RC1: 'Reviewers comments for esurf-2018-18', Ian Townend, 13 Mar 2018

This paper provides a useful examination of along-channel variations in channel width hypsometry. The paper is well organised and clearly written. The data used and method of analysis are, in themselves, sound. However, I would like to suggest a few changes that would give the paper a more precise focus. These relate to the methodology and what it can be said to be examining.
We found the review helpful and positive and thank the reviewer in the acknowledgements. Below we describe (in italics) how we used the reviewer comments to improve the manuscript.

The method of Strahler is adopted without any substantive explanation. However the Strahler equation was proposed for terrestrial landscapes and is based on plan areas as a function of elevation. The paper considers submerged (or at times partially submerged) bodies in terms of the cross-section width. The basis of this transposition is not explained and the definitions of the terms in Equation 1 are not particularly clear. My reading is that 'h' is the proportion of total section height, and that 'y' is the proportion of the total section width. This does however omit the basis of r (which is a function of minimum and maximum plan area in Strahler) and makes it a fit parameter. This is useful strategy but Equation 1 is now simply a fitted shape function. In the literature other authors (e.g. Boon and Byrne, 1981; and Townend, 2008) have adapted Strahler for use in the marine environment. The authors here have preferred the original (terrestrially based) Strahler equation. Given that they are all empirical relationships this may be entirely appropriate but some discussion as to why would provide a stronger link with the existing literature.
We now clarify in the text (1) why we adopted the Strahler formulation, (2) why the environment for which it was proposed is less relevant and (3) that we indeed made r and z fitting parameters and use Equation 1 as a fitting function. The text now reads: "In the past, multiple authors have proposed empirical relations for the hypsometric shape of terrestrial landscapes (Strahler 1952) and (partially) submerged bodies (Boon, 1981; Wang, 2002; Toffolon, 2007; Townend, 2008) (see Townend, 2008 for review). All equations, except for Wang (2002), predict a fairly similar hypsometric curve based on the volume and height range of the landform (Townend, 2008). While it is of interest to use these empirical relations to predict the occurring altitude variation of a landform, the framework here is different, because in this case the hypsometric curve is applied across channel width: it is a cross-sectional hypsometric curve. We aim to use the general hypsometric curve to characterise the occurring cross-sectional hypsometry, which is similar to the approach of Toffolon & Crosato (2007) who fitted a power function to 15 zones along the Western Scheldt. To that end, it is less relevant for which environment the hypsometric relation was proposed, as long as it is capable to describe the range of occurring hypsometries. For the case of the estuarine environment (Fig. 3), the hypsometric curve should be able to describe variations in concavity and variations in the slope of the curve at the inflection point. Here we use the original (Strahler 1952) formulation, which is capable to do so, but in principle any equation that fits well could be used."

After the Strahler equation we added the reviewers suggestion that 'h' is the proportion of total section height, that 'y' is the proportion of the total section width and that our approach changes the definition of r (which is a function of minimum and maximum plan area in Strahler) to make it a fitting parameter.

In the light of the above, I would suggest that it might also be appropriate to add the word empirical to both the title and the section entitled 'Relation between morphology and hypsometry'.
We added the word 'empirical' in the manuscript title and section title, as well as in the abstract text.

My other main concern relates to the use of the word 'ideal' in relation to the width of the channel. The study is essentially a geometric one, extracting width information from detailed bathymetries in four estuaries. Without consideration of some other metric such as tidal elevation/velocity, energy dissipation or the energy flux in the system it is not possible to assert a "state" of the system relative to equilibrium and hence to define what constitutes an "ideal" system, as classically defined. Whilst the authors make clear how they have defined their ideal plan form (width at the mouth and river) this only serves to compound a prevailing myth that the ideal is based on convergent width. If the cross-sectional area is exponentially convergent the estuary meets the basis of Pillsbury's original definition for an ideal estuary. If it happens that the hydraulic depth is constant along the channel then the CSA convergence length equates to the width convergence length.

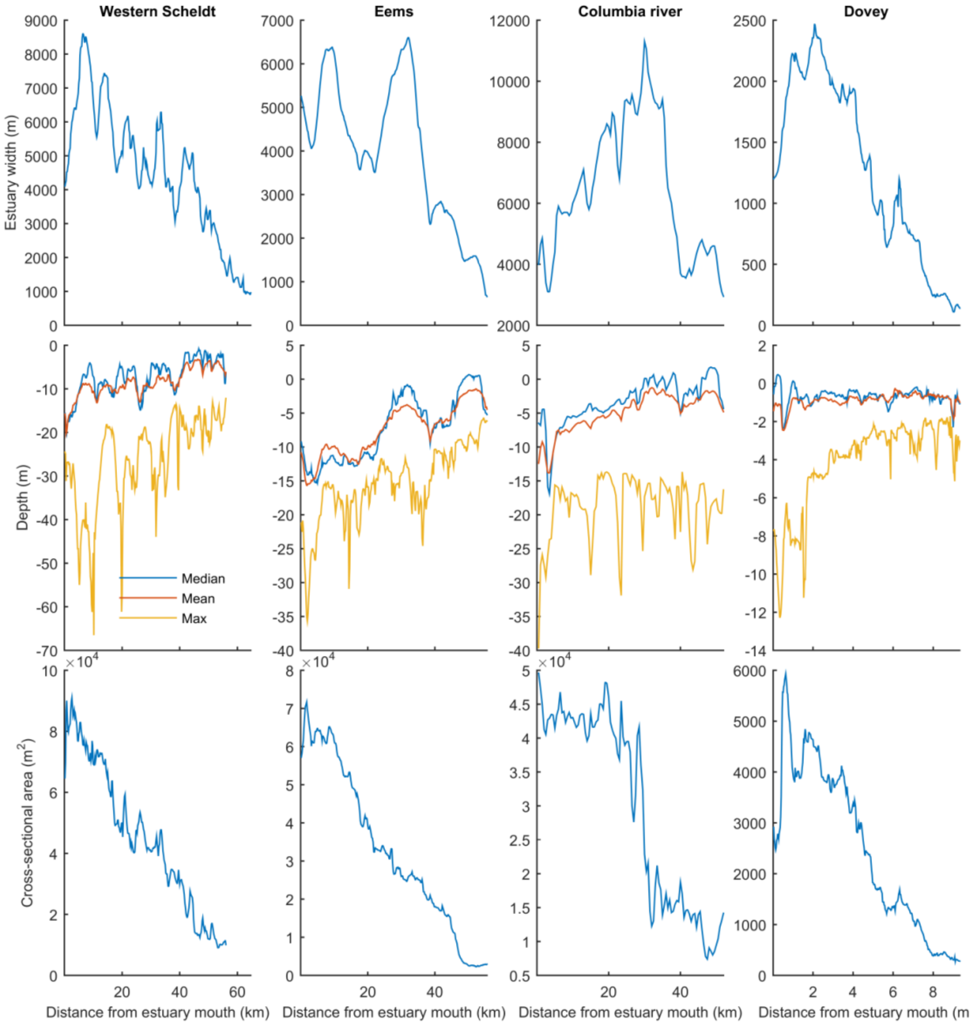
The figure below shows the along-channel profiles of width, width-averaged depth and cross-sectional area for the systems we studied. Cross-sectional area profiles are rather linear than exponentially convergent and along-channel depth profiles are rarely constant, so the estuaries deviate from Pillsbury's original definition for an ideal estuary, but it is precisely the effect of deviation from an ideal shape on bar patterns and bed levels that we are interested in.

The reviewer comments that the equilibrium ideal state of an estuary might be confused with the geometric ideal width that we use in our study. To prevent any confusion between the equilibrium estuary state and the geometric width profile, we now clarify the definition of an ideal estuary in a separate section at the start of the methods and explicitly state its relation with geometric properties. Given that channel width is the only geometric property that we can measure from aerial photography we subsequently explain that deviation from an ideal converging width profile is the only way we can approximate deviation from an ideal estuary shape.

The new text reads:

"A useful model to describe the morphology of estuaries is that of the 'ideal estuary'. In an ideal estuary the energy per unit width remains constant along estuarine channels. This ideal state can be met when tidal range and tidal current are constant along-channel, such that the loss of tidal energy by friction is balanced by the gain in tidal energy per unit width by channel convergence (Pillsbury, 1956; Dronkers, 2017). In case the depth is constant along the channel, the ideal estuary conditions are approximately met when the width is exponentially decreasing in landward direction (Pillsbury, 1956; Langbein, 1963; Savenije, 2006; Toffolon & Crosato, 2010; Savenije, 2015), which also implies an along-channel converging cross-sectional area. However, when depth and friction are not constant along-channel, but for example linearly decreasing in landward direction, less convergence in width is required to maintain constant energy per unit of width. Many natural estuaries are neither in equilibrium nor in a condition of constant tidal energy per unit width. They deviate from the ideal ones as result of varying degree of sediment supply, lack of time for adaptation to changing upstream conditions and sea-level rise (Townend, 2012; de Haas et al., 2017). Whether continued sedimentation would reform bar-built estuaries into proper ideal estuaries remains an open question. For our application, the concept of ideal estuaries is useful to assess the degree of deviation from ideal because of the width variations observed as bars, tidal flats and saltmarsh. While we expect a somewhat different degree of convergence such that the ideal state of constant energy per unit of width is approximately maintained, we do not study the deviation of this convergence length from that in ideal estuaries.

Ideally, we would want to assess the degree to which an estuary is in equilibrium from an aerial photograph, because this is often the only data available. However, the only indicator derivable from aerial photography is channel width and thus deviation from a converging width profile. Therefore, in Leuven et al. (2017), we defined the excess width, which is the local width of the estuary minus our approximation of the potential ideal estuary width. Here, the ideal estuary width is approximated as an exponential fit on the width of the mouth and the width of the landward river. While the empirical measure of 'ideal width' should not be confused with the 'ideal state' of an estuary, it is the only practical way to estimate deviation from an ideal estuary based on the estuary outline only. Moreover, it proved to be a good indicator of occurring bar patterns (Leuven et al., 2017) and will therefore be applied in this paper to study hypsometries."



We considered alternative wording for ideal width in the remainder of the manuscript. Because we now explain how we derived this geometrical property from the concept of an ideal shape and explicitly state that it should not be confused with the equilibrium state, we decided to keep te wording of ideal width. Moreover (1) it is precisely the deviation from an ideal shape that we are interested in, (2) the use of this terminology is in agreement with previous work (Leuven et al., 2017) and (3) other wordings that we considered might lead to misunderstanding, e.g. minimal width convergence can be read as minimal convergence. If the editor prefers different, we will consider the use of minimal width convergence, convergence of minimum width or something similar.

There is some evidence from UK estuaries that width-depth variations provide a degree of system redundancy, allowing the system to adapt and so do minimum work, whilst maintaining the CSA convergence. This is illustrated in the attached figure for the Humber, where the CSA is clearly exponentially convergent. The corresponding width and depth values vary about the exponential fits (seemingly in an inverse manner that it has been suggested is linked to overall channel sinuosity). Importantly in this context the width is invariably narrower and deeper at the mouth for a number of reasons (geology, drift, etc). Consequently, I would reason that the authors have examined the variance from the minimal width convergence. This does not detract from the results but it is important not to confuse a valid conclusion relating to along channel variation in width hypsometry, with assertions relating to an ideal system and its state relative to equilibrium. For the latter, I am of the opinion that we need a physically based determination of the hypsometry, rather than an empirical one.

Thank you, we agree and this case agrees with our findings. See reply to comment above about confusion of ideal width with ideal system state. We now clarify this in a separate section in the methods.

As for the suggestion that we need a physically based determination of the hypsometry: this is the ultimate aim that is presently beyond reach. We added a paragraph to the discussion about this idea, which reads: "Here we found that the cross-sectional hypsometry relates to occurring bar patterns and estuarine geometry. In contrast to an empirical description, ideally, a physics-based determination of the hypsometry would be favourable. However, with the current state of the art of bar theory (Leuven et al., 2016) and relations for intertidal area, tidal prism, cross-sectional area and flow velocities (O'Brien, 1969; Friedrichs & Aubrey, 1988) it is not yet possible to derive a theoretical prediction of hypsometry. For example, bar theory (Seminara & Tubino, 2001; Schramkowski et al., 2002) could predict occurring bar patterns on top of an (ideal) estuary shape, but current theories overpredict their dimensions (Leuven et al., 2016) and it is still impossible to scale these to bed level variations, because the theories are linear. In addition to that, the resulting predictions would need to meet the requirement that the predicted bed levels and the intertidal area together lead to hydrodynamic conditions that fit the estuary as well."

Finally a point of detail. In the discussion, you refer to whole system hypsometry as an oversimplification. However, these whole system descriptions are consistent with the original Strahler concept of a basin hypsometry based on plan area. In a landform context these remain entirely valid descriptions. In terms of estuary dynamics they do not capture the along channel variations. As you note, there can be a significant variation of the high a low water surfaces along the estuary. Consequently, the along-channel cross-section hypsometry should not be assumed to be relative to a fixed vertical datum. Interpreting these along channel variations remains an open question because of the reasons outlined above.

We added the suggestions of the reviewer to the paragraph about the degree to which whole system hypsometry are oversimplifications for estuaries. The paragraph now reads, with bold parts added:

"Previously, hypsometry was used to summarise the geometry of entire tidal basins or estuaries (Boon 1981; Dieckmann 1987; Townend 2008). ***The whole system descriptions are consistent with the original Strahler (1952) concept of a basin hypsometry based on plan area, which is a valid description in a landform context.*** *However, these descriptions oversimplify* ***the along-channel variability*** *in estuaries that are relatively long. These estuaries typically have a linear bed profile varying from an along-channel constant depth to strongly linear sloping (e.g. the Mersey in UK). In the latter case, the elevation at which subtidal and intertidal area occur varies significantly along-channel (Blott 2006). Additionally, friction and convergence may cause the tidal range to either dampen or amplify causing variation in tidal elevation, subtidal area and tidal prism (Savenije 2006).* ***Consequently, the along-channel cross-section hypsometry should be assumed to be relative to an along-channel varying high water level or mean sea level rather than an along-channel fixed vertical datum. Interpreting these along channel variations remains an open question because of the reasons outlined above.*** *Nevertheless, if desired, along-channel varying hypsometry predictions can be converted in one single summarising curve (Fig. 12), which shows that also the basin hypsometry can be predicted when limited data is available."*