the specific aims of the manuscript listed at the end of section 1, which are only focused on model improvement based on the analysis of using three different DEM's. Also in Discussion and Conclusions there is nothing regarding methane emissions. Therefore, I still find difficult to agree that this manuscript should be published in Biogeosciences in the current state, and I believe is still suitable for GMD.
Despite this, I made some comments that the authors may find useful to improve the current version of the manuscript. Some of the statements made by the authors are ambiguous and it needs several language corrections, this makes it

sometimes hard to understand what the authors really mean. The wording is particularly hard to follow in the Discussions sections, although I make some specific comments, I suggest that the authors revise carefully their sentences and re-arrange the wording for a clearer reading.

Still if there error are corrected, and comments here included answered, I encourage the authors to make more emphasis in the CH_4 fluxes, e.g. include a specific aim in section 1 and discuss further other works that had published CH_4 fluxes using similar approaches (e.g. Kleinen et al., 2012). Also compare to more representative studies for the regions of interest with other methodologies (see my comments below for this). Therefore, I cannot support at this point the publication of this manuscript in its current form in Biogeosciences.

In terms of potential errors that might be introduced during preprocessing of TOPMODEL parameters as reviewer mentioned, we would like to clarify that there is no additional errors introduced in the processes, this is because the discrete cumulative distribution function (CDF) was used to derive original F_{max} instead of using fitted CDF curve. For computational efficiency, we admitted that our approach might not be applicable at all resolutions (especially for researches at fine resolutions), but for applications at coarse resolutions in Earth System Models, it is a essential step to save computational time since there are ~ 10^4 pixels (if use DEM at 500 m resolution) within 0.5° grid cell and the discrete cumulative distribution of all the sub-grids need to be calculated at each time step.

For the analysis of methane fluxes, we strengthened the discussions regarding the sensitivity of CH_4 emissions to TOPMODEL parameterizations by comparing global and regional estimates of CH_4 emission among model experiments. In evaluation part section 4.1, the importance of F_{max} calibration in CH_4 estimation was justified, and then a new Table 5 was added to summarize the differences. The new statements are listed below:

In addition, TOPMODEL parameterizations have considerable influence on simulated CH_4 fluxes that the uncertainty of mean annual CH_4 emissions from topography inputs is estimated to be 29.0 Tg yr^{-1} (Table 5). All of the model estimates generally fall within the range of inversion estimates. The differences of CH_4 emissions among the model experiments is related to simulated magnitude of wetland extents because the fraction of CH_4 emissions from tropics (~63%) and Extratropics (~27%) keep constant due to same parameters $r_{C:CH4}$ and f_{ecosys}. The importance of hydrological correction is highlighted by results based on GMTED, suggesting that applying topography map without hydro-correction may potentially underestimate CH_4 fluxes due to lower hydrological connectivity that dampen generating of inundation. In addition, fine-scale topography data like HydroSHEDS show higher CH_4 fluxes than HYDRO1k, suggesting its influence on capturing small wetlands/inundated areas that may be ignored by coarse-resolution products.

Table 5. List of global and regional wetland CH_4 estimates from our model experiments (see Table 2) over the period 1980-2000. All units are Tg CH_4 $yr^{-1} \pm 1\sigma$, where standard deviation represents the interannual variation in the model estimates. Note that estimates from some reference studies are not for the same period.

Estimates	Global	Regions			Hotspot			
		Tropics (20N-30S)	Temperate (20-45N, 30S-50S)	Northern (>45N)	Central Amazon[b]	WSL	Hudson Bay	Alaska
SHEDS_BASIN	171.9	109.3±2.3	26.4±1.0	36.1±1.8	10.9±0.3	5.4±0.9	6.5±0.5	1.7±0.3
SHEDS_GRID	193.0	123.7±2.2	31.4±1.0	38.7±1.9	11.4±0.3	5.5±0.9	7.1±0.6	1.5±0.3
GMTED_BASIN	130.1	85.5±2.3	19.0±0.9	26.3±1.4	9.5±0.4	4.5±0.9	4.4±0.6	1.6±0.3
GMTED_GRID	117.2	76.7±2.3	16.4±0.9	24.2±1.4	9.2±0.4	4.1±0.9	4.2±0.6	1.4±0.3
HYDRO1K_BASIN	148.3	96.4±2.3	21.5±0.9	30.3±1.6	10.4±0.3	4.4±0.9	5.8±0.6	1.7±0.3
HYDRO1K_GRID	128.8	85.0±2.3	17.8±0.9	26.0±1.4	10.0±0.4	3.9±0.9	4.8±0.6	1.5±0.3
Melton et al. (2013)[a]	190±39						5.4±3.2	
Zhu et al. (2015)	209-245			38.1-55.4				
Chen et al. (2015)				35			3.11±0.45	
Zhu et al. (2014)				34-58			3.1± 0.5	
Ringeval et al. (2012)	193.8	102	51	40.8				
Glagolev et al. (2011)						3.91±1.3		
Melack et al. (2004)					9.1			
Zhuang et al. (2004)				57.3				
Chang et al. (2014)								2.1±0.5
Bloom et al. (2012)		111.1						
Bousquet et al. (2011)	151±10	91±11						
Bloom et al. (2010)	165±50	91±28					4.9±1.4	

[a] WETCHIMP estimates for 1993-2004

[b] Central Amazon (54-72°W,0-8°S)

In the new version of the manuscript, we've clarified some points in Discussions. Please see below responses.

Major comments:

- The full name of an acronym should be always stated when is first mentioned in the paper. I could not find the full name of LPJ-wsl or LPJ-DGVM, please write it in full either in the Abstract or in the Introduction when is first mentioned (P17957, L23?). There are also other acronyms that should be written its name in full, please check this throughout the manuscript.

Revised

- L14 – In the sentence: "… which has been proven to at least partly cause biases due to limited spatial resolution…", I don't think 1km is a limited spatial resolution for such datasets, please elaborate here what the authors really mean with these sentence.
L26 – mention some examples of physical processes the authors refer to in this line (e.g.)

The sentence has been changed to read:
Currently, most of the global applications derive a CTI product at 1km resolution from HYDRO1k global dataset released by U.S. Geological Survey (USGS) in 2000, which has been proven to potentially overestimate inundation extent due to the quality of the underlying digital elevation model (Marthews et a., 2015).

L26 we add: (e.g. snow aging effect on thermal properties)

P17967-L26; P17968, L1-2. Although the correlation between the model simulated frozen-days and the in Fig. 3 agrees well, the authors speculate that the low correlation in East Siberia could be due to the nature of the data, while in the satellite observations it is included the ice condition in the vegetation canopy, snow layer and frozen water in the upper soil layer, in the model it is only considered the frozen state of the top soil, but if this is true, why in the southern regions of Siberia the correlation seems to agree better? I would expect that this behavior remain at least in most part of northern latitudes.

Thanks for pointing out this issue. The low correlation in some arctic regions was due to the insulation of soil temperature. This is because in our model, frozen day is calculated in condition that unfreezing water fraction is close to zero in all of the upper soil layers. When there is a large amount of snow above surface, the timing of soil temperature to reach frozen status will be delayed due to extreme high snow depth in those regions.

- It is misleading the explanation of F_{max} and F^{wet}_{max}. To what I understood from the manuscript, F_{max} is taken for the satellite observations and used to calibrate F^{wet}_{max} which is then used to obtain the wetland area fraction F_{wet}. However, the authors repeat in the manuscript that what they propose is a "calibration of F_{max}", shouldn't be F^{wet}_{max}? Please correct me if I am wrong or otherwise, be more explicit and careful in the description of the method and correct where necessary in the manuscript.

To avoid misunderstanding and for consistence with other studies, the F^{wet}_{max} has been replaced with F_{max} to make it clear.

- The newly available DEM product from the Centre for Ecology and Hydrology (an improvement from HYDRO1k from 30" res to 15" res) https://data.gov.uk/dataset/high-resolution-global-topographic-index-values1, should be at least mentioned and discuss how this new product can improve the representation of wetlands at global scale and how this can be combined with the F_{max} (or F^{wet}_{max}?) calibration proposed in this manuscript.

Sorry we didn't find the DEM dataset from the website you provided. If the new topographic index product based on HydroSHEDS DEM is what you mean, we added sentence to describe this dataset. We didn't use this new dataset because we need to keep all the topographic maps generated from the three DEMs in our model experiments following the same algorithm to make it comparable. Below is the description:
To avoid mismatch of CTI value inherent in computing CTI with different CTI algorithms, we generated a global CTI map based on the three DEM products, instead of relying on existing CTI products (e.g. HYDRO1k CTI, HydroSHEDS CTI product from Centre for Ecology and Hydrology (Marthews et al., 2015)).

Specific comments:
P17954,
L2 – spatio-temporal
L16 – Define here what DEM stands for

Revised

P17957,
L10 – Add citation year for Ward and Robinson (2000)
L12 – is really 1 km limited?
L26 – e.g. physical processes

Revised

P17958,
L16-17 – remove parenthesis in Hodson et al., 2011 AND Wania et al., 2013
L17 – "and is a function of two scaling …"
L17 – the authors does not define f_{ecosys} and $r_{CH4:C}$ in the text, nor say how they are obtained
L24 – delete "contributed as"

Revised

P17959,
L18 – move parentheses before "Cosby" to before "1984" (Cosby et al., (1984))

Revised

P17960,
L20 – add in parenthesis after the name the acronym CTI

Revised

P17961,
L14 – delete "furthermore"
L18 – to my understanding a gamma function can be also exponential, and this in the end is a similar treatment than the gamma function, thus not reducing the computational cost.

Revised

P19762,
L3 – "... topographic information generated by fitting the ..."
L4 – add a comma after CTI
L4 – here the authors should be more specific on "observed maximum wetland fraction" starting that this information was obtained
L15 – write the meaning here of SWAMPS-GLWD

Revised

P17963,
L4 – write the meaning of HWSD
L4 – reference for the HWSD soil texture database?
L8 – replace "more" by "mainly"
L10 – latitudes
L19 – write the spatial resolution of the DEMs after they are mentioned in the following lines

Revised

P17964,
L14-20 – Here it is a misleading whether the authors generated ONE single CTI maps based on the three DEM products or if there were THREE CTI maps been one per DEM product. This becomes confusing along the manuscript, particularly arriving at Figure 7. See my comment below for it.
L20-25 – Here it is not really clear in the paragraph if GMTED was also used to generate the global CTI map despite was not hydrologically corrected as the other two DEM products? What do the authors mean with "retaining GMTED DEM without hydrologically correction"?
L25 – change "hydrologically" by "hydrological"

We made some revision to make it clear. We generated three CTI map based on three DEM products with same algorithm. Here below is revision:

To avoid mismatch of CTI value inherent in computing CTI with different CTI algorithms, we generated three global CTI maps based on the three DEM products, instead of relying on existing CTI products (e.g. HYDRO1k CTI, HydroSHEDS CTI product from Centre for Ecology and Hydrology (Marthews et al., 2015)). Since studies show that multiple flow direction algorithms for calculating CTI give better accuracy compared with single-flow algorithms in flat areas (Kopecký and Čížková, 2010; Pan et al., 2004), thus we selected an algorithm from R library 'topmodel' (Buytaert, 2011), which applies the multiple flow routing algorithm of Quinn et al. (1995) to calculate the global CTI maps. The DEMs from HYDRO1k and HydroSHEDS had been previously processed for hydrological-correction, meaning that the DEMs were processed to remove elevation depressions that would cause local hydrologic 'sinks'. To include a comparison of (hydrologically) corrected and uncorrected DEMs in our analyses as some studies have been done previously (Stocker et al., 2014), the GMTED DEM was applied without hydrological correction.

P17965,
L4 – "generating a global catchment map"
L9 – "The description of the DEM products used in this study are summarized in Table 2"
L13 – here the word spin up is separated, while in L18 is a single one (spinup), the correct should be separated

Revised

P17966,
L27 – Poulter et al., 2015

Revised

P17967,
L24 – "in those regions"

Revised

P17969,
L4 – correct here and throughout the manuscript that CH_4 is with subscript (i.e. CH_4)
L5-10 –As stated in the caption of Figure 6, the authors should mention here the DEM product used is Hydro-SHDES for TOPMODEL. However, this is confusing since earlier in the manuscript the authors mention that they generate a mean CTI map of the three DEM products to actually "calibrate" TOPMODEL, so why here it is only comparing Hydro-SHEDS?

L5-10 – I would try to avoid using the expression "calibrated TOPMODEL" and "non-calibrated TOPMODEL" for the correction on the maximum fraction of wetland extent. This is what it was actually corrected (F_{max}) but TOPMODEL itself not only provides the maximum fraction.
L14-19 – I am not convinced with the comparison of results from the West Siberian Lowland to the CARVE observations in Alaska. Although both are boreal wetland regions, there are published works that match better the region of interest in question. I would rather use for example previous observations at least in the Siberian region with other techniques like Eddy covariance like the works of Parmentier et al., 2011 (J. of Geophys. Res.) or Wille et al., 2008 (Global Change Biology).
L22-25 – Figure 7 is really well explained here nor in the Figure caption. What do the authors mean with the prefix BASIN and GRID? This part needs more detailed information in the simulations description before it is presented in the results. If they are the aggregation schemes they briefly mention in the introduction, then the authors need to refer to them by their name there. Furthermore, the authors mention "both datasets" but they should be specific to what they mean (e.g. the results from the simulations with BASIN and GRID aggregation schemes?). I honestly, don't see much the sense of this figure plus it is hard from it to visually look at the "uncertainties" of the parameterization.
L27 – replace "differing" by "different"

We agree that evaluating our CH_4 fluxes with independent estimates from flux tower measurement or airborne campaigns is important but we found it is difficult to directly apply Eddy Covariance results in evaluations as there is scale mismatch between model estimates at 0.5 degree resolution and flux tower results at ~ 1-10 km^2. Upscaling point measurements might introduce large uncertainties due to the influence of spatial heterogeneity. The measurements conducted over broad areas such as aircraft can span similar temporal and spatial scale as our model results and is independent.
W revised a few sentences in this paragraph to make it clear:
To evaluate the effect of F_{max} calibration on CH_4 emission estimates, two estimates of CH_4 (with and w/o calibration) over the WSL regions were compared with observation-based estimate from Glagolev et al. (2011) (Figure 6). The 3-year mean annual total emission from original version is 6.29±0.51 Tg CH_4 yr^{-1}, falling into the upper part of range from land surface models and inversions (Bohn et al., 2015), whereas the calibrated version is close to the estimate of Glagolev et al. (2011) (3.91±1.29 Tg CH_4 yr^{-1}) with 4.6±0.45 Tg CH_4 yr^{-1},. In addition, the spatial pattern of CH_4 emission with F_{max} calibration shows better agreement with observation than non-calibration one with relatively larger emissions in Taiga forests and central region (55-65°N, 65-85°E). We also compared our estimates with recent airborne campaign observations for Alaska during 2012 growing seasons. Estimates with F_{max} calibration also falls well into the range of recent estimate (2.1±0.5 Tg CH_4 yr^{-1}) for Alaska based on airborne observations (Chang et al., 2014) with a total of 1.7 Tg CH_4 yr^{-1} during 2012 growing season (3.1 Tg CH_4 yr^{-1} from non-calibrated estimate), indicating necessity of F_{max} calibration to accurately capture annual CH_4 emission and spatial variability for boreal wetlands.

L22-25: We added descriptions in caption of Figure 7 and rearranged Section 3.1 and 3.2 to make the description easier to follow:

3.2 Description of the simulation

For running LPJ-wsl with permafrost and TOPMODEL, we used global meteorological forcing (temperature, cloud cover, precipitation and wet days) as provided by the Climatic Research Unit (CRU TS 3.22) at 0.5° resolution (Harris et al., 2014). To spin up the LPJ-wsl model using the CRU climatology, climate data for 12-months were randomly selected from 1901-1930 and repeated for 1000 years with a fixed pre-industrial atmospheric CO_2 concentration. The first spinup simulation started from initial soil temperature derived from LPJ-wsl simulated results on January 1901 and continued with a land use spin-up simulation. These procedures ensure that carbon stocks and permafrost are in equilibrium before performing transient simulations. The transient simulations, with observed climate and CO_2 were performed with monthly climate disaggregated to daily time steps over the 1901-2013 period. The 1993-2013 years were used for evaluation against satellite data and inventories.

One of key assumptions in TOPMODEL is that the water table is recharged at a spatially uniform and steady rate with respect to the flow response timescale of the catchment (Stieglitz et al., 1997). Given the fact that we consider the water to be stagnant within each grid, the mean CTI parameter was estimated with two alternative schemes: (1) a regular 'grid-based' or gridded approach, i.e., the subgrid CTI values were averaged per 0.5° grids, and (2) an irregular 'basin-based' approach, where mean CTI were calculated over the entire catchment area in which the respective pixel is located. For generating a global catchment map at 0.5° resolution, we applied a majority algorithm in the case of multi-catchments in a grid with consideration of avoiding isolated pixels for specific river basin. There are two catchment area products applied in this study, HYDRO1k (2013) and HydroSHEDS. Similarly, the parameter C_s was generated using nonlinear least squares estimates from both of these two different CTI calculation strategies. Two sets of model experiments were carried out to compare the wetland dynamics under basin and grid-based TOPMODEL parameterizations respectively (Table 2).

P17970,
L5 – replace "sensitivity" by "sensible"
L10-12 – I thought GMTED was not hydrologically corrected?
L11 – Add the degrees symbol to 60N
L16-17 – replace "estimation" by "estimates"
L18 – replace "paddy" by "paddies"
L21 – replace "digitalized" by "digitized"
L22 – move the word "directly" after "… when comparing …" at the end of line 20
L25 – I guess it should say "… due to permanent wetlands that are hard to detect by GIEMS."
L27 – please elaborate here more about the satellite inundation datasets, what the authors really mean with "non-specific measurement of inundation"?
L28 – This paragraph is also misleading, do the authors meant to say that the definition of wetland in this work is in agreement to the definition used by the National Wetlands Working Group? Please also reference this in the reference section as: National Wetland Working Group, 1988. Wetlands of Canada, Ecological Land Classification Series, No., 24. Canada Committee on Ecological

Land Classification. Sustainable Development Branch, Environment Canada and Polyscience Publications Inc. Montreal, Quebec, Canada.

We changed the sentence as below:
L10-12: *Note that GMTED is derived from the same DEM product SRTM as HydroSHEDS but without hydro-correction, indicating the importance of hydro-correction in simulating spatial patterns of wetlands*.
L27-28: *Remotely sensed inundation datasets emphasizes on open water while wetland area in our study is specifically defined from inventories following the National Wetlands Working Group (1988) classification that include peatlands, mineral wetlands, and seasonally inundated shallow waters.*
L28: We also add reference in reference section.

P17971,
L7 – SON is not a season but the acronym of a list of months that accumulated corresponds to a season (autumn), please rephrase correctly (replace the word seasons by months).
L9 – what do the authors mean here with "masked estimates"? ambiguous
L10 – pluralize latitude
L11 –an area cannot be higher, only larger
L12 – rephrase, seasons are not unfrozen, you can instead say "… from longer periods of unfrozen and relatively water saturated soil in the model data"
L16 – replace "seasons" by "months" (or "SON seasons" by "autumn")
L22 replace "underestimated" by "underestimates"
L24 – replace "estimates" by "data sets"
L24 – replace "base" by "based"

Revised

P17972,
L4 – here the authors refer to the "grid" experiments as "tile-based", please keep consistency with your nomenclature here and throughout the manuscript
L10 – "the" Pearson's correlation coefficient
L13 – Define what is a "Transcom region"?, it was only mentioned before in the figure caption of Fig. 2 and also in caption of Fig. 8
L17-18 – This sentence is a confirmation of previous works, like Kleinen et al., 2012. Taking this into account I would rather make more emphasis throughout the manuscript that the aim of the correction in the maximum wetland extent is to actually improve the representation of wetlands by the models using TOPMODEL at a regional scale. This has to be highlighted even in the abstract section.

Revised
Thanks for your comments. We've added sentences in abstract to highlight it as below:
Abstract: *This study demonstrates the feasibility of TOPMODEL to capture spatial heterogeneity of inundation at large scale and highlights the importance of correction in maximum wetland extent to improve modeling of spatio-temporal variations in wetland areas.*

P17973

L7-8 – wording of sentence a bit strange, I suggest: "… TOPMODEL with calibrated parameters as described in this study, allows the dynamical simulation of wetlands, in particular their geographic location and extent."

L9-13 – this sentence is particularly hard to follow, please re-arange the wording to make it clearer

L21 – strange wording, do the authors mean: "… in absolute values, which is necessary for global wetland modeling."? I would modify this sentence since is confusing in the way is written now.

L23 – change to: "…because the physical processes are described in a robust way"

L25 – "allows the retrieval of the maximum water saturated fraction (F_{max}) of a model grid cell, which is defined by …"

This paragraph was changed to:

The coupling between LPJ-wsl and TOPMODEL with calibrated parameters as described in this study, improves the dynamical simulation of wetlands, in particular their geographic location and extent. This is based on the recent discussions of the suitability of TOPMODEL applications to simulate wetland variations at large spatial scale (Ringeval et al., 2012), and intercomparisons of the wetland-area-driven model bias in CH_4 emission at regional scale (Bohn et al., 2015a). The large discrepancies of wetland area among LSMs so far have shown extensive disagreement with inventories and remotely sensed inundation datasets (Melton et al., 2013), which is partly due to large varieties of schemes used for representing hydrological processes, or due to the parameterizations for simulating inundations. Our results suggest that benchmarking F_{max} is necessary for global wetland modelling.

P17974,

L2 – Replace "This" by "The"

L14 – pluralize "application"

L15 – pluralize "parameterization"

L16 – "fine scale"

L16 – "which complicates the comparison to inventories"

L17-22 – the wording of this paragraph is wrong, and hard to follow, please correct it.

Revised paragraph is below:

Integration of satellite-based and inventory-based observations to calibrate F_{max} is highlighted in this study. Combining SWAMPS and GLWD led to simulated wetland area consistent with detailed regional distribution (Poulter et al., in preparation). Our estimation of global wetland potential/maximum is ~ 10.3 Mkm^2, and in agreement with the deduction (10.4 Mkm^2) from recent estimates at finer resolution for total open water (~17.3 Mkm^2) (Fluet-Chouinard et al., 2015), lakes

(~5 Mkm²) (Verpoorter et al., 2014), and rice paddies (1.9 Mkm²) (Leff et al., 2004). The calibration of F_{max} maintains capability of simulating the wetland dynamics on decade-to-century long time scales. As shown in Figure 9, the wetland potential for permafrost and arid/semi-arid regions is high. Even in tropical regions, there is ~ 20-30% of potential areas can be inundated.

P17975,
L14 – "... size and location that make hard to reconcile a single definition for wetlands"
L15 – pluralize "parameterization"
L19 – pluralize "area"
L25 – elaborate in "limitation therein"
L18 – and complete paragraph should be moved to the introduction since this is a better start for the background knowledge and motivation of this study. This paragraph will certainly improve the flow of the method if it is moved forward in the manuscript.
L26 – move "during the last decade" to the beginning of the sentence
We moved the paragraph to Introduction and revised Introduction section to improve the flow. Here below is revised part of Introduction:
While prognostic wetland dynamics schemes are promising to resolve these observational issues, the configuration parameters for TOPMODEL are a potential source of uncertainty in estimating wetland dynamics (Marthews et al., 2015). Among all parameters in TOPMODEL, the Compound Topographic Index (CTI) is of critical importance for determining inundated areas in terrain-related hydrological applications (Ward and Robinson, 2000; Wilson and Gallant, 2000). It measures the relative propensity for soils to become saturated (Beven and Cloke, 2012) and consequently it drives the accuracy of wetland area scaled to the larger grid cell (Ducharne, 2009; Mulligan and Wainwright, 2013). Although the importance of CTI has been highlighted, only few studies have so far evaluated the effect of CTI on modelling the spatial and temporal patterns of global wetland dynamics. This is due to a limited availability of global CTI products. During the last decade, the first CTI product at 1km resolution from HYDRO1k global dataset released by U.S. Geological Survery (USGS) in 2000 has become the most commonly applied global dataset for large-scale applications (Kleinen et al., 2012; Lei et al., 2014; Ringeval et al., 2012; Wania et al., 2013). However, HYDRO1k has been proven to potentially overestimate inundation extent due to the quality of the underlying digital elevation model (DEM) (Grabs et al., 2009; Lin et al., 2010; Lin et al., 2013; Sørensen and Seibert, 2007;). With recent development of DEMs (Danielson and Gesch, 2011; Lehner et al., 2008), there is a requirement to investigate uncertainties caused by CTI parameters.

P17976,
L2 – "from regional to global scales"

L2 – The reference Lin et al., must be separated as: Lin et al., 2010; Lin et al., 2013; the first one corresponds to Kairong Lin and the second to a different author (Shengpan Lin)
L6 – "benefit"
L7 – "creating a more realistic representation …"
L9 – "This is supporting the ideas of …"
L16 – "closed depressions"
L24 – "As a result"

Revised

P17977,
L23 – "describe"
L25 – "need"

Revised

P17978,
L27-28 – "Remotely sensed global inundation is prone to underestimate small wetland areas, …"

Revised

P17979,
L3 – "This raises the need for benchmark dataset useful to generate accurate products with lower uncertainties"
L14 – "and captured well the spatio-temporal …"

Revised

References
P17980,L24 – Update the reference by Bohn et al., 2015a (not in discussion anymore)
Missing reference USGS, 2000 (cited in P17964, L5-6)

Revised

Figures
Besides specific comments on figures' captions mentioned before, here are some more comments.
Figure 1 – replace the symbol lambda with the horizontal line on top by lambda with subscript m as in the text. Also in the label of the x-axis lambda should have the subscript l corresponding to the local CTI value. Change this also in the legend of the figure
Figure 2 – the figure caption must be considerably improved, by making reference to the panels and their meaning, also by editing the text (italics, subscripts, etc.)
Figure 4 – add year "Tanocai2009" in both title of subplot and caption

Figure 5 – include in the caption the area of study (e.g. Amazon River Basin or Lowland Amazon Basin)
Figure 6 – Change the units of CH_4 emissions with the area unit before the time unit (e.g. g CH_4 m^{-2} yr^{-1})
Figure 8 – replace "variation" by "variability"

Revised

Caption of Figure 2 was changed to:
Figure 2. TOPMODEL parameter maps in model experiments. Mean CTI (a, b) and C_s (c, d) aggregated by river basin (denoted as "By Basin") and by grid cell (denoted as "By Tile") schemes from HydroSHEDS were listed. F_{max} (e) for calibration was generated using SWAMPS-GLWD and GLWD. Map of regions (f) was used to partition globe into boreal, temperate, tropical biomes (Gurney et al. 2003).

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