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Hydrology and Earth System Sciences Discussions

Interactive comment on "Hydroperiod and hydraulic loading for treatment potential in urban tidal wetlands" *by* T. T. Eaton and C. Yi

Anonymous Referee #1

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General comments: The manuscript presents a method to determine the hydroperiod and hydraulic loading in schematic tidal wetlands, in order to apply the results to help evaluate and assess restoration and construction of tidal wetlands for the treatment of contaminated urban coastal waters. The topic is very interesting, as well, as the approach which combines typically non-concurrent areas like hydrology and ecology. This synergic collaboration can lead to new interesting results. However, the paper, at its current state, lacks the required clarity and mathematical rigour.

Specific comments: The paper proposes a quantitative method for estimating the hydroperiod and hydraulic loading, which is pointed out as scarce in the introduction. However, it's not very clear how the quantitative results are directly used. The text rather suggests a fairly qualitative employment of the results of the presented meth-

ods, also derived from the sometimes not fully explained assumptions that lead to he equations presented. The methods are also developed for very simple assumptions (linear or sinusoidal water level evolution). However, with little additional effort, much more general and, potentially, powerful and interesting results could be derived. The hydroperiod is calculated based on the time that the water depth is strictly positive. Considering all the tidal cycle for the integration wouldn't imply any additional difficulty, but would conceptually be more robust. Even more taking into account that, later on, (eq. 13, page 602, line 21) the average water depth over time is calculated as the ratio of the hydroperiod to the total time that the system is submerged(?). The hydroperiod (1st moment of the positive water depth with teepect to time) should rather be divided by the total time of the tidal cycle, regardless of whether the system is submerged or not. The same can be said about the hydraulic loading calculated later on (eqs. 14 and 15, page 603), which are weighted sums taking into account only the time periods when the particular position of the system is submerged.

The range of applicability of Manning's equation (eq. 6, page 600, line 14) to the specific problem dealt with is, later on in the text, questioned by the authors themselves. However, they still decide to apply to the conditions of a tidal wetland. What is the reason for this decision? Is it then possible to employ that formula according to he authors? Wouldn't it be better to use a different equation relating the flow and the water depth? Have the authors eventually considered using a more advanced hydrodynamical model (e.g. 2-D depth-averaged model which takes into account temrs like inertia and friction in the water mass) to derive a relationship q-h for the proposed simplified model in the case of a tidal system?

The presentation/derivation of the equations specifying the capacity of the system to reduce the contaminant load (eqs. 16 and 17, page 604) is not detailed enough. The assumptions that lead to their derivation are not sufficiently explained.

The comments on the results are fairly qualitative (e.g. "Hydraulic loading increases apparently exponentially...", page 608, line 3), with no rigourous analysis of the obtained

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numbers. A less schematic representation of the system (e.g. with more terraces) would have made it possible to really check the hypothetical exponential relationship between the hydraulic loading (or the hydroperiod) and the distance to the coast line.

Technical comments:

page 593, line 15-16: "..., we define hydroperiod to include the proportion of time over which inundation occurs..." Why not all the time? Why isn't hydroperiod clearly defined in its general sense, before specifying some particular assumptions?

page 593, lines 23-25:"We also show how the resulting spatial distribution of hydroperiod...". Only 4 terraces are considered in the simplified system. Employing the term "spatial distribution" would make ine belive the existence of more points than just 4.

page 594, lines 5-6:"...because of the numerous sources of non-point source runoff...", rep. source.

page 595, lines 7-13: how relevant is the politics done by one specific mayor (Bloomberg) in this work? How is the particular information contained in these lines important to the paper?

page 596, lines 22-23: "...because the water depth and the wetted area change continuously in time and space (h=f(x,t), A=f(x,t)). ..." Does the water depth really change continuously in space?? Isn't there any discontinuity when going from one terrace to another one? Does continuously mean something different from "mathematically continuous"? Are both functions for water depth and wetted area the same h=f(x,t)=A=f(x,t)? Every time a generic (continuous) function is referred to f(x,t) is used, with no further purpose than indicating a generic (continuous) function.

page 597, lines 4-5: water level fluctuation is a translation of water level over time regardless of whether the mean slope is constant or not.

page 597, lines5-8: confusing, wordy.

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page 597, lines 12-13: geometric method is exaclty the same, by definition, as the analytical method (integration of a curve over an interval = area under that curve), pointless comparison.

page 597, lines 19-20: if the integral is done using consistent units, the result of integrating the hydrograph MUST be the total volume of flow.

page 598, lines 23-25: the integral is carried out over the interval 0.75 - 6, why not from 0 using a max(0,f(t)) function? The same holds for eq (5), page 599.

page 600, lines 11-12: _the_ manning's equation, _the_ manning's formulation should read => manning's equation, manning's formulation.

page 600, line 15: what is the parameter a, what does it represent in Manning's equation?

page 600, line 21: "continuously changing", f(x,t), see comment above.

page 602, line 21: h average is evaluated using only the length of time during which the point is submerged, why??

page 604, line 21:if the alternative is only "slightly more complex", but still better, why wasn't it used instead of the one explained in the manuscript?

page 605, line 18: "apparently exponential form of this trend is no doubt due to the bidirectional flow...". What does "apparently exponential" mean? No quantification? Why is it clearly due to the bidirectional flow? Any mathematical explanation? Anyway, very colloquial phrasing.

page 606, lines 14-21: not clear!

page 607, lines 15-23: is this quantitative or qualitative use of the developed method? How are the results (numbers) used??

page 608, line 3:"apparently exponential...", see comments above.

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page 608, line 9: why is that suggested?

page 609, lines 10-14: isn't there no dependence of hydraulic loading and hydroperiod on the distribution of the terraces? Is the influence of the tide so strong?

page 618: Table 1: column "geometirc area" is irrelevant, as explained before. "Equation integration" is a vague definition, numerical integration is also a way of integratiing the equation. It should rather be distinguished between "Analytical integration" and "numerical integration"

page 619, table 2: column parameter a doesn't really correspond to one column in the table. It is general information cooon to all the table, add to table legend.

621, table 4: difficult to read. It should be easier to distinguish the two cases of parameter a.

page 623, figue 2: legend should be not intertidal distance, but inland distance, or similar.

Page 625, figure 4: four points in each of the series are too few. It is fairly simple to even integrate analytically for a continuous slope (no terraces) and derive not some points but a continuos plot. Anyway, because the points are so few, the figure would be easier to read by joining the points corresponding to each of the series, even if the lines don't represent anything but the relationship among the points.

Summary: the paper presents a very interesting approach to merge two fields which can complement producing important new results. The method is simple, but it lacks rigour. With not much more effort, much more general results could be presented. The results need to be calculated according to the applicavle conditions of the tidal wetlands (not Manning's law for such a changeable inertial system). This manuscript shouldn't be published before all these points, among others, have been sufficiently addressed. Then it will present a very useful interesting advance.

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