## Reviewer Vivoni (C2151 and C2609):

The manuscript describes the development of a new global dataset on the topographic index derived from the HydroSHEDS database which itself is a packaged product from the SRTM dataset. The product appears to be a valuable contribution that will be used by data analysts and modelers working at large scales. The authors present well their work and illustrate it effectively by presenting some interesting comparison to prior efforts (HYDRO1K CTI). I also liked that they brought in their personal motivation related to estimating wetland areas into their analysis of the global dataset. Overall, a niece paper and the authors should be commended. I suggest some minor revisions in the follow and also encourage the authors to reduce repetition and the overall length, where possible, as a shorter piece will have greater impact. Care should also be taken with the scales and resolutions mentioned as these can confuse readers (high- resolution vs. coarse, mesoscale versus sub-mesoscale, 60-300 km vs. 1-10 km).

Thank you very much for these comments. We hope that our individual responses below are a sufficient reply to the various points raised.

### Minor Comments:

Page 6140: Please define the acronym GA2 in the abstract. Thanks very much for noticing this: done.

Page 6140: Please expand upon the references to support the definition of LSMs, including models that are soil-vegetation-atmosphere transfer schemes and those that would be classified as macroscale hydrology models. (I noted later that additional references are placed in the following page, suggest to include some of those in the first sentence.) The first sentence has been expanded and a slightly wider selection of representative references included.

Page 6140: LSMs can have a range of resolutions down to ~1 km, thus the use of the range of 60-300 km can be misleading. (I noted later that the resolutions are indicated to be approaching 1-10 km, thus please avoid the range of 60-300 km used in the second sentence). This has been amended to "up to 300 km": we are aware that different authors use different gridbox sizes, but our experience is that the great majority of LSM runs are in the range 60-300 km and we wished just to emphasise here the fact that there is a gap between the usual resolutions of LSMs and the resolutions of the majority of process-based hydrological models. We have also inserted "usually" to avoid the implication that LSMs are constrained somehow to be run at these resolutions.

Page 6141: Wood et al. (2011 and 2012) are not really about specific models. These are opinion or response to opinion papers. Please provide some references of LSM studies at high resolution that are used at the catchment scale for water resources or climate change investigations. We have included Ke et al. (2012) and Choi (2013) as good recent examples of high-resolution studies of this type.

Page 6141: For clarity, please define 'sub-mesoscale' We have now defined "mesoscale" more precisely as 10-100 km scale (broadly in line with use in e.g. the mHM model) so we believe that "sub-mesoscale" has become clear by implication.

Page 6141: The term 'statistically-generlized' could be replaced with 'distribution function'. See Beven (2000) book for nomenclature. We have rather decided to omit the term "statistically-generalised" at this point: it was not a necessary part of the sentence.

Page 6143: The acronym LSM need not be continuously defined. Once in the first occurrence of the term is sufficient. Apologies for this: we did define it at three separate points, but now only one definition has been retained.

Page 6144: Is there any special treatment or masking for other problematic areas such as croplands, urban zones, mines/quarries, wetlands, etc. that might have significantly altered flow paths? At the 450 m scale this is a legitimate concern. Flow paths within the HydroSHEDS data corpus have been individually scrutinised worldwide to take into account as many local modifications of this type as possible: they are not simply calculated from the void-filled SRTM DEM (please see HydroSHEDS methodology references cited). This step is part of the 'hydrological conditioning' step described in section 2.1.

Page 6145: An estimate of the number of pixels in the global layer and the approximate file size would be useful to report to justify the need for the cluster resources. These have been added (1.2e9 land pixels; 11.0 Gb as NetCDF).

Page 6145: It is more appropriate to refer to Figure 1 first and Figure 2 second and so forth. Apologies again: text has been amended to correct this.

Fig. 1: Can the authors use a color bar that more clearly illustrates patterns at the global scale? We note that this is a legitimate concern and have tried several different color scales for this figure during drafting. We experimented with more than 6 colour levels and found that it was either immediately too busy or looked too reminiscent of a physical terrain map (which we have tried to avoid). We have also

experimented using an inset on a particular river catchment to show finer-scale features, but this unavoidably introduces bias in our presentation favouring a particular river catchment over others and we had to accept that no river catchment is truly globally representative and therefore remove the inset. Using shades of brown and blue only, divided at the global mean value 5.99, has been for us the best way to show the very broad-scale pattern of water-retaining/water-poor areas of the globe (discussed in the text) and no other global patterns can be shown on a figure such as this. In the final version of this figure we will ensure that a high-quality image is included, which will allow a reader to zoom into the PDF and see regional-scale patterns and we hope that this will be sufficient to address the reviewer's concern here.

Page 6145: It is more appropriate to refer to Table 1 first and Table 2 second and so forth. Apologies again: text has been amended to correct this.

Page 6145: It is important to indicate if this statement "indicating that wetlands in these areas are maintained by factors other than topography." refers to the former (in Asia) or latter (in Canada/Russia) wetlands. We have inserted "both" in this sentence to clarify that we meant both these areas rather than one or the other.

Page 6147: The more appropriate term is 'general circulation models' Thanks: done

Page 6147: There is quite a bit of repetitious material between this quite lengthy discussion and the introduction. I suggest to tailor this discussion back to the essential elements learned from the study and not the generalities more appropriate for the introduction. The identified section at the start of the Discussion has been trimmed from 316 words to 170 and the text elsewhere tightened up to avoid the identified repetition with the Introduction.

Page 6148: What are 'knock-on' effects? Define or use a more common term. We have added the following to replace "beneficial knock-on effects", drawing on comments in Wood et al., (2011): "many benefits including the more realistic representation of processes currently subgrid and, ultimately, better weather and inundation prediction (Wood et al., 2011)"

Page 6149: The authors should expand upon the limitations of the TOPMODEL approach and topographic index for regions where the saturation-excess runoff assumed in the theoretical development is not the dominant mechanism. Such mismatches between field hydrological processes and model assumptions lead to divergences in the topographic index interpretation, such as that shown by having high TI in Subsaharan Africa and Australia in Fig. 1. How valuable is the global map of TI if the underlying processes do not support its application in large areas? We state in section 4.2 that "it is important to remain careful when interpreting [topographic index values] in different regions, such as arid vs. humid, or shallow vs. deep soils (i.e. when factors other than topography influence water accumulation in the landscape)". In order to expand slightly on this, we have now also added "In regions where saturation-excess overland flow (the component of runoff most affected by topography) is less than dominant as a runoff generation mechanism, uncertainties in inundation predictions based on TOPMODEL must be carefully calculated and predictions interpreted with care (see Beven 2012)". However, we have resisted including a summary of runoff generation mechanisms and their relative significance in different areas of the globe because standard textbooks such as Beven (2012) discuss these issues in depth.

It is clear from the many TOPMODEL references cited in this paper that Beven and Kirkby never intended TOPMODEL to be the last word in inundation modelling: indeed it is stated in Quinn et al. (1995) that "We have always believed that both TOPMODEL and the methods used to calculate the ln(a/tan(beta)) index should be open to development". Therefore, we are unwilling to criticise TOPMODEL for being 'limited' (and we have consistently been careful not to do so): we would prefer to follow the majority of TOPMODEL references such as Wolock (1993) and Quinn et al. (1995) by taking TOPMODEL rather as a modelling *framework* that has rather not yet been developed sufficiently for our particular application (and our contribution through this paper is to develop this modelling framework slightly further).

Finally, we do strongly believe that a global map of TI is immensely valuable at this stage despite the underlying processes of runoff generation not being consistent worldwide. Given that topographical factors controlling runoff generation are arguably much better studied than other controls, our new map serves not only as a parameter map for TOPMODEL users but also may be used in conjunction with maps of other properties to help improve runoff prediction in areas where topography is not the main control (e.g. soil parameters).

Page 6150: The product is at ~500 m resolution, about 5 times or 10 times larger than proposed by Wood et al. (2011). Wood et al., (2011) define "hyperresolution scale" to be 100-1000 m ("*O*(1 km) globally and *O*(100 m) at continental scales", although resolutions up to 50 m are mentioned elsewhere in the paper as desirable), which we believe makes it legitimate to describe our global dataset as comfortably within the bounds proposed by Wood.

Page 6150: The last paragraph is a repetition of material from the introduction and can be excluded without any loss of information. The Conclusions have been edited and shortened by 1/3rd. Thank you for noting this.

## Anonymous Reviewer #1 (C2256):

### General Comments

The authors present a new high-resolution map of the topographic index (TI) for ice-free lands. The novelty of the paper is that the new map is 4 times finer than previous maps based on the HYDRO1k data set. The new map is obtained by merging two a existing datasets, namely HydroSHEDS upscaled from a resolution of 3 arcsec to a resolution of 15 arcsec (approximately 450 m at the equator), and a downscaled HYDRO1k dataset for areas having higher latitude than 60°N. A known procedure is used to perform the TI analysis of the obtained dataset. The GA2 algorithm used in this paper is based on the GRIDATB algorithm originally written in 1983 by Keith Beven and revised in 1991 and 1995 by Paul Quinn et al., and implements the classical O'Challagan and Mark's (1984) D8 method for the determination slope directions and terrain slopes. The paper is well written and presents a technically sound work. However, in the opinion of this reviewer, it lacks originality. This reviewer appreciates the contents paragraph 4.2 on page 6149, where the limitations of this study are acknowledged. He feels, however, that the authors should make a further effort to increase the impact of their study. Following, there are some specific comments that this reviewer hope will help the authors to improve their paper.

Many thanks for these supportive observations. Please see below for our responses to your specific recommendations.

### Specific Comments

It may be acknowledged that the single flow direction method D8 has been improved by D8-LTD method (Orlandini et al., 2003; Orlandini et al, 2014). The impact of the D8-LTD method is especially relevant in the analysis of high-resolution complex terrains. However, since the determination of the slope is crucial in TI calculation, it shuuld at least acknowledged that different solpe didection methods can produce different results. This reviewer agrees with the statements reported on page 6149, lines 13–20, of the manuscript, but he feels that some more comments about the more advanced slope direction methods developed in the last decade would be beneficial.
Firstly, thank you very much for the steer towards these Orlandini references (the more recent one has been included in the text at three points). We have discussed the slope calculation extensively during the development of our paper and we accept completely that this is a fundamental issue. However, an overriding concern of this project was to base our work on the HydroSHEDS DEM and related data layers developed over the last decade by Prof. Lehner at McGill. The slope numbers we used in our calculations were therefore necessarily the numbers behind the HydroSHEDS drainage directions layer http://www.hydrosheds.org/page/availability . These drainage directions and related slopes are based ultimately on the D8 scheme (as described in Appx. A and also in Lehner 2013) so our use of HydroSHEDS has unavoidably involved an acceptance of the D8 algorithm. Because these parts of the calculations are described in full in Lehner (2013) and the HydroSHEDS documentation, we did not see a need to replicate these details in the text of our paper and we felt it was sufficient simply to state that we had used D8 "for consistency with the HydroSHEDS drainage direction approach used to derive *UPLAND* areas in this study", as stated in Appx. A.

The analysis of the obtained results is another weak point in the manuscript. This study will grow in novelty by providing a new method for comparing the results obtained from GA2 and existing TI computations. For instance, the authors may want to make a further effort to compare TI and CTI in a selected basin or in a limited number of representative basins where wetlands may be surveyed. Testing the new procedure by using a single catchment would allow the authors to overcome some limitations of the present form of the study. In fact, TI calculated for pixels lying above the 60°N parallel are obtained from different terrain data set compared with pixels lying below that latitude. How does the dataset influence the TI calculation? Are differences between TI and CTI for pixels lying above 60°N affected by the selection of the slope direction method or by the downscaling of the same Land Surface Model? Which disaggregating method is used to resample to 15 arcsec the HYDRO1k data set? In Figure 4, histograms of Lena river basin for TI and CTI calculations are reported. A wide portion of this basin extends above the 60_ N parallel and the two histograms seem to be very similar and close each other. A more comprehensive evaluation of the downscaling method preformed by using selected basins for which both HydroSHEDS and HYDRO1k are available would certainly increase the impact of the presented study.
Several legitimate points are raised here. Firstly, in an earlier draft of the paper we did undertake some analyses for specific catchments (e.g. the St Lawrence) and for specific wetlands (e.g. Araguaia). However, we found that this unavoidably introduced bias in our presentation favouring particular areas over others and we did not feel that it was legitimate to concentrate on one or two areas in a paper taking a global perspective, especially given that our choice of example areas would necessarily have been based on data availability within our team rather than the 'representativeness' of the particular wetland or

catchment chosen. We have indeed already presented comparisons of TI and CTI in selected basins where wetlands occur (see Tables), with our selection there being unbiased because we have included every catchment above $10^6$ $km^2$ worldwide in our comparison (complete samples are automatically representative).

Secondly, HydroSHEDS is an EO product that is indeed based on mosaicking SRTM data (up to 60°N) with coarser HYDRO1k data further north (derived from GTOPO30), however quantifying how this affects our calculations is very difficult because the only global dataset to which we can compare is HYDRO1k itself (HYDRO1k is the only other global TI layer: there is no third source that can be considered an objective standard). Divergent histograms in Fig. 4 for a particular catchment south of 60°N would imply a combination of the differences between the DEMs as well as differences in the TI vs. CTI calculations. Unfortunately, it is not possible to deduce which of these effects is dominant for a particular catchment because large catchments wholly contained north of 60°N do not exist (e.g the Lena catchment has approx. 40% of its watershed south of 60°N). As we have specified in our Methods, our DEM is of finer resolution than HYDRO1k, our hydrological conditioning step has been carried out with access to much better comparison data (HYDRO1k was derived before 2000) and our GA2 algorithm (very similar to the GRIDATB algorithm used by HYDRO1k) has been carried out on data layers composed of complete continental land masses (HYDRO1k were not specific in their documentation about their tiling scheme). As we put in section 4.2, “although we cannot state conclusively that our revised values are more correct than those of the CTI from HYDRO1k, the consistency and rigour of the algorithm used and our closeness to the original GRIDATB implementation as well as the improved HydroSHEDS base data used for the calculation lead us to believe that our values are indeed more robust.”. We hope that the Reviewer can be assured here that we are making a carefully cautious statement here in order to avoid claiming too much for our work: HYDRO1k was a great step forward in its time (as we state in our Introduction), but we have spent a great deal of time ensuring that our product is a definite step-improvement, and as good as it is possible to be within the bounds of 2014 technology and computing resources.

## References


O’Callaghan, J., and D. M. Mark (1984), The extraction of drainage networks from digital elevation data, Comput. Vision Graphics Image Processes, 28(3), 323–344.

Orlandini, S., G. Moretti, M. Franchini, B. Aldighieri, and B. Testa (2003), Path-based methods for the determination of nondispersive drainage directions in grid-based digital elevation models, Water Resour. Res., 39(6), 1144, doi:10.1029/2002WR001639.

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