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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Conservative finite-volume forms of the Saint-Venant equations for hydrology and urban drainage" *by* Ben R. Hodges

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Response to Anonymous Referee 1

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I thank the reviewer for the time and effort they put into reviewing this manuscript. The comments have helped me see the problems in the exposition of the original paper, and have inspired a substantial rewrite. The complete rewrite is uploaded as a supplement file with blue fonts used for major changed sections.

In the following response, I will first provide an overview of the changes that have been made in the manuscript, and then address the specific comments. Finally, I will provide a summary of where my response differs from the reviewer's comments and my reasons why.

Page and line numbers for the new manuscript are provided in brackets. Page and line numbers in parentheses are associated with the original manuscript.

1 Overview of revision

The rewrite has included a completely new Introduction section, which provides an overview of recent arguments about the SVE in large-scale hydrology. As a matter of personal preference in writing style and readability, the number of citations in the introduction is kept to a minimum and the reader is referred to succeeding sections for details.

The section that previously constituted the Introduction, which details the setting for understanding the SVE forms, is now a Motivation section. The changes in this section include some new citations on conservative vs. non-conservative forms [lines 25-30] and the presentation of the differential form of the equations using a free surface [eq 3], which is contrasted to the slope form of the equations [eq 2] and a new discussion of smoothness in the source term [lines 17-25]. There are also minor additions in this section to address specific comments (see below).

In the rewrite, the Background section has be subdivided into several topic areas - *Origination of the SVE, Preissmann v. Godunov methods,* and *"Well-balanced" problems and S_0*. Some minor additional text was added in a few places to clarify problems noted in the specific questions of the reviewer (see responses below). I disagree with the reviewer on what detail in model methods and discretizations should be presented in the Background, as discussed in the Disagreements section below.

For the Finite-Volume SVE section of the manuscript, the reviewer's comments indicated that I had not explained issues with the clarity that I had hoped. I have substantially revised the presentation in this section. The derivation is now divided into subsections to help delineate the arguments. The use of piezometric pressure and the fact that it is uniform along a vertical face is now discussed in a single section [pg 12,

lines 17 – 29] rather than implied in the equations. The explanation of the stair-step approximation, which resulted in the greatest confusion in the initial manuscript, has been improved with new figures and text from [pg 14, line 10] to [pg 19, line 16].

The section on Approximate Finite-Volume Forms in the manuscript has seen substantial additions, including 2 new figures [Figs 7 and 8] and text [pgs 21-22] to explain the meaning of the discrete quadrature of polynomial approximations of $\eta(x)$ and $\lambda(x)$.

The Discussion section of the manuscript now includes an enhanced discussion of S_0, the well-balanced condition, and smoothness [pg 29, lines 18 – 28] and [pg 30, lines 3 – 20].

2 Specific questions and response

Below are restatements of the specific questions of the reviewer and my responses.

REVIEWER: (Pg 2 Line 26): breadth = width ?

Yes. [pg 4, line 3] Breadth is used as synonym for width as using B is less confusing than W in the nomenclature. The latter can be confused with a vertical velocity. Some minor clarification in the text has been provided.

REVIEWER: Equation 8. Explain why f1 and f2 are necessary in eq 8 while it was not necessary in eq. 7.

Done. Now eq. 9. The f_1 and f_2 are intended as generic (unspecified) functions that have a similar form to Cunge-Liggett. This has been clarified by re-writing the equation and revised text [pg 4 line 20 – pg 5, lines 1–4]

REVIEWER: Page 4 line 7. Review the frictional force per unit area

Done. Corrected to F and clarified that its frictional force at the bottom of the channel. Thank you for your detailed reading that caught this typo [Pg 5, line 20].

REVIEWER: (pg 4 lines 17 – 29). Also review recent work using local inertia approximation from Bates et al. 2010 for large scale river networks.

Partially Done. [pg 6, lines 3 – 5] I have included the paper by Getirana et al (2017) as an example of the adaption of the local inertia approach to large scale modeling. I have also added a sentence referring to Hodges (2013) for arguments on SVE v. reduced physics models so as not to repeat the discussion here. I have not included a citation to the Bates et al (2010) paper as it was proposed as a 2D method for flooding rather than a 1D method for hydrology. Similarly, I have not included deeper references to the origins of the Muskingum or kinematic wave methods but instead have provided more recent references on their application. These few background sentences are to acknowledge that there are simplified forms for the SVE that are used in large-scale hydrology as the full SVE are considered to difficult to use. See also the section below on my disagreements with the reviewer.

REVIEWER: (pg 4 lines 17 – 29). Review work for short scale using HEC-RAS.

Done. [pg 6 lines 11 – 14]

REVIEWER: Please discuss explicit vs implicit schemes. Discuss local inertia explicit formulation. Review 6 point Abbot and Ionescu scheme (1967). Explain that Preissman scheme is a finite difference based on integral relations that improve conservation.

Partially Done. I have added an additional explanation [pg 6, lines 27 – 29] that clarifies why I am focusing only on the most fundamental issues (finite difference vs. finite volume) and not the details of the schemes (explicit vs. implicit). As the reviewer requested I have also added the Abbot and Ionescu paper, but only as one of the non-Preissmann finite-difference schemes. I have *not* added a discussion of 6 point v. 4 point finite-difference schemes. My reasoning is the Abbot and Ionescu is clearly related to the Preissmann scheme, but very different from Godunov methods, which is the key point for the background comparison. See also the section below on my disagreements with the reviewer.

REVIEWER: Please use more figures to help the reader to understand the paper

Done. See new Figures 1, 4, 5, 6, 7, and 8.

REVIEWER: Please use a figure to define the control volume

Done. See new Figure 1.

REVIEWER: (Page 8 line 2.) "This vector is local and change along the channel . . ..". Please use a figure to clarify it

Done. Refer to new figure 1 and revised text [pg 11, lines 2 – 3].

REVIEWER: (Page 8 line 25.) "It is known that . . ..". Please cite references showing these arguments.

Done. I have rewritten this section for clarity and provided reference to a typical work on flow in bends. [pg 11, lines 25 – 30]

REVIEWER: Figure defining the free surface slope n(x), these angles, etc.

Done. See new figure 1.

REVIEWER: Eq 25. Why P(x) is independent to z?

Done. Note that Eq. 25 is now Eq. 32 – this issue has been clarified with the definitional statement of the piezometric pressure as $\rho g \eta$, which is provided in the new subsection [pg 12, lines 17 – 30]

REVIEWER: How the shape of cross section and its variability is considered?

This is the purpose of the stair-step model, the A_R term, and λ. The section regarding the bottom pressure and topography have been significantly re-written to explain the A_R, as discussed in the Overview, above. See [pgs 14 – 19]

REVIEWER: Page 12 equations 27, 28, 29. What is m? What lambda represents? Is it just a mathematical trick ? Or does it have any physical meaning? How it relates to zb(x) ? How it relates to river cross section shape and its variation along x? Why int (lambda)= 1 ? (eq 28)

Done. These are now the revised eq. 27 – 40. Hopefully it is clear that m is simply an index in a summation and the λ is a weighting function that represents the distribution of the stair steps areas over the length of the bottom. The requirement that $\int \lambda \, dx = 1$ is a mathematical identity based on eqs 37 – 39 so that the sum of all the stair-step areas is equal to the difference between A_d and A_u.

REVIEWER: Equation 32. Ve is confusing here. It is volume but one can make a confusion with flow velocity. Please, use a good figure to define the control volume, what is Ve, Le, upstream and downstream cross section, etc.

Done. Figure 1 provides nomenclature. Note that this is now eq. 43. I agree that V can be confusing, but I have been consistent throughout in using V for volume in all the integration and U or u for velocity. As a matter of personal preference, I dislike use Ω for volume as is done by some authors.

REVIEWER: Equation 33. How gAn was obtained? Define how gAn is derived from P, using water surface n instead of depth H.

Done. Eq. 33 is now eq. 45. Additional text [pg 20, lines 15 – 19] has been added to explain the substitutions from eq. 44 to 45. The relationship between piezometric pressure and the free surface is given in the new subsection [pg 12, lines 16 – 30] and eq. 23.

REVIEWER: Section 4. Please define the meaning of lambda function. It is important to understand the choice for polynomial approximations.

Done. See eq. 38 and discussion on [pg 19, lines 8 – 17]

REVIEWER: Please show in a figure the approximations T(0,0), T(1,0), T(2,0), T(1,1) of n(x) and lambda(x) or zb(x).

Partially Done. I disagree with the reviewer on the usefulness of a figure illustrating the difference between zeroth, first, and second-order polynomial interpolation. However, the reviewer's comment has pointed out that I was not clear on explaining what was being done. I have extensively re-written the description of the approximation approach, beginning [pg 21, line 22] and ending on [pg 24, line 5]. I have included two new figures that I think are helpful in illustrating key concepts. Also, new text has been added on [pg 26, lines 4 – 6] and [pg 27, lines 1 – 3] to clarify the meaning of the $T_{e(1,1)}$ approximation.

REVIEWER: Equation 60. Check it. I guess the last term is g.Ae.Le.Sfe.

Done. This is now eq 72. Thank you for catching the typo.

REVIEWER: Pg 19, line 22. "S0 brings a host of problems". What problems? Please show it based on past publications.

Done. See revised explanation [pg 29, lines 22 – 28]

REVIEWER: Eq. 62. Not clear

Done. This was speculation on future uses. As it was not clear I have removed the equation and revised the discussion [pg 30, lines 21 – 28].

REVIEWER: Final discussions and conclusions. In my view, the paper would be much more convincing if a few examples showing that application of the new forms of SV equations provide the same results as other classical forms or better results in critical cases

Not done. I disagree with the reviewer on this point. Please see the discussion below.

3 Disagreements and rebuttal

The reviewer has made suggestions on three major points where we disagree: (1) extended background on reduced-physics models, (2) extended background on numerical discretization schemes, and (3) examples of numerical implementation of

the new governing equation form and comparison against other methods. Although I appreciate the reviewer's viewpoint and the usefulness of each of these ideas, adequately addressing each would make this paper overly-long and substantially diffuse the principle message of the paper. Each of these issues is addressed separately below.

Extended background on reduced-physics models

My interpretation of the reviewer's comments is that they believe there should be greater comparisons/background with reduced-physics methods and/or some attempt to demonstrate that there are problems with the SVE in large-scale models. That is, the reviewer appears to want me to prove that there is a problem with reduced-physics models before I can present a potential solution. Unfortunately, our present approach to scientific publication makes it difficult (if not impossible) to present a paper that shows the difficulties of using SVE for large-scale systems – our journals are biased towards presentation of success rather than analyses of failure. I have added citations to forum papers where this issue has been discussed, but I believe the overall issue is beyond the scope of this manuscript and is not necessary to put the present work into context. That is, the theoretical derivation of the new finite-volume method is a new advance whether or not reduced-physics models are adequate. Thus, an extended discussion of reduced-physics models will detract from the focus of the paper on showing a different form of governing equations that are compared to other full SVE governing equations.

As a matter of philosophy, in my view the SVE are the fundamental governing equations for river flow so the fact that they are *not* used for large-scale hydrological modeling is all the evidence that we need that there are problems with the SVE. It should be incumbent on those presenting reduced-physics models to show that their neglect of dynamics is well founded rather than merely convenient. Unfortunately, reduced-physics models typically rely on comparison of calibrated results to observations rather than analyses showing that the missing physics are unimportant. Thus, results from reduced-physics models cannot distinguish whether they correctly neglected unimportant physics or merely calibrated an important (but neglected) piece of physics within another term.

Extended background on numerical discretization schemes

I do not think that a detailed discussion of explicit vs. implicit methods, the number of stencil points, or the motivation for the Preissmann stencil relative to other stencils etc. is appropriate for this paper. I am presenting a new form of the governing equation for momentum – i.e. an integral form that is suitable for finite-volume discretization. Thus, the Motivation and Background sections focus on the prior differential and integral forms of the SVE rather than on the details of the numerical schemes used to solve the equations. I have only discussed different numerical techniques (Preissmann and Godunov) to provide a convenient way to classify the basic methods and introduce how others have dealt with problems of conservation, well-balanced methods, and bottom slope. All of these issues have been the subjects of discussion within the cited literature.

Examples of numerical implementation of the new governing equation form and comparison against other methods

I agree that the the manuscript would be "more convincing" of the value of the new equation form with one or more discrete numerical examples. However, it would also be twice as long. This would be a problem as the paper has already reached 12000 words. To keep the paper within reasonable length, I am focused only on the

general formulation of the equations rather than the detailed numerical implementation.

This manuscript actually evolved from a working paper on a new numerical scheme with a particular discretization method; however, whereas most numerical methods papers can introduce the differential or integral form of the governing equations with a single paragraph, it became clear that the approach I wanted to take would require many pages to present and justify the new integral finite-volume form. As the manuscript grew in length, it soon became clear that there were really two papers: (1) the theoretical derivation of the new integral form, presented here, and (2) a numerical implementation of the new form and analyses of its behavior – which is under review at another journal.

My argument for *not* presenting an example numerical scheme and results is that *whether the new form is valid or not is a matter of mathematical derivation and proof, which does not depend on a specific implementation*. Similarly, de Saint-Venant's paper was a valid and useful contribution despite the inability to actually solve the equations in 1871.

4 A final note

Again, I thank the reviewer for their thoughtful comments. Their opinions and ideas have forced me to revisit my methods of presentation and provide clearer explanations for the mathematics of the derivation. I hope they find the end result pleasing and recognize that they have made a positive addition to my work.