

Tsunami modeling at lake-scale using non-linear shallow water equations

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Dear Authors,

I read your manuscript with a lot of interest. A mountain lake is not a small ocean: the waves are shorter, the water is shallower, the basin is closed, the shores are steep, and people live a few hundred metres from the water. Yet most of what we know about tsunami modelling comes from the ocean, and an engineer who has to produce hazard maps for a Swiss lake wants to know which solver to trust and what it costs. You ask exactly this question: can the hydrostatic nonlinear shallow-water equations, solved by the finite-volume codes BASEMENT and GeoClaw, replace a dispersive solver such as BoussClaw at lake scale? You answer in two steps. In the first, a solitary wave of amplitude A between 0.1 and 8 m is sent along a flat channel of 6.4 km, with depth between 25 and 200 m, and the three codes are compared through the height and the duration of the pulse at nine gauges. In the second, BASEMENT alone is compared with the laboratory measurements of Fuchs of the run-up and of the depth of overland flow after a solitary wave has climbed a plane slope. You conclude that the hydrostatic codes can be used in most lake settings, with two limits, a large ratio of amplitude to depth and a long distance, and that BASEMENT is a fast and accurate tool for hazard assessment.

Let me say first that the question is the right one and that the study can become a useful one. A systematic sweep over depths and amplitudes, with the same input wave for three codes, is what practitioners would like to have, and your appendices print every number, which allowed me to check your work in detail. I also enjoyed the use of one laboratory data set for both run-up and overland flow.

However, I could not follow the conclusions all the way, and my recommendation is therefore a major revision. I say it at once, and I also say at once that every defect I found can be repaired, and that most of what is needed is below, with proofs. Four points organise the report. First, the tables of the appendices and the figures of Section 5 do not show the same quantity: the tables are computed from the total depth and the figures from the elevation, so that the tables understate every amplitude error by the factor $(1 + \varepsilon)/\varepsilon$, between 4 and 1000 ([Remark 6](#)). Second, the three codes are compared with the initial wave, which is the exact solution of none of them, and the losses of the hydrostatic codes mix two things that a good study should separate: the physics of the hydrostatic equations, which is governed by one dimensionless number derived in [Appendix III](#), and the numerical diffusion of first-order schemes ([Remark 4](#), [Remark 7](#) and [Remark 8](#)). Third, in your own Figure 10 the run-up of BASEMENT increases with the steepness of the slope while the laboratory run-up decreases; an independent solution of the hydrostatic equations shows that this is what the equations do, so that the code is right and the test shows the limits of the model, and the sentences of Sections 7 and 8 about slopes and about a conservative result say the opposite of what the figure shows ([Remark 14](#) and [Remark 15](#)). Fourth, the conclusions about lakes, about cost and about interchangeable solvers go further than a flat channel and a solitary wave can support ([Remark 21](#)).

The report follows the order of the manuscript, from the equations to the conclusions. The smaller remarks come page by page, and the mathematics that I offer is in the appendices, each of which is

written so that you can drop it into your text if you wish. Please treat every point as a proposal and not as a verdict.

THE EQUATIONS AND THE INPUT WAVE

1. The dispersive system as printed is not the system of Madsen and Sørensen. Equations (5)–(7) cannot be the system that BoussClaw solves. In (6) the function Ψ is exactly the operator D of (7) applied to $N = \partial_x(q_x^2/h) + gh\partial_x\eta$, and in (5) the factor $(1 - D)$ multiplies $\partial_t q_x$ alone. Linearised about the state at rest over a flat bed, this system has the dispersion relation equation (R2) of Appendix I, $\omega^2 = ghk^2(1 - \beta K^2)/(1 + \beta K^2)$ with $K = kh$ and $\beta = B + 1/3$. It differs from the relation of Airy already at order K^2 (unless $B = -1/6$), it is not Peregrine’s relation [R1] for $B = 0$, and for $B = 1/15$ it has $\omega^2 < 0$ when $K > \sqrt{5/2}$, with growth rates that increase with k : the initial value problem is ill-posed. In the form given by Kim et al. (2017), and by Berger and LeVeque [R2] in their Section 3.1, the factor $(1 - D)$ multiplies the whole hydrostatic momentum operator, and Ψ contains one more term, $-Bgh_0^2\partial_x^2(h_0\partial_x\eta)$; with these two changes the dispersion relation is equation (R3), the Padé approximant of Airy’s relation for $B = 1/15$ (Figure R4), and your equation (8) is correct as it stands. I believe that this is a misprint and not a difference with your code, and of course I will be glad to be corrected.

Three more things belong in the text. The value of B is never given (presumably $1/15$, the choice of Madsen and Sørensen [R3]). The depth h_{ref} in (6) and (7) should be the local still-water depth $h_0(x)$ of Kim et al. and not a constant, which would limit the model to a flat bed in a paper about lakes with steep shores. And it should be said whether the breaking switch of BoussClaw, and the switching off of the dispersive terms near the shore, were active in your runs. The equations are written in one dimension while the runs are two-dimensional; the two-dimensional operator is in Section 3.2 of [R2].

2. Signs, definitions and the friction law in Section 2.1. In equations (2) and (3) the right-hand side is $gh(S_{bx} + S_{fx})$ with $S_{bx} = \partial_x b$ and $S_{fx} = u|u|/(gc_f^2 h)$. If b is the bed elevation, the bed term has the wrong sign: the lake at rest is a solution only if b is the depth below a datum. More seriously, since S_{fx} has the sign of u , friction as printed accelerates the flow: for a uniform flow $\partial_t q = q|q|/(c_f^2 h^2)$, whose solution $q = q_0/(1 - q_0 t/(c_f^2 h^2))$ blows up in finite time. I assume that these are misprints; the form $gh(S_{0x} - S_{fx})$ with $S_{0x} = -\partial_x z_b$ and $h = \eta - z_b$ is what one expects. The definition $h = h_{\text{ref}} + \eta$ of page 3 holds over a flat bed only, while Section 4.3.2 uses a slope. The source S_h of the mass equation has the units of a velocity, so it is a discharge per unit area and not per unit width.

The quantity c_f is called the ‘dimensionless friction coefficient’, but with $c_f = 5.75 \log_{10}(12h/k_b)$ (page 9; I assume a decimal logarithm, for which $5.75 = \ln 10/0.4 \approx 18/\sqrt{g}$) it is the dimensionless Chézy coefficient $C/\sqrt{g} = U/u_*$, the inverse square root of the drag coefficient, $C_d = 1/c_f^2$. This is the rough-wall law of Keulegan [R4], continuous at $h = k_b$ where $c_f = 6.2$, and valid for turbulent flow over a rough wall with h much larger than k_b . For $k_b \rightarrow 0$ one has $c_f \rightarrow \infty$: the bed is not smooth, it is frictionless. This explains what you call ‘somewhat surprising’ on page 17 (lines 340–343): the run with $k_b = 0$ has the largest error because it has no friction at all (see also Remark 19).

3. What the hydrostatic equations dissipate, and rules (i) and (ii). Page 3 (lines 77–78) says that hydrostatic models work well for wave fronts ‘due to their natural dissipation mechanisms, which prevent numerical oscillations’, and page 4 (lines 95–96) that the hydrostatic formulation ‘can result in excessive dissipation’ for short waves in deep water. Smooth solutions of the hydrostatic equations

conserve energy, $\partial_t E + \partial_x F = 0$, and energy is dissipated only in shocks. For a bore that joins the depths h_1 and h_2 with the mass flux m through the shock, the Rankine–Hugoniot conditions give the speed $s^2 = gh_2(h_1 + h_2)/(2h_1)$ into still water and the dissipation rate

$$D = \rho g m \frac{(h_2 - h_1)^3}{4h_1 h_2}$$

per unit width, where ρ is the density; a bore of 4 m on 25 m of water dissipates 95 kW per metre of crest. The ‘natural dissipation’ is therefore the dissipation of shocks, which is physical for a bore and which in your tests begins when the pulse breaks (Remark 7); it is the numerical scheme, and not the equations, that suppresses oscillations. What you call ‘excessive dissipation’ of short waves is, in your own tests, premature breaking together with a phase speed which is too high, by 3.9 percent at the wavenumber κ_0 of the largest pulse of Table 1 and by 16 percent at the wavenumber below which 99 percent of its energy lies (Appendix I). The criterion (ii) of page 3, ‘amplitudes A with $A \ll h_{\text{ref}}$ ’, should be dropped: the hydrostatic equations are fully nonlinear and need no small amplitude, while criterion (i) cannot distinguish your cases (Remark 7). I would add the mass and the energy of the pulse to the diagnostics at each gauge: all the models conserve the mass, so that a change of it reveals a leak of the scheme or of a boundary, while the smooth hydrostatic flow conserves the energy $\int (Hu^2/2 + g\eta^2/2)dx$, so that its decrease before breaking measures the dissipation of the scheme and its decrease after breaking that of the shock.

4. The reference of every error is the exact solution of none of the three models. Equations (9) and (10) are the exact solitary wave of the Serre–Green–Naghdi (SGN) equations, $\kappa^2 = 3A/(4h^2(h + A))$, $c^2 = g(h + A)$ and $u = c\eta/(h + \eta)$; I checked this symbolically, and the wave is given in closed form, and used as an exact test of a finite-volume scheme, by Dutykh, Clamond, Milewski and Mitsotakis [R5]. It is not a solution of the hydrostatic equations, which have no solitary wave, nor of the system (4)–(7), nor of the Euler equations. Yet every ‘error’ of Section 5 is measured against it, through the amplitude and the duration at gauge 1. Appendix II gives the consequences with proofs and numbers, and I summarise them. For the hydrostatic equations the data are not a simple wave: the right-going part has the amplitude $A_R = (1 - \varepsilon/8 + O(\varepsilon^2))A$, that is 0.995, 0.990, 0.981, 0.965 times A for $\varepsilon = 0.04, 0.08, 0.16, 0.32$, and a left-going wave of height about $\varepsilon A/8$ leaves the pulse; so a part of the loss that you see at gauge 1 is not numerical. For the system (4)–(7) the solitary wave has the speed $c^2/(gh) = (\varepsilon^2/2 + \varepsilon^3/6)/(\varepsilon - \ln(1 + \varepsilon))$ whatever B , and it is wider than the SGN wave by 0.9 percent at $\varepsilon = 0.04$ and 6.4 percent at $\varepsilon = 0.32$. For the Euler equations, the SGN speed is right to 0.3 percent, but the SGN wave is 0.7 percent too wide at $\varepsilon = 0.04$ and 8 percent too wide at $\varepsilon = 0.32$ (Table R3).

I would therefore compare each solver with the exact solution of its own model when there is one (the SGN wave for an SGN solver, the Madsen–Sørensen wave for BoussClaw, a converged solution for the hydrostatic codes), and all of them with the exact solitary wave of the Euler equations, which is the physical reference. The latter can be computed to machine precision by a short algorithm, given by Clamond and Dutykh [R6] for the fully nonlinear solitary wave of the Euler equations. I would also say in the text which wave (9)–(10) is; it appears in exactly this form in [R5], which also gives the original sources.

5. Section 4.2: why a solitary wave, and what the cited papers show. Page 7, lines 168–169, say that Hammack and Raichlen (1972), Hammack (1973) and Segur (1973) ‘showed that a sudden positive or negative change in the water level ... results in a solitary wave’. For a negative change this is not what these authors found: Hammack [R7] writes in his abstract that a negative displacement produces only a dispersive train of waves, with no solitons, and Hammack and Segur [R8] say the

same. Appendix IV gives the short proof (the Schrödinger operator of the Korteweg–de Vries equation has no bound state for a depression and at least one for an elevation) and the corrected sentence.

Second, the relation $x/h_{\text{ref}} \approx 0.062\varepsilon^{-3/2}$ that you take from Madsen, Fuhrman and Schäffer [R9] gives $x = 775$ m for $h_{\text{ref}} = 100$ m and $A = 4$ m, which you call ‘still (too) large, but much closer to what is realistically possible’. In that paper, however, the number 0.0621 belongs to equation (31) and is, for a rectangular initial disturbance, the distance at which the dispersion of the leading waves, which decreases with time, matches the nonlinearity, so that the Korteweg–de Vries equation becomes the relevant model; it is not the distance at which a solitary wave forms. For the separation of the first soliton the paper gives $7.9\varepsilon^{-3/2}$ (equation (34)), which is 99 km in your lake case, and $\varepsilon^{-3/2}$ for the scale of the nonlinear steepening (equation (20)), which is 12.5 km. In your runs the wave starts as a formed soliton, so that a distance of formation is not the relevant scale, and 775 m is 12 percent of your channel in any case. The relevant scale for the hydrostatic equations is the breaking distance of Appendix III, which is $h_{\text{ref}}\varepsilon^{-3/2}(1 + O(\varepsilon))$, and 13.8 km in your lake case, to be compared with the 6.4 km of the channel. Third, I could not verify that Goring and Raichlen (1978), which I take to be a report by Goring alone, and Yeh et al. (1994), whose subject is the amplification of tsunamis at coastal boundaries, state that a tsunami takes the shape of a solitary wave after a long propagation; please give page numbers.

Fourth, the solitary wave is conservative only for the first wave. For wave trains, resonance between the wavelength and the slope can amplify the run-up of waves that follow the first one, and dispersion reduces the maximum only slightly, as Stefanakis, Dias and Dutykh [R10] show in the nonlinear shallow-water equations. The sentence of lines 179–181, and the use of the solitary wave as a ‘conservative’ choice in Sections 7 and 8, should be limited accordingly.

THE PROPAGATION STUDY (SECTIONS 4.3.1 AND 5.1, APPENDICES A TO D)

6. The tables and the figures show different quantities. The amplitude change that you tabulate in Tables C1 to C4 and D1 to D4, and in Table A2, is not the one shown in Figures 7 and 8, although the printed definition (page 12, line 281) is the same. The ordinate of Figure 6 is the total depth h , from 50 to 54.5 m, and the tables are computed in this way: dividing the values of Figure 7 by those of Table C, I find for every curve a constant, with a spread below one percent, equal to $(h_{\text{ref}} + \eta_1)/\eta_1$, where η_1 is the amplitude at gauge 1. For $h_{\text{ref}} = 25$ m this gives $\eta_1 = 0.49, 1.92, 3.77$ and 7.31 m for $A = 0.5, 2, 4$ and 8 m (BASEMENT and GeoClaw), and the heights at gauge 9 of Figure 6 are reproduced to 2 cm. The entry -7.367 of Table C1 ($A = 4$ m) is therefore a loss of 56 percent of the amplitude at gauge 1 (the value read in Figure 7), and the tables understate every amplitude error by the factor $(h_{\text{ref}} + \eta_1)/\eta_1$, which lies between 4 and 1000. The ‘improvements of up to 20 percent and of more than 35 percent’ of page 15 (lines 303–305) come from Figure 8; in the measure of the tables they are 1.45 and 8.74 percentage points. Table A2 has the same measure as the tables, and the axis of Figure A1a, ‘ $\max_t A_s - \max_t A_{\text{ref}}$ ’, is a difference, while Table A2 gives the same numbers in percent.

Two further consequences follow. The waves have already lost 1.9 to 8.6 percent of their amplitude at gauge 1 when $h_{\text{ref}} = 25$ m, and 0.4 to 3.4 percent when $h_{\text{ref}} = 50$ m, which is reported nowhere; and the reference of Figure 5 (the analytic amplitude A) is not that of Figure 7 (the amplitude at gauge 1). I suggest to define $A_s = \max_t \eta(x_s, t)$, to give amplitude changes in this measure only, and to say relative to what. Table R1 gives the elevation measure of the entries of Tables C and D at 6.4 km, together with a converged solution of the hydrostatic equations, which I describe in Remark 8.

h_{ref} [m]	A [m]	Π	converged hydrostatic	first order			second order	
				BM	GC	BC	GC	BC
25	0.1	0.065	0	-1.76	-2.26	-2.26	–	–
	0.5	0.72	0	-6.53	-9.74	-8.77	-0.20	-0.10
	1	2.05	-4.7	-15.21	-18.72	-13.81	-6.63	-0.34
	2	5.8	-32.4	-35.25	-37.06	-20.34	-33.47	-0.77
	4	16.4	-55.2	-53.41	-54.60	-28.54	-54.85	-0.66
	8	46.3	-70.2	-65.41	-66.07	-37.99	-68.32	-1.93
50	0.1	0.011	0	0	-0.50	-0.50	–	–
	0.5	0.13	0	-1.01	-1.51	-1.51	–	–
	1	0.36	0	-2.04	-2.81	–	-0.05	-0.05
	2	1.02	0	-3.51	-5.46	-4.50	-0.10	-0.23
	4	2.9	-12.7	-16.67	-17.79	-7.10	-14.45	-0.63
	8	8.2	-39.0	-39.53	-40.22	-10.22	-39.58	-1.17
100	0.1	0.002	0	0	0	0	–	–
	0.5	0.023	0	-0.20	-0.20	-0.20	–	–
	1	0.064	0	-0.30	-0.40	-0.40	–	–
	2	0.18	0	-0.51	-0.71	-0.71	0	0
	4	0.51	0	-0.94	-1.33	-1.30	-0.03	-0.16
	8	1.45	0	-2.56	-3.21	-2.29	-0.55	-0.45
200	0.5	0.004	0	0	0	0	–	–
	1	0.011	0	0	0	0	–	–
	2	0.032	0	-0.10	-0.10	-0.10	–	–
	4	0.09	0	-0.15	-0.15	-0.15	0	0
	8	0.26	0	-0.26	-0.31	-0.34	0	-0.03

Table R1. Amplitude change between gauge 1 and gauge 9 (6.4 km), in percent, in the elevation measure. The shaded column, ‘converged hydrostatic’, is a converged solution of the hydrostatic equations for the data (9)–(10), with an uncertainty of about half a point after breaking and zero before it; the other columns are your Tables C and D converted by $E = T(h_{\text{ref}} + A)/A$, where T is the printed entry. This assumes that the amplitude at gauge 1 is A ; the values read in Figure 7, which refer to the amplitude at gauge 1, are smaller by up to 5 points in modulus at 25 m. BM, GC and BC stand for BASEMENT, GeoClaw and BoussClaw; a dash means that the case is not in your tables, and Π is $\varepsilon^{3/2}s/h_{\text{ref}}$ at $s = 6.4$ km.

7. One number governs the hydrostatic losses. Your two limits, ‘ $\varepsilon \geq 0.08$ ’ and ‘a propagation distance greater than 6 km’ (page 21, lines 432–447), are really one limit, and it is exactly neither of them. The reason is a scaling that I prove in [Appendix III](#) and that, I believe, explains most of your results. For a pulse $\eta = A \operatorname{sech}^2(\kappa_0 x)$ with $\kappa_0^2 = 3A/(4h^3)$, the hydrostatic evolution is, to leading order in ε , the inviscid Burgers equation in the variables $X = \kappa_0(x - c_0 t)$ and

$$\Pi = \varepsilon^{3/2} \frac{s}{h}, \quad (\text{R1})$$

where $s = c_0 t$ is the distance travelled; in the same variables the Korteweg–de Vries model has this pulse as an exact travelling wave. All the pulses of your Table 1 are therefore one and the same pulse, seen at different values of Π . It breaks at $\Pi = 1$, that is at the distance $h\varepsilon^{-3/2}(1 + O(\varepsilon))$; its maximum stays unchanged up to $\Pi_c = 1.45$ and decays afterwards, tending to $1.755/\sqrt{\Pi}$ (Figure R5). At 6.4 km the values of Π are 0.06 for $(h_{\text{ref}}, A) = (25 \text{ m}, 0.1 \text{ m})$, 0.72 for $(25, 0.5)$, 2.05 for $(25, 1)$, 5.8 for $(25, 2)$, 16 for $(25, 4)$, 46 for $(25, 8)$, then 1.0, 2.9 and 8.2 for $(50, 2)$, $(50, 4)$ and $(50, 8)$, 1.4 for $(100, 8)$, and less than 0.52 in the other thirteen cases. The leading-order curve predicts losses of 5, 35, 59 and 75 percent of the amplitude for the four cases at 25 m with $A = 1$ to 8 m, and of 14 and 44 percent for $(50, 4)$ and $(50, 8)$; a converged solution of the hydrostatic equations, which includes the corrections of order ε , gives 5, 32, 55, 70, 13 and 39 percent (Table R1).

Your own data confirm it. Once the second-order GeoClaw results of Tables D are converted to the elevation measure and plotted against Π , they fall on the curves of the hydrostatic equations

(Figure R6); the large losses at large ε are the exact dynamics of the hydrostatic equations, which break too early, and not errors of either code. The practical criterion is $\Pi < 1$, or $\Pi \leq 1.45$ for the maximum, that is $\varepsilon < \varepsilon_c = (1.45h/s)^{2/3}$: at $s = 6.4$ km, $\varepsilon_c = 0.032, 0.051, 0.080$ and 0.127 for $h_{\text{ref}} = 25, 50, 100$ and 200 m. This is also the content of the ‘dispersion time’ $\tau = 6h^2L/\lambda^3$ of Glimsdal et al. [R11], which for your pulses, with $\lambda = L_w$, equals 0.0157Π ; these authors find that the effect of dispersion is small for $\tau < 0.01$ and generally significant for $\tau > 0.1$, and in your table $\tau > 0.1$ only for (25, 4), (25, 8) and (50, 8), while $\tau < 0.01$ in 14 of the 23 cases. The criteria (i) and (ii) of page 3 cannot decide: all pulses of Table 1 have the same Ursell number $AL_w^2/h^3 = 16\pi^2/3$, and the hydrostatic equations are fully nonlinear and need no small amplitude. For a single long wave of positive polarity travelling over a constant depth and then up a plane beach, the same route (the Riemann wave for the propagation, the Carrier–Greenspan solution for the run-up) was followed, with finite-volume simulations, by Abdalazeez, Didenkulova and Dutykh [R12] in this journal; their set-up is that of your Figure 3.

8. Model error and numerical error are mixed in the first-order results. Before breaking, the hydrostatic equations conserve the maximum of the pulse and its width at every level: a simple wave carries each level of η at its own constant speed (Appendix III). Every loss shown by your first-order codes for $\Pi < 1.45$ is therefore numerical diffusion, and Figure 7a, where the case (25 m, 0.5 m), with $\Pi = 0.72$, loses 6.5 (BASEMENT), 9.7 (GeoClaw) and 8.8 percent (BoussClaw) in the measure of the figure, is a picture of the schemes and not of the models. I computed a converged solution of the hydrostatic equations (a fifth-order scheme, validated on the lake at rest, on the dam-break solution of Stoker and on a smooth simple wave, with observed orders 4.0 and 3.6), and a first-order scheme with cells of 2 m and Courant number 0.8. The latter loses 5.5, 2.9, 0.7 and 0.2 percent for $(h_{\text{ref}}, A) = (25, 0.5), (50, 2), (100, 4)$ and $(200, 8)$ m, where the exact loss is zero; BASEMENT loses 6.5, 3.5, 0.9 and 0.3 percent, and GeoClaw 9.7, 5.5, 1.3 and 0.3 percent. The estimate $\Delta A/A \approx -2D\kappa^2s/c$, with the numerical diffusion $D = (c\Delta x/2)(1 - \nu)$ of a first-order scheme, gives the right sizes, and I repeated the analysis of your time step: the statement of page 5 (line 130) that the HLLC solver is stable for a Courant number up to one, defined with the inscribed radius of the triangle, is correct (a von Neumann analysis on four shapes of triangles gives exactly one), and for the mesh of 3 m² triangles of BASEMENT at your Courant number 0.8 the diffusion is $D = 0.27c$ m, that of a one-dimensional cell of 1.1 m at Courant number 0.53. At second order GeoClaw loses 0.2, 0.1, 0.03 and 0 percent in the same four cases, that is 96 to 100 percent of its loss disappears.

Table A2 does not fit this picture. From R1 to R5 the cell size decreases by a factor 5.7 (the cell area by 32), and the value at 6.4 km changes by at most 1.1 percent of its value, whereas first-order theory predicts that R1 loses about 5.7 times more than R5; for R5 the theory gives -0.98 and -0.127 percent for $A = 4$ and 0.5 m, against -0.92 and -0.130 percent printed (in the elevation measure). The product of the number of cells and the cell area is not constant (2.7 to $3.9 \cdot 10^5$ m²), and I wonder whether the meshes were refined along the path of the wave. The sentence ‘not a convincing gain in accuracy by using a finer mesh’ (page 23) is therefore not a statement of convergence. A convergence study needs an exact or a converged reference and the error measured against it. Froude similarity gives a neat diagnostic: for the continuous models the relative change at (h, A, s) and at $(2h, 2A, 2s)$ is the same, while for a first-order scheme with fixed cells it is twice as large; your tables give 2.0 for GeoClaw (median of 24 pairs) and 1.8 for BASEMENT before breaking, 1.0 for both after breaking, and 1.7 to 2.1 for BoussClaw throughout.

After breaking the picture changes. For (25 m, 4 m), with $\Pi = 16$, the converged hydrostatic solution loses 55 percent, and BASEMENT and GeoClaw (first and second order) lose 53, 55 and 55 percent in Table R1: the codes are right where the model matters, and the loss is a property of the equations. In

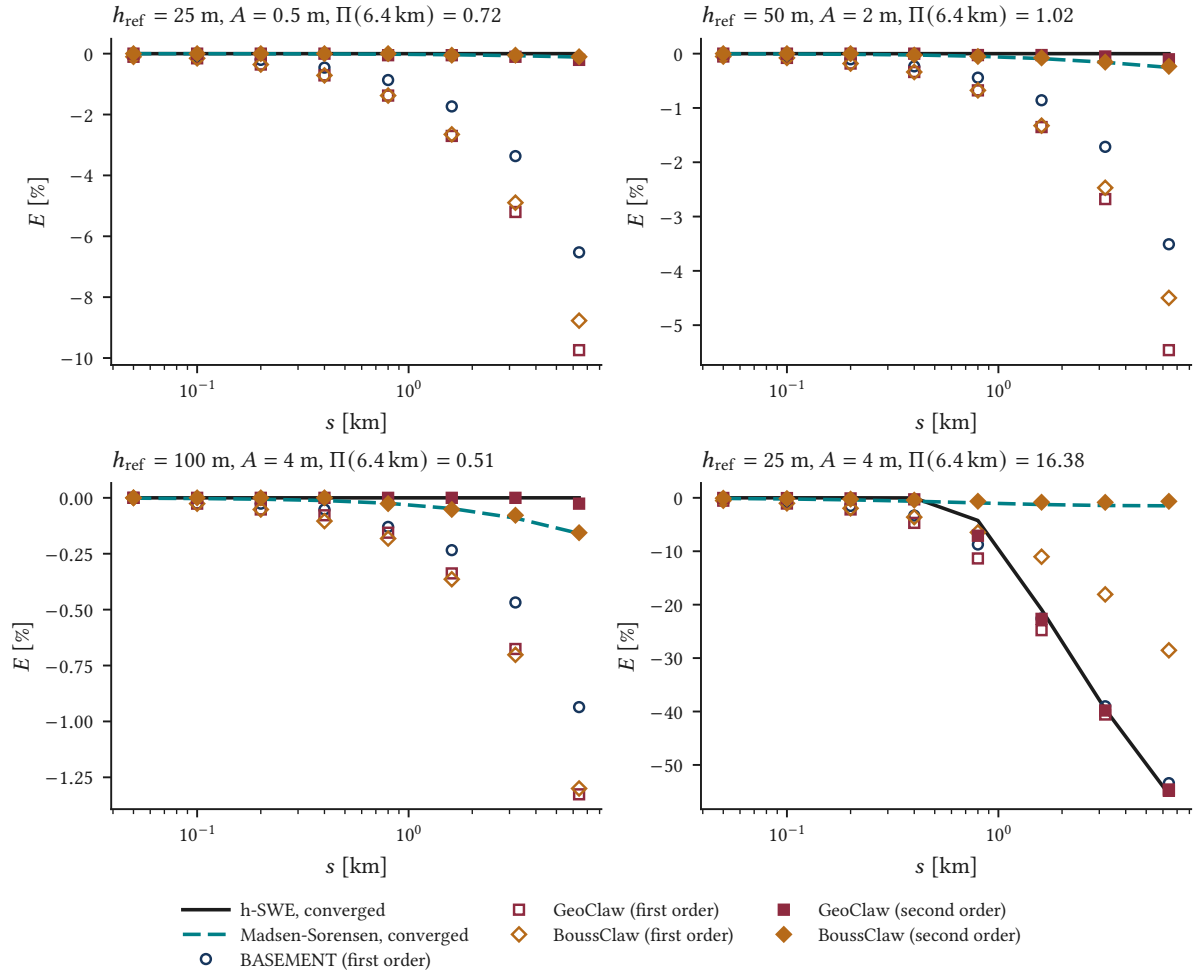


Figure R1. Amplitude change in the elevation measure against the distance s for four of your cases. The converged solutions of the hydrostatic equations (black) and of the Madsen–Sørensen system with $B = 1/15$ (dashed teal) are the references; open symbols are the results of your first-order runs, filled symbols those at second order. In the three cases with $\Pi = 0.72$, 1.02 and 0.51 the hydrostatic loss is zero and everything that the first-order codes lose is numerical; in the case with $\Pi = 16$ the loss of all hydrostatic results is the loss of the equations. The first-order BoussClaw results are far from the dispersive model, the second-order ones are on it.

your Figure 7c BASEMENT and GeoClaw lose 56 and 57 percent at first order. Figure R1 shows four cases against the converged hydrostatic solution and the converged dispersive solution of the next remark.

9. The dispersive solver: first order and second order. The Madsen–Sørensen model itself, which I solved by a spectral method in the form of Remark 1 (the linear phase error is $6 \cdot 10^{-8}$, and the solitary waves obey the speed relation of Appendix II to 10^{-8} and propagate unchanged to 10^{-9}), loses between 0.1 and 2.2 percent of the amplitude of the SGN pulse at 6.4 km in the six cases (25, 0.5), (50, 2), (100, 4), (200, 8), (25, 4) and (25, 8): the pulse reshapes towards the slightly wider Madsen–Sørensen soliton. BoussClaw at second order, in the Tables D, loses 0.03 to 1.9 percent, within one point of the model; at first order it loses 0.3 to 38 percent. The large losses of BoussClaw in your Figures 5 to 7 (about 30 percent at (25 m, 4 m), about 40 percent at (25 m, 8 m)) are therefore not dispersion: they are the numerical diffusion of its first-order finite-volume step. The sentences of page 13 (lines 287–291) saying that the dispersive solver has the ‘smallest errors’ and that BASEMENT is ‘the most accurate’ for the smallest depth and amplitude compare numerical diffusion and not models, and the same is true of the corresponding sentences of Sections 7.1 and 8. The conclusion that you

want, namely that the hydrostatic equations fail where Π exceeds about one while a dispersive model does not, survives; the ranking of the codes at first order does not. I suggest to present the results at second order, to add the converged hydrostatic and Madsen–Sørensen curves as references, to plot against Π , and to keep the first-order runs for what they are good for, a study of numerical diffusion. The ‘greater time expenditure’ of dispersive solvers (page 2, line 59) is never measured in your paper, whereas the cost of the elliptic step and the use of mesh refinement are what Berger and LeVeque [R13] address in the implicit dispersive solver now included in GeoClaw. I would measure the cost of the three codes on the same problem and the same hardware.

10. Orders of accuracy, and Tables B and D. The rows of BASEMENT in Tables D1 to D4 are identical to those of Tables C1 to C4 (all 14 pairs, amplitude and period), so that BASEMENT was never run at second order, and the sentence of page 14 (line 302) saying that both hydrostatic solvers are compared at second order is not correct; only GeoClaw is. In Tables D1 to D3 the column of ε is $A/200$ and not A/h_{ref} (twelve entries, copied from Table D4). In Appendix B the captions of Tables B1 and B2 speak of ‘different mesh resolutions’, but no column gives the resolution; the test matrix ($h_{\text{ref}} = 40, 80, 120, 160, 200$ m, s from 800 m to 6.4 km, $A = 0.1, 0.4, 1, 6$ m) occurs nowhere else in the paper; and 36 of the 51 entries present in both tables are identical between ‘BASEMENT – GeoClaw’ and ‘BASEMENT – BoussClaw’, including the whole block $h_{\text{ref}} = 160$ m, with the value 0.4121 occurring three times. Negative values cannot be the maximum over time of a difference of two records that start at rest. I suspect a copying error, and in any case a real convergence study should replace these tables.

11. The ‘period’ is not defined, and the values of BASEMENT jump. A solitary pulse has no period, and the formula for Δt_s is printed with Δt_{ref} in both numerator and denominator (pages 13, 25 and 30); the captions of the tables call the same quantity a ‘phase shift’. Which width is measured, the width at half height, the width at a threshold, the base? With the converged solutions I find that the hydrostatic equations conserve every width before breaking, that the Madsen–Sørensen model changes the width at half height by less than 2.6 percent for $\varepsilon \leq 0.04$ and by 3 to 4.5 percent for $\varepsilon = 0.16$ and 0.32, while the width at one percent of the maximum grows by 355 percent at $\varepsilon = 0.32$ because of the dispersive tail, and no standard width reproduces your GeoClaw column. In your tables BoussClaw broadens the pulse by 17 to 37 percent for $\varepsilon \leq 0.04$ at both orders, which has no counterpart in the model; and for the smallest amplitude BASEMENT shows single jumps of -16.4 , -23.7 and -4.5 points between two gauges (from 800 to 1600 m at 25 and 50 m, from 400 to 800 m at 100 m) while its amplitude does not change. If gauge 1 is, as I understand, at the front of the initial pulse, at $L_w/2$ ahead of its crest, its record starts at 0.75 to 1.7 percent of A instead of zero, which affects any measure of duration. I would ask for the definition, for the records themselves at a few gauges, and for the use of the width at half height.

12. Wave generation, gauge 1 and what is needed to rerun a test. In GeoClaw and BoussClaw the wave is an initial condition, in BASEMENT it is a boundary condition built from the GeoClaw record at ‘ $1.5L_w$ ’ (page 9, lines 211–214), and the text of page 8 (lines 183–186) says that the boundary condition is computed from (9) and (10). Which variables are imposed? With the hydrostatic equations a subcritical inflow accepts one condition. If the state of (9) and (10) is used as a ghost state, the boundary transmits the amplitude $(1 - \varepsilon/8)A$; if only the discharge is imposed, $(1 - \varepsilon/4)A$ (0.9906, 0.9675 and 0.9445 at $\varepsilon = 0.04, 0.16$ and 0.32); if only the level is imposed, A . The choice matters at the level of one to five percent. The left boundary condition, the width and the length of the channel, the side walls, the position of the initial crest, the time steps, the code versions and the use of mesh refinement in GeoClaw are not given, and a plane wave computed in two dimensions only adds cost and transverse noise. The word ‘first order’ for BoussClaw should be explained: I understand that it

is the finite-volume step for the hydrostatic part, the dispersive part being always of second order in space with a four-stage Runge–Kutta method in time.

THE RUN-UP AND OVERLAND-FLOW STUDY (SECTIONS 4.3.2 AND 6)

13. The set-up of the run-up tests cannot be reconstructed from the text. The run-up r is never defined. In Figures 9 and 10 it must be the vertical height above still water, since 13 of the 14 plotted values are below h_{ref} and cannot be heights above the bed. But the slopes start at $20w$, $22.5w$ and $23.5w$ and rise by $w = 0.25$ m, which puts the three crests at $25w = 6.25$ m, and for $h_{\text{ref}} = 0.2$ m each crest stands only 0.05 m above still water; every run-up of Figure 10 (0.065 to 0.23 m) is higher, so that the waves would overtop. Either the slopes are higher than w in the run-up tests, and then ‘the shore height is set to $w = 0.25$ m for all experiments’ (page 9, lines 227–228) is not true for them, or the waves overtop and r is not a run-up. Other details do not add up. The ‘propagation distance $S = 25h_{\text{ref}}$ m’ equals $20w$ only for $h_{\text{ref}} = 0.2$ m on the 1:5 slope; the domain $2h_{\text{ref}} + 2L_w$, which is 5.7 m for $h_{\text{ref}} = 0.2$ m and $\varepsilon = 0.3$, is shorter than the 6.25 m to the crest; and in the overland-flow tests the slopes would end at $13w$ and $7.4w$ ($S = 11.5w$, $x_{\text{tr}} = 1.5w$ and $S = 2.4w$, $x_{\text{tr}} = 5w$) instead of $25w$. I assume that these are misprints, and I would be grateful for a table giving, for every test, the geometry, the height of the slope, h_{ref} , the actual amplitude, k_b , the size of the cells and the wetting threshold.

The amplitudes of page 9 ($A = 8, 18, 19, 27, 38, 58$ mm) give $\varepsilon = 0.08, 0.18, 0.27$ for $h_{\text{ref}} = 0.1$ m and 0.095, 0.19, 0.29 for 0.2 m, and they are plotted as 0.1, 0.2, 0.3; since the run-up of non-breaking waves grows like $\varepsilon^{5/4}$, this changes run-up values by 4 to 6 percent in a comparison with theory (Remark 14). The instrument is a camera at 5 Hz on page 9 and an ultrasonic device on page 17 (lines 218–220 and 346); I suppose that one measured the run-up and the other the flow depth, which should be said, with the accuracy of each. If the maxima of the run-up were read from photographs at 5 Hz, a swash tip that decelerates as a ballistic particle, at $g \sin \beta$, can be missed by up to $g \sin^2 \beta \Delta t^2 / 8 = 2, 7$ and 15 mm on the three slopes, that is up to 10 percent of the run-up on the steepest one.

14. In the laboratory the run-up falls with the steepness of the slope, in BASEMENT it rises, and the text says the opposite. Figure 10 is, in my view, the heart of the validation, and I read it slope by slope. For $h_{\text{ref}} = 0.2$ m and $\varepsilon = 0.3$ the laboratory run-up on the slopes 1:5, 1:2.5 and 1:1.5 is 0.197, 0.180 and 0.155 m: it falls when the slope steepens, and so it does at $\varepsilon = 0.2$ (0.132, 0.120 and 0.100 m). The theory of the run-up of a non-breaking solitary wave says the same (0.288, 0.209 and 0.169 m at $\varepsilon = 0.3$; Appendix V). BASEMENT gives 0.167, 0.190 and 0.228 m: it rises. Its error $r_{\text{BM}}/r_{\text{lab}} - 1$ is +5, –6 and –15 percent on the slope 1:5 (for $\varepsilon = 0.1, 0.2$ and 0.3), +33 and +6 percent on 1:2.5 (for $\varepsilon = 0.2$ and 0.3), and +57 and +47 percent on 1:1.5 (Table R4).

The text reads this differently. Page 15 (lines 326–332) finds ‘no clear trend’ and calls the slope 1:5, the gentlest of the three, ‘the largest slope inclination’; with this vocabulary the sentence that BASEMENT overestimates for the ‘smaller slope inclinations’ and underestimates for the largest one is correct, but it reads backwards. Page 19 (lines 385–387) then calls 34 degrees ‘the largest slope inclination’ and says that BASEMENT underestimates there, whereas Figure 10 has +47 percent; and page 20 (lines 399–405) uses ‘smaller slope inclinations’ for the gentle slopes of lakes and concludes that BASEMENT overestimates there, which ‘leads to a conservative result in risk assessment’. On the slope of 11 degrees, the only one of the three below the 20 degrees that you give for lakes, BASEMENT underestimates: by 15 percent in Figure 10 and by 18 to 23 percent in Figure 9 at $\varepsilon = 0.3$. The ‘conservative result’ of the discussion and of the conclusions is therefore true on the steep slopes

and false on the gentle ones. Could you please let the words ‘large’ and ‘small’ refer to the angle only, and rewrite these sentences after Figure 10?

15. The rise with the steepness is that of the hydrostatic equations with friction, and not a defect of the code. Is it a defect of BASEMENT, or a property of the hydrostatic equations? I tried to refute my own reading by solving the hydrostatic equations with a solver of my own, in second-order finite volumes with the hydrostatic reconstruction of Audusse et al. [R14], validated on exact solutions in Appendix V, with the friction law of page 9 and $k_b = 0.5$ mm, the central value of Figure 9. The set-up cannot be reconstructed from the text (Remark 13), and I took the slopes high enough not to be overtopped. I therefore tried eight readings of it: the crest of the wave at 0.5 , 0.75 , 1 or $1.5L_w$ from the left boundary, at three widths before the toe, entering through the boundary $x = 0$ (with the depth and the velocity, or with the depth alone), and with the geometry of your Figure 3. The answer, in Figure R2 and Table R4, is that the rise belongs to the model. With the crest at $0.5L_w$ or $0.75L_w$ my values are 0.89 to 1.11 times yours, and the effect of k_b in Figure 9 (from 0.2 to 1 mm) is the same to within 1.5 points: -6.2 percent against your -6.2 at $\varepsilon = 0.3$, and -6.0 against -7.1 at $\varepsilon = 0.2$. In seven of the eight readings the run-up with friction rises with the steepness at $\varepsilon = 0.3$, as yours does, and on the slope $1:5$ it is below the laboratory value in all the readings that I could run there, by 2 to 25 percent (0.148 to 0.193 m against 0.197 m).

The reason is the one of Remark 7, seen in a run-up. The hydrostatic pulse of $h_{\text{ref}} = 0.2$ m forms its first jump 8.0 , 3.5 and 2.3 m after the position of its crest for $\varepsilon = 0.1$, 0.2 and 0.3 (Appendix III), and in the readings above it travels 1.4 to 7.5 m before the toe: at $\varepsilon = 0.3$ it arrives at the toe as a bore in most of them, at $\varepsilon = 0.2$ in about half of them, and at $\varepsilon = 0.1$ in none. In the laboratory, dispersion balances nonlinearity and a solitary wave keeps its shape; in the Korteweg–de Vries model it is an exact travelling wave (Appendix III). The steeper front of the hydrostatic pulse raises the run-up by itself, as Stefanakis, Xu, Dutykh and Dias [R15] find for transient long waves on a beach: steeper waves reach higher run-up. Released three widths before the toe of the slope $1:1.5$, that is 1.4 m or seven depths, a distance on which the pulse steepens but does not yet break, the pulse with $\varepsilon = 0.3$ climbs without friction to about 0.22 m, 31 percent above the 0.169 m of the solitary wave, and the exact linear response of the slope to the steepened incident record gives 0.225 m (Appendix V). The dependence of the run-up on the steepness of the front of the incoming wave, for the same arrangement of a constant depth followed by a plane beach, is the subject of Abdalazeez, Didenkulova and Dutykh [R12], who propose a formula for the run-up of a wave with a steep front.

Friction then does the rest. Without it the hydrostatic run-up falls with the steepness, as in the laboratory, but by 5 to 6 percent only from $1:5$ to $1:1.5$ when the pulse arrives as a bore, against 21 percent in the laboratory, and in every reading with the toes of page 10 it exceeds the laboratory value, by 23 to 79 percent at $\varepsilon = 0.2$ and 0.3 (these values are lower bounds, since behind a bore they still grow when the cells are refined). The law of page 9 with $k_b = 0.5$ mm removes a third of the run-up on the slope $1:5$, a quarter on $1:2.5$ and a sixth on $1:1.5$ (crest at $0.5L_w$, $\varepsilon = 0.3$), and this is what reverses the trend. The shore is smooth PVC (Remark 19), and a logarithmic law is not meant for layers of a few millimetres; I read this friction as a calibration that absorbs the error of the incident bore, and not as the physics of the laboratory. The agreement within 6 percent on the slope $1:2.5$ at $\varepsilon = 0.3$ is then the sum of two errors of opposite signs. If one takes the amplitudes actually measured (19 , 38 and 58 mm) instead of the nominal ones, my run-ups are lower by 2 to 7 percent, and none of this changes.

I draw three conclusions, with the caution that my solver is not BASEMENT, a two-dimensional code on triangles, and that I do not know your reading of the set-up, so that about 12 percent is all that I claim. First, BASEMENT is not at fault: its numbers are what the hydrostatic equations

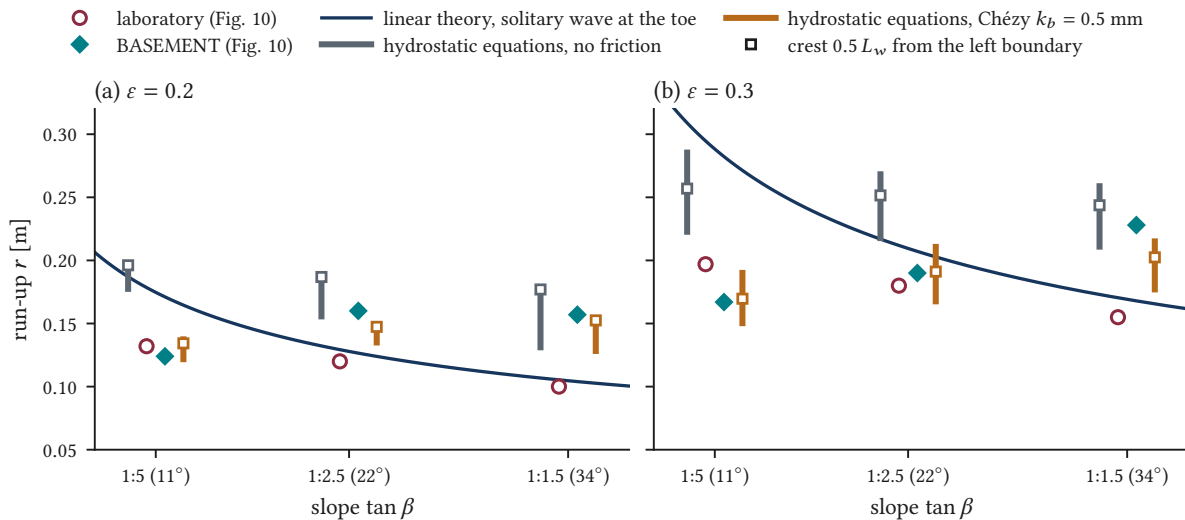


Figure R2. Run-up on the three slopes for $\varepsilon = 0.2$ and 0.3 , $h_{\text{ref}} = 0.2$ m. Circles: the laboratory; diamonds: BASEMENT, both read in your Figure 10. Navy curve: the linear theory for a solitary wave that arrives at the toe, which is the exact run-up of a wave that does not break (Appendix V). Grey and ochre bars: the range of the run-up of the hydrostatic equations, solved independently, without friction and with the friction of page 9 ($k_b = 0.5$ mm), over eight readings of your set-up; the square marks the reading with the crest $0.5L_w$ from the left boundary. With friction the hydrostatic run-up rises with the steepness at $\varepsilon = 0.3$, like that of BASEMENT, and it lies below the laboratory value on the slope 1:5.

predict. Second, for this very reason Figure 10 does not validate the hydrostatic equations for run-up; it shows their limits, a bore that the laboratory wave does not have, compensated by a friction whose physical meaning is doubtful. Third, ‘conservative’ is not supported for lake slopes. Could you please say which reading of the set-up and which k_b Figure 10 uses, show a frictionless run beside each run with friction, and, above all, run BoussClaw on the same cases (Remark 18)? This would tell whether a dispersive model, which keeps the solitary wave intact, brings back the trend of the laboratory. Before any comparison with experiments, the shoreline treatment of BASEMENT can also be verified against exact solutions of the nonlinear equations on a plane beach: the solver of Bueler-Faudree et al. [R16] compares a finite-volume solution with the analytical solution of the one-dimensional nonlinear shallow-water equations on an inclined plane beach, for solitary waves and for N-waves; a shoreline algorithm built on local high-order expansions and compared with analytical and experimental results is described by Khakimzyanov, Shokina, Dutykh and Mitsotakis [R17]; and Appendix V gives the exact reference for the non-breaking solitary wave.

16. The same experiments, computed twice, give two results. Figure 12f (slope 1:5, $h_{\text{ref}} = 0.2$ m) shows at the transition point 26.2 and 35.6 mm for $\varepsilon = 0.2$ and 0.3 , whereas Figure 11, which has the same laboratory data (they agree to 0.05 mm), shows 21.2 to 21.6 and 28.6 to 28.8 mm for every k_b from 0 to 2 mm. The gaps, 4.6 and 6.8 mm, are 13 to 30 times larger than the whole effect of k_b . The run-up at $\varepsilon = 0.3$ on the 1:5 slope is 0.167 m in Figure 10 and 0.151 to 0.161 m in Figure 9, and at $\varepsilon = 0.1$ the value of Figure 10, 0.068 m, lies below every run of Figure 9, so that no single k_b explains both figures. The value of k_b is not stated for Figures 10 and 12. Please say which runs are shown (mesh, k_b , wetting threshold) and use the same ones throughout.

17. There is no error measure, and ‘within the measurement error’ is not what the figures show. In Section 6.1 you quote differences of 21.2 to 23.2 percent at $\varepsilon = 0.3$ for $h_{\text{ref}} = 0.2$ m and of 15.7 to 21.6 percent for 0.1 m, and call the agreement good. Values read from Figure 9 are 18.3, 21.1 and 23.4 percent for $k_b = 0.2, 0.5, 1$ mm at 0.2 m (the first is outside your range) and 15.9, 19.3 and 21.6

percent at 0.1 m. A run-up difference of 20 percent is not what one calls good agreement for hazard assessment, and its sign matters (Remark 14). In Section 6.2 you write that all numerical results lie within the measurement error of 3 mm. But 14 of the 60 gauge values of Figure 11 and 11 of the 90 of Figure 12 differ from the laboratory by more than 3 mm: in Figure 11, for $k_b = 0$ and $\varepsilon = 0.3$, five gauges of six; in Figure 12 the largest difference is -7.0 mm at 0.5 m for the 1:1.5 slope, $h_{\text{ref}} = 0.24$ m and $\varepsilon = 0.3$ (laboratory 33.1 mm, BASEMENT 26.1 mm). The root-mean-square differences are 1.2 to 3.8 mm in Figure 11, against flow depths of 5 to 35 mm; relative errors reach -51 and $+65$ percent where the layer is thin, and in Figure 12e BASEMENT has 1.8 to 2.7 mm where no water arrived in the experiment (Figure R3). I would ask for a table of the root-mean-square and the maximum errors of every series, and for the sentences of Sections 6.2, 7.2.2 and 8 ('very good agreement', 'does not show any shortcomings') to follow it. A companion to the comparison with flow depths of 5 to 30 mm is the examination of the pertinence of the nonlinear shallow-water equations for the flooding of a dry bed, by comparison with two-fluid Navier–Stokes simulations of the dam-break problem, by Dutykh and Mitsotakis [R18].

18. The dispersive solver is never used for run-up and overland flow. Your aim (page 3, lines 62–66) is to compare the hydrostatic and the dispersive approaches 'in particular, run-up', and the abstract speaks of benchmark tests and experimental data for both. But BoussClaw, and GeoClaw as well, are not used in Section 6: the run-up and the overland flow are computed by BASEMENT alone. The statement of Section 7.2.2 (lines 419–424) that hydrostatic solvers capture inundation well, and the conclusion that faster hydrostatic solvers 'can produce results as accurate as those obtained using more computationally expensive nh-SWE solvers' for run-up, are statements about BASEMENT against the laboratory; nothing is known about the dispersive solver there. BoussClaw can compute run-up: Kim et al. (2017) do so, switching the dispersive terms off in very shallow water, and report that the hydrostatic model gives premature breaking and a too high run-up for a non-breaking solitary wave on a 10 degree slope. The instabilities of Boussinesq models on slopes of 10 to 50 degrees are described by Løvholt, Lynett and Pedersen [R19], which is a further reason why such a test would be

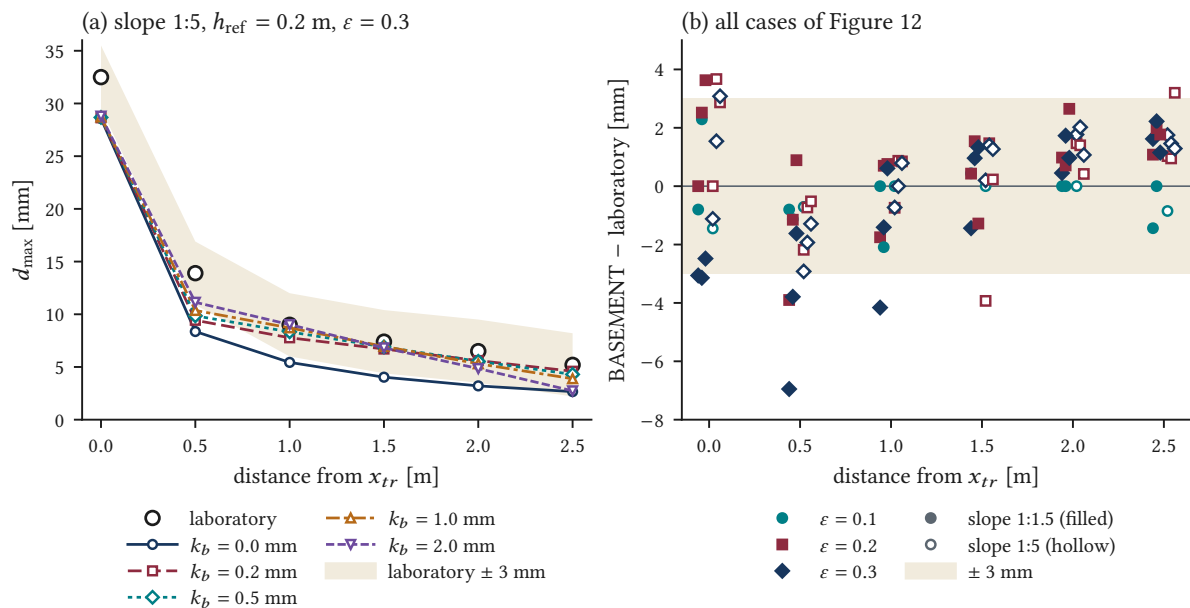


Figure R3. Overland flow, BASEMENT against the laboratory. (a) Figure 11, right panel (slope 1:5, as the laboratory values of Figure 12f show; $h_{\text{ref}} = 0.2$ m, $\varepsilon = 0.3$): the laboratory depths with the band of ± 3 mm that you quote, and the five runs of BASEMENT. (b) Difference BASEMENT minus laboratory for all cases of Figure 12, by gauge: eleven of 90 values leave the band, the largest by -7 mm.

informative. Finite-volume schemes for Boussinesq-type systems, with the run-up of breaking and non-breaking long waves, are described and tested by Dutykh, Katsaounis and Mitsotakis [R20].

And the direction of the dispersive correction to the run-up is not universal. For a solitary wave on flat slopes, Gusev, Khakimzyanov, Chubarov and Dutykh [R21] find that the non-dispersive model overestimates the maximum run-up and the amplitude of the reflected waves by 10 to 20 percent. For an underwater landslide in a closed basin, the Boussinesq system predicts a larger run-up than the shallow-water theory in the example of Dutykh and Kalisch [R22], and the finite basin has a significant influence on the run-up. In experiments in a wave tank with a plane beach, the maximum run-up of single pulses is not sensitive to dispersion, while dispersion matters for signals with higher frequency components during their propagation, as Torsvik et al. [R23] show. The same comparison of the nonlinear shallow-water equations with a Boussinesq system of Peregrine type, for a plane beach joined to a zone of constant depth, which is also the arrangement of your tests, and for regular, bichromatic and modulated wave trains, is made against laboratory data by Abdalazeez, Didenkulova, Dutykh and Denissenko [R24]. So the ‘conservative result’ that you infer for BASEMENT in Sections 7.2.1 and 8 follows neither from your figures (Remark 14) nor from the literature. Running BoussClaw and GeoClaw on the three slopes, at least for the cases of Figures 9 and 10, is the test that is missing (Remark 15).

19. Roughness, smooth walls, and scale. The laboratory shore is made of PVC, which is hydraulically smooth: for a roughness of 0.003 mm one finds $k_s u_* / \nu = 0.05$ to 0.15 in the flows of the experiments, far below the value 3.3 above which the rough-wall law of Keulegan [R4] applies. With his smooth-wall law, the friction coefficient of the overland layers of the experiments (depths 5 to 30 mm, Froude numbers 1 to 2) is $c_f = 14$ to 22, the value of the rough-wall law with k_b between 0.06 and 0.2 mm; the run with $k_b = 0$ is not smooth but frictionless (Remark 2). This is also why ‘as was demonstrated in the laboratory experiments, ... the bed roughness does not significantly affect the run-up’ (page 21, lines 439–440) cannot be a laboratory result: as far as I can tell from the papers of Fuchs and Hager [R25, R26], no roughness was varied in the experiments, the shores being smooth PVC, so that the statement is a numerical finding for one set of conditions. Nor can it be transferred to a lake: the relative roughness k_b/d of the laboratory overland layers is between 0.007 and 0.4, and at lake scale a stony shore has k_b of centimetres to decimetres under flow depths of decimetres to metres. Moreover, at a depth of 20 cm the run-up on a slope is affected by laminar or transitional boundary layers and by capillarity, which make it markedly smaller than the prediction of inviscid theory, as shown by Pedersen et al. [R27]; a logarithmic law in k_b is not the right model in that regime, and in your laboratory data the run-up divided by h_{ref} is 11 to 20 percent smaller at $h_{\text{ref}} = 0.1$ m than at 0.2 m, which is a scale effect inside your validation data.

The geometric scale factor between the laboratory and the lakes is 100 to 2000. The Reynolds number based on the depth is 10^5 to $4 \cdot 10^5$ in the laboratory against $4 \cdot 10^8$ to $9 \cdot 10^9$ in the lakes, and the Weber number is 10^3 to $8 \cdot 10^3$ against $8 \cdot 10^7$ to $5 \cdot 10^9$. The paper uses laboratory data to ‘validate the applicability at lake scale’ without a scaling argument. Fuchs and Hager [R25] studied the scale effects of exactly these experiments, and Heller [R28] gives criteria to limit them. The laboratory data validate the shore processes at the laboratory scale; they cannot validate the propagation at lake scale, for which the paper has no data, and the comparison with the sedimentary evidence of the 1601 event on Lake Lucerne (Nigg et al., 2021), which you cite, would be a natural test at lake scale.

THE CLAIMS AND THE LITERATURE (SECTIONS 1, 7 AND 8)

20. A straight flat channel and a solitary wave are not a lake. The title and the conclusions speak of lakes, but the tests are one-dimensional: a flat bed, a plane wave that starts as a formed soliton, and no source. Several things that decide the answer in a lake are outside the study. The first is shoaling. By Green's law, valid for a gently varying depth, the local ε grows like $h^{-5/4}$, so that a wave of 4 m at 100 m depth ($\varepsilon = 0.04$) has $\varepsilon = 0.23$ at 25 m, and the ε that matters for the run-up is the one near the shore: extrapolated to 5 m depth, 20 of your 23 cases would have $\varepsilon > 0.08$ and 10 of them would be breaking. The statement that the two methods 'can be used interchangeably' should refer to the depth where the waves arrive and not to the depth where they start. The second is geometry: the radial spreading of a wave generated by a delta or a slope failure, and the difference between channel and basin decay laws of landslide tsunamis, which Heller and Spinneken [R29] relate by a transformation from two to three dimensions; a plane wave over-estimates the amplitude that a basin receives. The third is reflection: in a closed basin the relevant distance for Π is the length of the whole path of the wave, which can be several times the length of the lake, as Couston et al. and Svennevig et al., whom you cite, show for the persistence of waves in enclosed water. The fourth is the source: for landslide tsunamis it is generation and the near field that decide, and a solitary wave arriving after 6 km is not a landslide tsunami (see Remark 22). Beizel, Chubarov, Dutykh, Khakimzyanov and Shokina [R30] couple the motion of a landslide with the nonlinear shallow-water equations in a bounded reservoir, and study numerically the influence of the parameters of the motion on the maximal splash. The fifth is steep bathymetry: on slopes of 20 to 60 degrees the hydrostatic assumption is questionable by itself, independently of dispersion, and a hyperbolic non-hydrostatic extension of the shallow-water equations for significantly varying seabeds, with a finite-volume discretisation, is given by Dutykh and Clamond [R31].

21. What the evidence supports. *Tra il dire e il fare c'è di mezzo il mare*¹. I went through every sentence of the abstract, of the aims, and of Sections 7 and 8, and looked for the result that supports it; many are supported, and here are those that I could not support, so that you can adapt the text. 'Faster' and 'more expensive' (abstract, pages 2 and 21): no run time of GeoClaw or BoussClaw is given, and Table A1 with Figure A1b compare BASEMENT on a graphics processor with BASEMENT on one core of a processor that has 16, so that dividing by 16 turns the speed-ups 47, 26 and 19 into 2.9, 1.6 and 1.2 at least. 'In particular, run-up' (page 3): the dispersive solver is never run on run-up (Remark 18). 'In most lake settings the two solving methods can be used interchangeably' (page 21): one flat channel and one wave shape (Remark 20), and at $h_{\text{ref}} = 25$ m the first-order codes differ by 15 to 30 points when $\varepsilon \geq 0.08$ (Table R1). 'Attention must be paid when the propagation distance is greater than 6 km': 6.4 km is the end of your range and not a threshold; BASEMENT loses 24 percent already at 1.6 km for (25 m, 4 m) in your Figure 7c, so the sentence of page 12 (lines 274–276) that the deviations occur 'solely at gauge 9' is not correct. 'A large ratio of wave amplitude to still water depth (e.g. $\varepsilon \geq 0.08$)': the case (25 m, 1 m), with $\varepsilon = 0.04$, loses 15 percent at first order and 5 percent exactly, while (100 m, 8 m), with $\varepsilon = 0.08$, loses 2.6 percent (Remark 7). 'The numerical experiments lie within the measurement error' (Remark 17). 'the agreement between experimental and numerical results is very good', 'BASEMENT does not show any shortcomings' and 'a conservative result' (Remark 14, Remark 15 and Remark 17). And 'a fast, accurate solution in a lake-scale setting' is not demonstrated by a flat channel and a laboratory flume.

I would restrict the conclusions to what is shown, which is already a good deal: for the propagation of a solitary wave over a flat bed, the hydrostatic equations are adequate as long as Π is below about one,

¹Italian saying: between saying and doing lies the sea.

and below this value the numerical diffusion of first-order schemes is a larger source of error than the equations; the dispersive model at second order is accurate throughout; for run-up, the comparison with the laboratory is what [Remark 14](#) describes.

22. Literature and novelty. The paper would be better placed with respect to the literature in four points. First, the introduction says that few studies compare hydrostatic and dispersive solvers for lake tsunamis and quotes St-Germain et al. (2014), which deals with wind waves overtopping a coastal dike. Comparisons exist, and several reach conclusions different from yours for landslide tsunamis. Gylfadóttir et al. [R32] show, for the rockslide tsunami of Lake Askja in 2014, that frequency dispersion is needed to explain the radiation pattern of the waves and that the shallow-water model gives a different run-up pattern. Harbitz et al. (2014), whom you cite, found that hydrostatic models overestimate the leading wave. In the validation and inter-comparison of nine models in 21 configurations by Kirby et al. [R33], the inclusion of frequency dispersion is found to be critical, and a model with at least a leading-order representation of dispersion is recommended for landslide tsunamis whenever possible. Zafar, Dutykh, Sabatier, Banjan and Kim [R34] compare GeoClaw and BoussClaw for a landslide tsunami of 767,000 m³ in Lake Aiguebelette (5.45 km², up to 70 m deep), in which the wave crosses the lake in about two minutes. They find waveforms that are almost the same, with a nonlinearity of 0.077, a wavelength of 1.5 km, a dispersion parameter of $7 \cdot 10^{-4}$ and a Stokes–Ursell number of about 107, and they conclude that the dispersive effects may be negligible in a relatively small lake. This is what [Remark 7](#) gives: with these figures and a path of 2.4 km the number Π is of order one, between 0.3 and 1.3 according to the amplitude and the depth that one takes. The study is close to yours in setting and it should be discussed; it also shows what your flat channel leaves out, a source of given volume which decides the amplitude. The sentence on Løvholt et al. (2015) (‘a rigorous comparison of different solvers in terms of results and computational characteristics’, with COULWAVE and MOST) does not match what I read: linear shallow-water, standard and higher-order dispersive solvers against a full-potential model, without timings. And the criterion that unifies your two limits is the dispersion time of Glimsdal et al. [R11], which was published in this journal and is not cited. What is new in your paper is a systematic sweep of depth, amplitude and distance at lake-scale values with two hydrostatic codes, one of them a river code, against a dispersive one, and the validation of BASEMENT against the run-up and overland-flow data of the VAW; what is not new is that dispersion matters at large ε and long distance; and what is not supported is that generation, the near field, radial spreading and reflections can be ignored.

Second, Fuchs (2013) is a thesis, and its peer-reviewed counterparts, which give the definition of the run-up, the geometry and the instruments that the manuscript lacks, are Fuchs and Hager [R26] on the transformation of a solitary wave into overland flow, Fuchs and Hager [R25] on scale effects, and Fuchs, Heller and Hager [R35] on a run-over benchmark; the VAW has more, for example the compilation of 359 run-ups on slopes of 10 to 90 degrees of Evers and Boes [R36]. Third, the standard benchmarks for the validation of tsunami codes, with analytical solutions for the run-up of solitary waves on plane beaches, are collected by Synolakis et al. [R37]; Synolakis [R38] gives the run-up law and the breaking criterion, Li and Raichlen [R39] treat breaking solitary waves and Zelt and Raichlen [R40] the overland flow. A finite-volume code on unstructured triangular meshes with a careful treatment of the wet–dry transition, validated with analytical solutions and with experimental data, is described by Dutykh, Poncet and Dias [R41]; it shows what a validation of the kind that your Section 6 lacks looks like. Fourth, the reference list has defects, which I collect in the smaller remarks; codes are named without a reference (SWASH, COULWAVE, MOST, GloBouss, DpWaves, MassMov2D, DualSPHysics), BASEMENT is cited through its manual only, whereas Vanzo et al. [R42] is the paper, and Clawpack as ‘(Team)’.

23. Availability, versions and competing interests. The statement on code availability mentions BASEMENT only. The versions of GeoClaw and BoussClaw, the input files of all runs, the post-processing and the data behind the figures should be made available, as the journal asks. The sentence ‘The authors declare that they have no conflict of interest’ deserves a second look: the institute of the authors develops and distributes BASEMENT, the code that the paper concludes to be fast and accurate for hazard assessment. This is not a conflict in itself, but it is a competing interest that readers will want to see declared, and the paper would gain from having some of the tests repeated independently, for example with the exact solutions of [Appendix V](#). Finally, a table of all runs, with the sizes of the cells, the time steps and the computing times, would make the study reproducible.

SMALLER REMARKS, PAGE BY PAGE

The devil hides in the details, so I list them, briefly and without irony, page by page.

24. Pages 1 to 5. Page 1, line 20: ‘loose’ should be ‘lose’. Page 1, lines 22–24: the abstract of Miller (1960) reports water surging to an altitude of 1,740 feet (530 m) on the spur opposite the impact; the values ‘365 m inland and 33 m above lake level’ do not appear there, and Lituya Bay is a tidal inlet and not a lake; could you say where these numbers come from? Page 2, line 58: the key ‘(?Kazolea and Ricchiuto, 2018)’ is unresolved, so that a citation is lost. Page 2, lines 56–60: the statement that hybrid coupling ‘is associated with greater time expenditure’ needs a reference or a measurement. Page 3, line 85: ‘constituting from the sum’ should be ‘consisting of the sum’; equation (3) lacks its final comma; line 90: S_h is a discharge per unit area ([Remark 2](#)). Page 4, line 98: Boussinesq (1872) and Peregrine (1967) are named in the text but not cited as such; line 105: μ in $O(\mu^4)$ is not defined. Page 5, line 130: your statement that the HLLC solver is stable for a Courant number up to one, with your definition, is correct; the time steps actually used should be given. Page 5, lines 140 and 145: ‘Geoclaw’ should be ‘GeoClaw’, and ‘equation 7’ should be ‘equations (4) to (7)’; the entry ‘Team’ of the list is printed twice as a citation (see the table below).

25. Pages 6 to 10. Page 6, lines 160–162: ‘href = between 25 m and 200 m’; the ranges ‘ ε between 0.0025 and 0.32’ and ‘ L_w/h_{ref} between 12.8 and 114.7’ differ from Table 1, which has 0.001 to 0.32 and 12.8 to 229.4. Page 7, Table 1: the entry for (25 m, 0.1 m) should read $\varepsilon = 0.004$, and the entry for (50 m, 2 m) should read 36.3 and not 26.3; L_w is never defined, and the entries are $2\pi/\kappa_0$ with $\kappa_0^2 = 3A/(4h^3)$, which differs from the κ of (9) by the factor $\sqrt{1+\varepsilon}$, 15 percent at $\varepsilon = 0.32$. Page 7, line 172: ‘ x/h_{ref} ’ is typeset as ‘x/hre f’; line 178: ‘sech2(*)’ should be ‘sech²’; ‘a N-wave’ should be ‘an N-wave’. Page 8, lines 194–195 and 242: ‘ Z_f ’ and ‘ z_f ’ denote the same quantity. Page 9, line 225: ‘the opposite side 3’ should be ‘the opposite side (Figure 3)’, line 226: ‘25href m’, line 227: ‘oftan’, line 229: ‘href = 0.2 m and 0.1 m.’, line 231: ‘s.t.’; the ‘roughness coefficients’ of lines 216–217 are roughness heights, and they concern the glass and the steel of the channel, while the shores are made of PVC. Page 10, line 235: ‘the run-up height 4’ should refer to Figure 4, line 237: ‘The slope starts ends at transition point’, line 242: ‘freebord’, line 245: ‘href = 0.2’ has no unit and ‘0,3’ has a decimal comma.

26. Pages 11 to 17. Page 11, lines 260–264: the text gives the depths of panels a) and b) of Figure 5 the other way round (the figure has 25 m in a) and 100 m in b)), and ‘again A = 0.5 m and but the largest’ has one word too many. Page 12, line 276: ‘Both h-SWE solver’; line 279: ‘asses’. Page 13: the definition of Δt_s is printed with Δt_{ref} in numerator and denominator (also pages 25 and 30), and the caption of Figure 7 reads ‘BASEMENT, (BM)’. Page 15, line 313: the text says that Figure 9 plots ε against r , while the axes are the other way round; line 326: ‘no clear trend’ is not what Figure 10 shows ([Remark 14](#)). Pages 15 to 17: the captions of Figures 9 to 12 do not give the slope, h_{ref} or k_b ,

and those of Figures 11 and 12 say that the gauges are ‘to the left’ of x_{tr} while the axis runs away from it. Page 17, line 355: ‘a smaller href a slope inclination leads to smaller flow depths’ is garbled.

27. Pages 19 to 23. Page 19, line 366: ‘not necessarily necessary’; lines 367–368: ‘(e.g. find lake sizes)’ is a note to yourselves left in the text, and the distances of 5 to 10 km that you give for Lake Lucerne (Section 4.1) are around the limit of 6 km that you find; line 370: Sections 7.2.1 and 7.2.2 do not discuss wave breaking. Page 20, lines 398–399: Strupler et al. (2020b) give 10 to 25 degrees as the range of the slopes on which subaqueous landslides tend to start, and I did not find that they ‘classified all slopes with an angle $< 20^\circ$ ’ as unstable; lines 420–422: Lynett (2022) is a text of 2009 reprinted in 2022; could you give the passage? Page 22, line 453: ‘Ge-Force’ should be ‘GeForce’; the launch price of a graphics card is not a parameter of the study; in Figure A1b the axis is ‘ $\times 10^{-5}$ ’ and should be ‘ $\times 10^5$ ’. Page 23: the caption of Table A2 ends with ‘href = 100m.’ on a separate line; ‘Th tables’ on page 23, line 466. Page 34: ‘founded by’ should be ‘funded by’.

28. Appendices B, C and D. The label ‘BM3’ is not explained. In Table C2 the column of 6400 m is empty for BoussClaw with $A = 1$ m; in Table C4 the first two entries of the BASEMENT row for $A = 8$ m are merged under the column of 100 m; in Table D1 the BASEMENT row for $A = 8$ m has ‘–0.3 –’ and ‘0.965’ where I expect ‘–0.3’ and ‘–0.965’; in Table D2 the entry of 400 m is missing in the BoussClaw row for $A = 8$ m and the following entries are shifted; Table C3 prints –0.036 where Table A2 gives –0.0352 for the same case; and in Table D4 the BoussClaw row for $A = 8$ m is not monotone (–0.001, –0.003, –0.001 at 1600, 3200 and 6400 m). The other points on these tables are in Remark 6, Remark 10 and Remark 11.

29. The list of references. The list has the defects of the following table; in addition 26 entries lack a DOI that exists (Couston et al., Didenkulova and Pelinovsky, Fathianpour et al., Gariano and Guzzetti, Hammack, Harbitz et al. 2014, Heinrich et al., Jacquemart et al., Kim et al. 2001, Kremer et al., Liang et al., Madsen and Schäffer 2010, McColl, Miller, Nigg et al., Patton et al., Rafliana et al., Ruffini et al., Schnellmann et al., Segur, Strupler et al. 2020b, Svennevig et al. 2025, Tadepalli and Synolakis, Wang et al., Yeh et al., Ambrosi), and the particles of some names are misplaced (Svennevig et al.: Van Noten, De Plaen, De Pinho Dias, Kjær Dideriksen). The codes named in the text without a reference are SWASH, COULWAVE, MOST, GloBouss, DpWaves, MassMov2D and DualSPHysics; DualSPHysics, for example, is Crespo et al., Computer Physics Communications 187 (2015) 204–216.

Entry	Correction
Madsen et al. (2008a) and (2008b)	One paper listed twice: J. Geophys. Res. Oceans 113 (C12) doi:10.1029/2008JC004932.
Kazolea and Ricchiuto (2018)	The citation key is unresolved on page 2 (‘?’).
St-Germain et al. (2014)	Garbled. Coastal Engineering Proceedings 34, structures.10 doi:10.9753/icce.v34.structures.10; the subject is a coastal dike, not a tsunami.
Team, C. D.	Clawpack Development Team, with the version used and its DOI, and Mandli et al., PeerJ Computer Science 2 (2016) e68 doi:10.7717/peerj-cs.68.
Hall et al. (1953)	Two authors, Hall and Watts; Technical Memorandum No. 33, Beach Erosion Board, U.S. Army Corps of Engineers; the web address given leads to a different book.
Goring and Raichlen (1978)	A report by D. G. Goring alone (California Institute of Technology, KH-R-38 doi:10.7907/Z9MS3QPH); the journal version is Goring and Raichlen (1992), J. Waterway, Port, Coastal, Ocean Eng. 118 (1) 43–61.
Hammack Jr and Raichlen (1972)	The authors are Hammack and Raichlen doi:10.1061/9780872620490.153.
Bacigaluppi et al. (2019)	Water Waves 2 (2) 207–241 (2020).

Entry	Correction
Boussinesq (1872)	J. Math. Pures Appl. (2) 17, 55–108.
LeVeque et al. (2011)	Acta Numerica 20, 211–289.
Strupler et al. (2020a)	‘Robert, B.’ is R. Boes; the journal, Wasser Energie Luft 112 (1) 11–16, is missing.
Fuchs (2013)	A thesis (VAW-Mitteilung 221); add Fuchs and Hager (2012, 2015) and Fuchs, Heller and Hager (2010).
BASEMENT (Vetsch et al., 2019)	Cite also Vanzo et al., Environ. Model. Softw. 143 (2021) 105102 doi:10.1016/j.envsoft.2021.105102.
Lynett (2022), Hoffmann and Reicherter (2016), Roberts et al. (2013), Hock et al. (2019)	The book or the editors are missing or truncated.
Beakawi Al-Hashemi, Kazolea and Ricchiuto, Lo et al., Qu et al.	‘ https://doi.org/https://doi.org/ ’ is doubled.

Table R2. Corrections to the list of references of the manuscript.

TO CONCLUDE

My recommendation is a major revision. The question of your paper is the right one, the parameter study is a good idea, and the laboratory data are valuable. In my view the revised version needs five things. The quantity of the tables and that of the figures should be one and the same, in the elevation measure and with a stated reference. The numerical error and the model error should be separated, with converged references, the second-order results and the variable Π , which will show that the hydrostatic equations are adequate for Π below about one. The run-up comparison should be redone with a definition of r , a table of the geometry, the dispersive solver and an exact reference, and its text should say what the figures show about the slopes. The conclusions should be limited to a flat channel and a laboratory flume, or supported by a test at lake scale with a realistic source and geometry. And the equations of the model, the list of references and the misprints should be corrected.

I perfectly understand that this is a substantial amount of work, and *Cent fois sur le métier remettez votre ouvrage*² but I believe that the paper will become a much stronger one, and a useful tool for the community that studies lake hazards. I hope these remarks will be useful, and I look forward to reading the revised version. Please treat every point as a proposal and not as a verdict; of course it is possible that I have misunderstood something, in which case I will be glad to be corrected.

With kind regards,

The Referee

²French saying, from Boileau: put your work back on the loom a hundred times.

I. THE LINEAR WAVES OF THE SYSTEM (4)–(7)

Remark 1 explains why I looked at the linear waves of the dispersive system of Section 2.2. This appendix gives the computation, so that you can repeat it in a few minutes and drop it into the text if you wish. Throughout, the bed is flat, h is the constant still-water depth, $K = kh$ is the dimensionless wavenumber, and $\beta = B + 1/3$.

PROPOSITION R1. (Linear waves) *For plane waves of small amplitude, proportional to $e^{i(kx-\omega t)}$, the squared frequency is $\omega^2 = ghk^2$ for the hydrostatic equations (1)–(3), and*

$$\frac{\omega^2}{ghk^2} = \frac{1 - \beta K^2}{1 + \beta K^2} \quad (\text{R2})$$

for the system (4)–(7) as it is printed. If the factor $(1 - D)$ multiplies the whole momentum operator and Ψ is replaced by $D(N) - Bgh^2\partial_x^2(h\partial_x\eta)$, with $N = \partial_x(q^2/h) + gh\partial_x\eta$, which is the system of Madsen and Sørensen [R3] in the form given by Berger and LeVeque [R2], then

$$\frac{\omega^2}{ghk^2} = \frac{1 + BK^2}{1 + \beta K^2}. \quad (\text{R3})$$

Proof. Linearising (4) about the state at rest gives $\partial_t\eta + \partial_x q = 0$, so that $\hat{q} = (\omega/k)\hat{\eta}$ for plane waves. For a constant depth the operator of (7) is $D = \beta h^2\partial_x^2$, which multiplies a plane wave by $-\beta K^2$. In (6) the term $\partial_x(q^2/h)$ is quadratic and drops out, so that $N = gh\partial_x\eta$ and $\Psi = D(gh\partial_x\eta)$. Equation (5) then reads $(1 - D)\partial_t q = -gh(1 + D)\partial_x\eta$, that is $-i\omega(1 + \beta K^2)\hat{q} = -ikgh(1 - \beta K^2)\hat{\eta}$. Eliminating $\hat{q}$ gives equation (R2). For the second system, $(1 - D)(\partial_t q + gh\partial_x\eta) = -D(gh\partial_x\eta) + Bgh^3\partial_x^3\eta$, hence $(1 - D)\partial_t q + gh\partial_x\eta = Bgh^3\partial_x^3\eta$ and $\omega^2(1 + \beta K^2) = ghk^2(1 + BK^2)$, which is equation (R3). ■

COROLLARY R2. (Consequences) *The exact relation of the Euler equations is $\omega^2/(ghk^2) = \tanh(K)/K$. The printed system agrees with it up to order K^2 only if $B = -1/6$, and for $B = 0$ it is not Peregrine's relation $1/(1 + K^2/3)$ [R1]. For $K^2 > 1/\beta$ it has $\omega^2 < 0$, so that plane waves grow exponentially with a growth rate which tends to $k\sqrt{gh}$ as $K \rightarrow \infty$, and the initial value problem is ill-posed. For $B = 1/15$, equation (R3) is the Padé approximant of $\tanh(K)/K$ and*

$$\frac{1 + K^2/15}{1 + 2K^2/5} - \frac{\tanh(K)}{K} = \frac{K^6}{1575} + O(K^8). \quad (\text{R4})$$

Proof. The expansion of the exact relation is

$$\frac{\tanh(K)}{K} = 1 - \frac{K^2}{3} + \frac{2K^4}{15} - \frac{17K^6}{315} + O(K^8),$$

while equation (R2) gives $1 - 2\beta K^2 + O(K^4)$; the coefficients of K^2 agree if and only if $2\beta = 1/3$, that is $B = -1/6$. At $B = 0$ one has $\beta = 1/3$ and equation (R2) is $(1 - K^2/3)/(1 + K^2/3)$. If $\beta K^2 > 1$, then $\omega = \pm i\sigma$ with $\sigma^2 = ghk^2(\beta K^2 - 1)/(\beta K^2 + 1)$, and $\sigma^2/(ghk^2) \rightarrow 1$ as $K \rightarrow \infty$. For $B = 1/15$ one has $\beta = 2/5$ and

$$\frac{1 + K^2/15}{1 + 2K^2/5} = 1 - \frac{K^2}{3} + \frac{2K^4}{15} - \frac{4K^6}{75} + O(K^8);$$

since $17/315 - 4/75 = (85 - 84)/1575 = 1/1575$, this is equation (R4). ■

In terms of phase speeds, the three results say that the hydrostatic model travels too fast by the factor $1 + K^2/6 + O(K^4)$ relative to the exact speed, the Padé system with $B = 1/15$ is exact up to $K^6/3150$, and the printed system with $B = 1/15$ is too slow by $7K^2/30$ (Figure R4). For the pulses

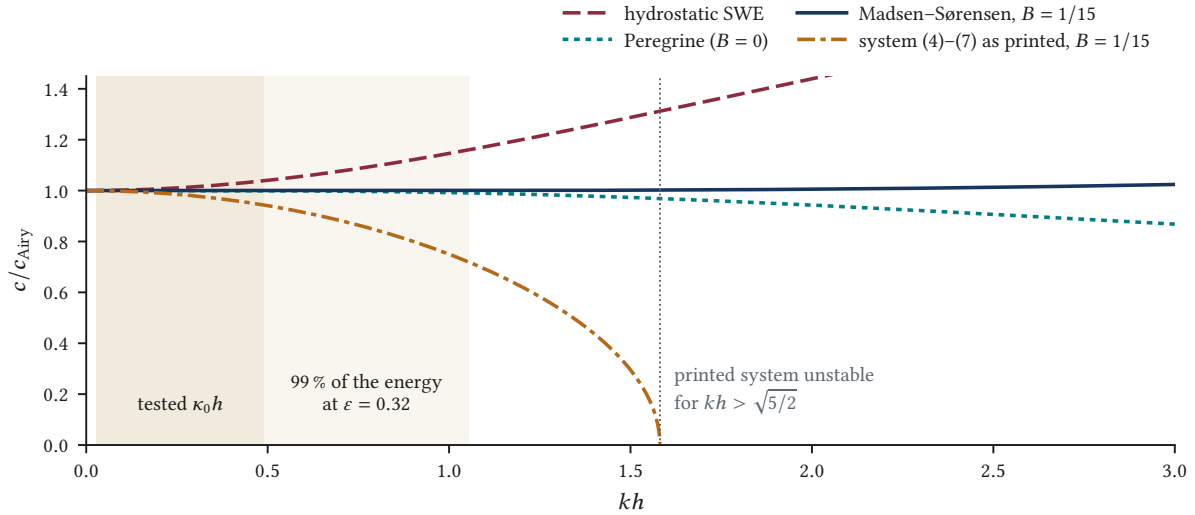


Figure R4. Phase speed divided by the exact phase speed $\sqrt{g \tanh(kh)/k}$, against kh . The hydrostatic model (dashed wine) is too fast at all wavenumbers; the Madsen-Sørensen system with $B = 1/15$ (navy) is on the line 1 over the whole range of the tests; the system (4)–(7) as printed (dash-dotted ochre) is too slow and has no real frequency beyond $kh = \sqrt{5/2}$. The shaded bands mark the wavenumbers $\kappa_0 h$ of the pulses of your Table 1 and the wavenumber below which 99% of the energy of the pulse with $\varepsilon = 0.32$ lies.

of your Table 1 the relevant wavenumber is $K = \kappa_0 h = \sqrt{3\varepsilon/4}$, between 0.03 and 0.49. There the hydrostatic phase speed is too large by $1.3 \cdot 10^{-4}$ ($\varepsilon = 0.001$) up to $3.9 \cdot 10^{-2}$ ($\varepsilon = 0.32$), and by 0.16 at the wavenumber below which 99% of the energy of the largest pulse lies; the Padé relation is exact to $4 \cdot 10^{-6}$ and $3 \cdot 10^{-4}$ in the same cases.

II. THE SOLITARY WAVES OF THE THREE MODELS

This appendix supports Remark 4. Let h be the still-water depth, $\varepsilon = A/h$, $H = h + \eta$ the total depth, and consider the following models over a flat bed. The Serre–Green–Naghdi (SGN) equations are $\partial_t H + \partial_x(Hu) = 0$ and $\partial_t(Hu) + \partial_x(Hu^2 + gH^2/2 + H^3(u_x^2 - u_{xt} - uu_{xx})/3) = 0$. The system of Madsen and Sørensen is the one of Proposition R1 with the corrected right-hand side, $(1 - D)(\partial_t q + N) = -DN + Bgh^3 \partial_x^3 \eta$ (flat bed, $D = \beta h^2 \partial_x^2$, $N = \partial_x(q^2/H) + gH \partial_x \eta$), and the hydrostatic equations are $\partial_t H + \partial_x(Hu) = 0$, $\partial_t u + u \partial_x u + g \partial_x \eta = 0$.

PROPOSITION R3. (Solitary waves) *First, (9) and (10) are an exact solution of the SGN equations. Second, a solitary wave of amplitude A of the Madsen–Sørensen system has a speed which does not depend on B , and*

$$\frac{c^2}{gh} = \frac{\varepsilon^2/2 + \varepsilon^3/6}{\varepsilon - \ln(1 + \varepsilon)} = 1 + \varepsilon + \varepsilon^2/6 + O(\varepsilon^3), \quad (\text{R5})$$

while its width depends on B . Third, for the hydrostatic equations the data (9)–(10) are not a simple wave: the Riemann invariant $r_+ = u + 2\sqrt{gH}$ is carried unchanged along the right-going characteristics and is largest at the crest, and after the left-going part has separated, the right-going crest has the amplitude A_R given by

$$\sqrt{1 + A_R/h} = \frac{1}{4} \left(\frac{\varepsilon}{\sqrt{1 + \varepsilon}} + 2\sqrt{1 + \varepsilon} + 2 \right), \quad (\text{R6})$$

that is $A_R = (1 - \varepsilon/8 + O(\varepsilon^2))A$; a left-going wave of height of order $\varepsilon A/8$ leaves.

Proof. The first statement is a direct substitution, which I checked symbolically; see also [R5]. For the second, a travelling wave $\eta(x - ct)$ of the Madsen–Sørensen system has $q = c\eta$ by the mass equation and the decay at infinity, so that $\partial_t = -c\partial_x$ and the momentum equation becomes

$$-c^2(h\eta/H)' + gH\eta' + (\beta c^2 - Bgh)h^2\eta''' = 0,$$

where I used $\eta - \eta^2/H = h\eta/H$. Integrating once, and then multiplying by η' and integrating again, gives $K_B\eta'^2/2 = W(\eta)$ with $K_B = (\beta c^2 - Bgh)h^2$ and

$$W(\eta) = c^2h(\eta - h\ln(1 + \eta/h)) - g(h\eta^2/2 + \eta^3/6).$$

A solitary wave of amplitude A has $\eta' = 0$ at $\eta = A$, hence $W(A) = 0$, which does not contain B and is equation (R5); the factor $K_B = gh^3(1/3 + \beta\varepsilon + O(\varepsilon^2)) > 0$ fixes the width. For the third statement, the invariant r_+ satisfies $\partial_t r_+ + (u + \sqrt{gH})\partial_x r_+ = 0$ and is increasing in η for the data (9)–(10), so its maximum is at the crest, where $u = \varepsilon\sqrt{gh/(1 + \varepsilon)}$ and $\sqrt{gH} = \sqrt{gh(1 + \varepsilon)}$. After the separation of the left-going part the right-going wave is a simple wave, $r_- = -2\sqrt{gh}$, and then $r_+ = 4\sqrt{g(h + A_R)} - 2\sqrt{gh}$; equating the two values of r_+ gives equation (R6). Expanding, $\sqrt{1 + A_R/h} = 1 + \varepsilon/2 - 3\varepsilon^2/16 + O(\varepsilon^3)$, whence $A_R/A = 1 - \varepsilon/8 + O(\varepsilon^2)$. ■

Table R3 gives the speed and the half-width at half height of the three solitary waves and of the exact solitary wave of the Euler equations, which I computed by a conformal-map method with a Newton iteration, to 10^{-10} . The amplitude A_R/A of the hydrostatic crest is 0.9951, 0.9904, 0.9815, 0.9654 for $\varepsilon = 0.04, 0.08, 0.16, 0.32$.

ε	speed squared, $c^2/(gh)$			half-width at half height/ h		
	SGN	MS	Euler	SGN	MS	Euler
0.01	1.010000	1.010017	1.009995	10.228	10.250	10.210
0.04	1.040000	1.040267	1.039917	5.189	5.234	5.152
0.08	1.080000	1.081072	1.079657	3.739	3.803	3.684
0.16	1.160000	1.164307	1.158518	2.740	2.831	2.653
0.32	1.320000	1.337353	1.312965	2.067	2.199	1.915

Table R3. Solitary waves of the Serre–Green–Naghdi equations (SGN), of the Madsen–Sørensen system with $B = 1/15$ (MS) and of the Euler equations. The SGN speed is within 0.3 percent of the Euler speed over the whole range, but the SGN wave is 0.7 percent too wide at $\varepsilon = 0.04$ and 8 percent too wide at $\varepsilon = 0.32$; the MS wave is wider than the SGN wave by 0.9 and 6.4 percent.

III. HOW A SOLITARY PULSE STEEPENS IN THE HYDROSTATIC EQUATIONS

This appendix supports Remark 7 and Remark 8. The hydrostatic equations over a flat bed are $\partial_t \eta + \partial_x((h + \eta)u) = 0$ and $\partial_t u + u\partial_x u + g\partial_x \eta = 0$. With $c = \sqrt{g(h + \eta)}$ and $c_0 = \sqrt{gh}$, the Riemann invariants $r_{\pm} = u \pm 2c$ satisfy $\partial_t r_{\pm} + (u \pm c)\partial_x r_{\pm} = 0$ as long as the solution is smooth. A right-going simple wave has $r_- \equiv -2c_0$, hence $u = 2(c - c_0)$, and η solves

$$\partial_t \eta + \lambda(\eta)\partial_x \eta = 0, \quad \lambda(\eta) = 3\sqrt{g(h + \eta)} - 2c_0. \quad (\text{R7})$$

PROPOSITION R4. (Breaking of a sech² pulse) *Let $\eta_0 = A \operatorname{sech}^2(\kappa x)$ be the initial value of equation (R7). The solution is smooth up to the time*

$$t_b = \frac{1}{3\sqrt{g/h}A\kappa G(s_*)}, \quad G(s) = \frac{s\sqrt{1-s}}{\sqrt{1+\varepsilon s}}, \quad (\text{R8})$$

where s_* is the maximiser of G on $(0, 1)$,

$$s_* = \frac{\sqrt{(1+\varepsilon)(9+\varepsilon)} - 3 + \varepsilon}{4\varepsilon}.$$

For $\kappa = \kappa_0 = \sqrt{3A/(4h^3)}$ and $\varepsilon \rightarrow 0$ one has $c_0 t_b = h\varepsilon^{-3/2}(1 + O(\varepsilon))$.

Proof. The solution is $\eta(x, t) = \eta_0(\xi)$ with $x = \xi + \lambda(\eta_0(\xi))t$, and it is smooth as long as $\partial x/\partial \xi = 1 + t\partial_\xi \lambda(\eta_0(\xi)) > 0$, that is until $t_b = 1/\max_\xi(-\partial_\xi \lambda(\eta_0(\xi)))$. On the front $\xi > 0$ put $s = \text{sech}^2(\kappa\xi)$, so that $\eta_0' = -2A\kappa s\sqrt{1-s}$ and $\lambda'(\eta) = (3/2)\sqrt{g/(h+\eta)}$; then $-\partial_\xi \lambda(\eta_0) = 3\sqrt{g/h}A\kappa G(s)$. The derivative of $\log G$ vanishes when $2\varepsilon s^2 + (3-\varepsilon)s - 2 = 0$, whose root in $(0, 1)$ is s_* , and this is the maximum since $G(0) = G(1) = 0$. As $\varepsilon \rightarrow 0$, $s_* \rightarrow 2/3$ and $G(s_*) \rightarrow 2/(3\sqrt{3})$, so that $3\sqrt{g/h}A\kappa_0 G(s_*) \rightarrow \sqrt{g/h}\varepsilon^{3/2}$ and $c_0 t_b \rightarrow h\varepsilon^{-3/2}$; the correction is of relative order ε . ■

The first jump appears at the distance $x_b = \xi_* + \lambda(\eta_0(\xi_*))t_b$ from the initial position of the crest, where $\text{sech}^2(\kappa\xi_*) = s_*$. For $h = 0.2$ m and the nominal pulses with $\varepsilon = 0.1, 0.2$ and 0.3 of your run-up tests, with the κ of equation (9), it is 8.0, 3.5 and 2.3 m, that is 40, 17 and 11 depths.

The distance is $h\varepsilon^{-3/2}$, the scale of nonlinear steepening of Madsen, Fuhrman and Schäffer [R9] (their equation (20)), here with its constant and its correction in ε ; it is sixteen times longer than the $0.062h\varepsilon^{-3/2}$ that you quote on page 7, which is another quantity of that paper (Remark 5). A more informative statement is that the whole evolution depends, to leading order, on one number.

PROPOSITION R5. (Similarity) Put $X = \kappa_0(x - c_0 t)$ and $\Pi = \varepsilon^{3/2}c_0 t/h$, and write $\eta = Af(X, \Pi)$. As $\varepsilon \rightarrow 0$ the simple wave equation (R7) with $\eta_0 = A \text{sech}^2(\kappa_0 x)$ satisfies, uniformly on compact sets and as long as it is smooth,

$$\partial_\Pi f + (3\sqrt{3}/4)f\partial_X f = 0, \quad f(X, 0) = \text{sech}^2 X, \quad (\text{R9})$$

while the Korteweg–de Vries equation $\eta_t + c_0\eta_x + (3c_0/(2h))\eta\eta_x + (c_0h^2/6)\eta_{xxx} = 0$ becomes $\partial_\Pi f + (3\sqrt{3}/4)f\partial_X f + (\sqrt{3}/16)\partial_X^3 f = 0$, which has the exact travelling wave $f = \text{sech}^2(X - \sqrt{3}\Pi/4)$. The pulse of equation (R9) breaks at $\Pi = 1$. Its maximum is 1 for $\Pi \leq \Pi_c$ and $F_B(\Pi) < 1$ afterwards, where

$$\Pi_c = \frac{4}{3\sqrt{3}} \cdot \frac{b}{1 - \text{sech}^2 b} = 1.4526, \quad \tanh b = b \frac{1 + \text{sech}^2 b}{2}, \quad b = 1.6061, \quad (\text{R10})$$

and $F_B(\Pi) \sim 1.755/\sqrt{\Pi}$ as $\Pi \rightarrow \infty$.

Proof. By the characteristics of equation (R7), the point of label X_0 is at

$$X = X_0 + \kappa_0 c_0 t (\lambda(\eta_0)/c_0 - 1), \quad \lambda(\eta)/c_0 - 1 = 3\eta/(2h) + O(\varepsilon^2).$$

Since $c_0 t = \Pi h \varepsilon^{-3/2}$ and $\kappa_0 h = (\sqrt{3}/2)\sqrt{\varepsilon}$,

$$\kappa_0 c_0 t \cdot \frac{3}{2}\varepsilon \text{sech}^2 X_0 = \frac{3}{2}(\kappa_0 h)\varepsilon^{-1/2}\Pi \text{sech}^2 X_0 = \frac{3\sqrt{3}}{4}\Pi \text{sech}^2 X_0,$$

so that $X = X_0 + (3\sqrt{3}/4)\Pi \text{sech}^2 X_0(1 + O(\varepsilon))$, which is the characteristic form of equation (R9). The largest slope of sech^2 is $4/(3\sqrt{3})$, attained at $\tanh^2 X_0 = 1/3$, so that the characteristics first cross at $\Pi = 1$. For the Korteweg–de Vries equation the same substitution gives the coefficient $3\sqrt{3}/4$ for the nonlinear term and, for the third derivative, $(\kappa_0 h)^3/(6\varepsilon^{3/2}) = \sqrt{3}/16$. The soliton of $f_\Pi + af f_X + bf_{XXX} = 0$ is $(3V/a) \text{sech}^2(\sqrt{V/(4b)}(X - V\Pi))$, and here the conditions of unit amplitude, $V = a/3$, and of unit width, $V = 4b$, coincide, both giving $V = \sqrt{3}/4$.

After breaking, the entropy solution of equation (R9) is given by the equal-area rule: the shock joins two characteristics with labels $a < b'$, which meet at the same X and satisfy

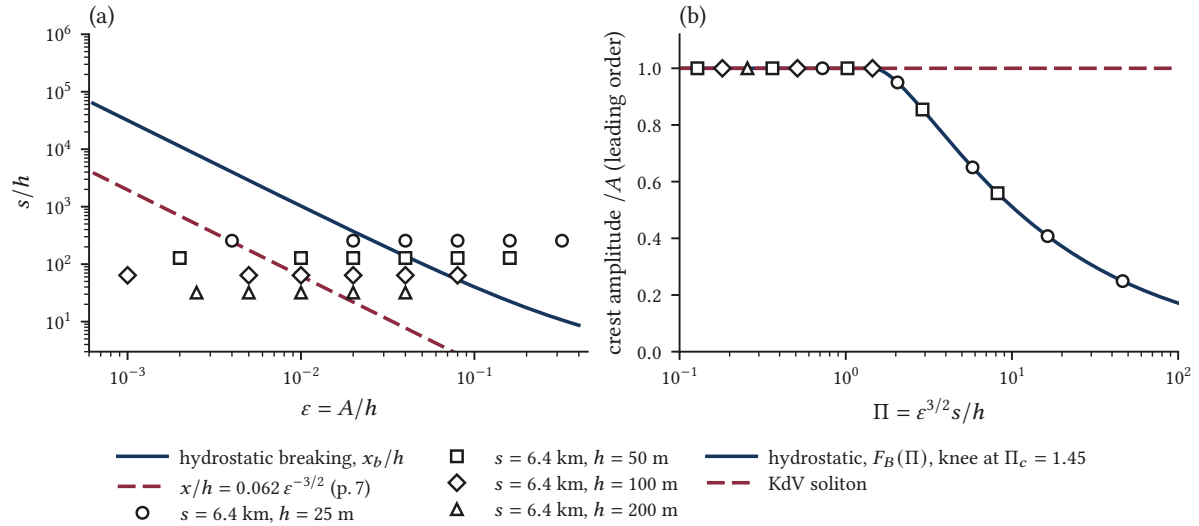


Figure R5. (a) Breaking distance x_b/h_{ref} of the hydrostatic pulse against ϵ (navy), the distance $0.062\epsilon^{-3/2}$ of page 7 (dashed wine), and the distance s/h_{ref} of your tests at 6.4 km for the four depths. A test lies above the navy curve when the pulse has broken before gauge 9. (b) Maximum of the pulse divided by A , to leading order, against Π , with your tests marked at 6.4 km; the maximum is unchanged up to $\Pi_c = 1.45$.

$$a + \frac{3\sqrt{3}}{4}\Pi \operatorname{sech}^2 a = b' + \frac{3\sqrt{3}}{4}\Pi \operatorname{sech}^2 b', \quad \tanh b' - \tanh a = (b' - a) \frac{\operatorname{sech}^2 a + \operatorname{sech}^2 b'}{2}.$$

The maximum is $\operatorname{sech}^2 a$, and it is 1 as long as the label a of the characteristic behind the shock is positive. At $a = 0$ the two conditions give the equations of equation (R10), which determine b' (written b there) and Π_c . For $\Pi > \Pi_c$ the label a is negative and the maximum decays; the conservation of $\int f dX = 2$ and the shape of a triangular wave give the law $2/\sqrt{c\Pi}$ with $c = 3\sqrt{3}/4$, that is $1.755/\sqrt{\Pi}$, as $\Pi \rightarrow \infty$ (see the discussion of the decay of a pulse in Chapter 2 of [R43]). ■

Values of F_B from the equal-area rule are 0.956, 0.844, 0.692, 0.565, 0.412, 0.297 and 0.213 for $\Pi = 2, 3, 5, 8, 16, 32, 64$; they approach $1.755/\sqrt{\Pi}$ slowly. Figure R5 shows the breaking distance against ϵ with the distances of your tests, and the decay of the maximum against Π ; Figure R6 shows your own tables against Π .

IV. WHAT A SUDDEN CHANGE OF THE WATER LEVEL PRODUCES

The sentence of page 7 (lines 168–169) says that a sudden positive or negative change of the water level at constant depth produces a solitary wave. For a positive change this is the content of the work of Hammack and Segur [R8] and of Segur [R44], which you cite. For a negative change it is not: Hammack [R7] states in the abstract of his paper that a negative displacement of the bottom produces only a dispersive wave train, without solitons, and the same is summarised by Hammack and Segur [R8]. The mathematics is short, and I offer it as a proposition, in the Korteweg–de Vries approximation used by these authors.

Let η solve $\eta_t + c_0\eta_x + (3c_0/(2h))\eta\eta_x + (c_0h^2/6)\eta_{xxx} = 0$, with $c_0 = \sqrt{gh}$ and an initial value η_0 which is continuous, integrable and decays fast enough for the inverse scattering transform to apply (for example $(1 + |x|)\eta_0 \in L^1$). With $u = -3\eta/(2h)$, $\xi = (x - c_0t)/h$ and $\tau = c_0t/(6h)$, the equation becomes $u_\tau - 6uu_\xi + u_{\xi\xi\xi} = 0$. By the work of Gardner, Greene, Kruskal and Miura [R45] and of Lax [R46], the solitons which emerge from η_0 for large time are in one-to-one correspondence with the negative eigenvalues $-\kappa_n^2$ of the Schrödinger operator $L = -\partial_\xi^2 + u_0$ on $L^2(\mathbb{R})$, the soliton

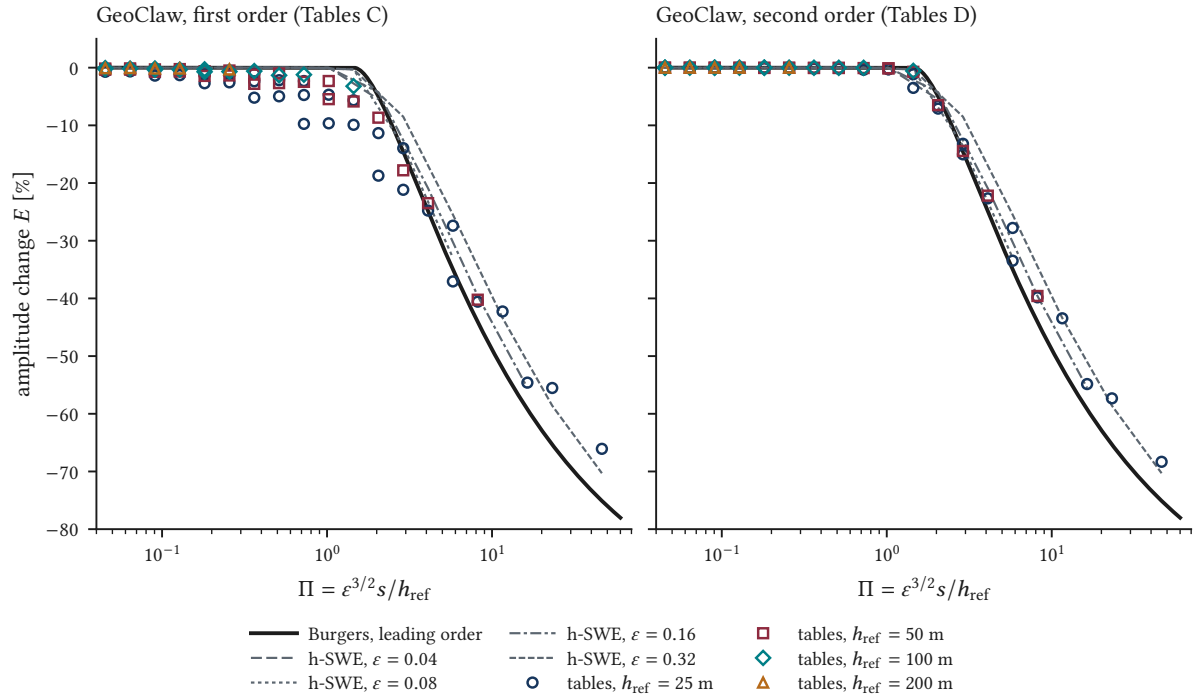


Figure R6. Amplitude change at all gauges for all cases of Tables C (left, first order, GeoClaw) and Tables D (right, second order, GeoClaw), converted to the elevation measure and plotted against $\Pi = \varepsilon^{3/2}s/h_{\text{ref}}$; the black curve is the leading-order solution of equation (R9) and the grey curves are converged solutions of the hydrostatic equations for four values of ε . At second order the points follow the hydrostatic curves; at first order the points with $\Pi < 1.45$ lie below them, and the vertical distance is numerical diffusion.

of $-\kappa_n^2$ having the amplitude $(4h/3)\kappa_n^2$ in η ; this is the form in which Segur [R44] uses the theory for water waves.

PROPOSITION R6. (Solitary waves from a sudden elevation or depression) *First, if $\eta_0 \leq 0$ then L has no negative eigenvalue, so that no soliton emerges: the depression disperses into a wave train. Second, if $\eta_0 \geq 0$ and η_0 is not identically zero, then L has at least one negative eigenvalue, so that at least one soliton emerges. Third, for $\eta_0 = A \text{sech}^2(\kappa_0 x)$ with $\kappa_0^2 = 3A/(4h^3)$, the operator L has exactly one negative eigenvalue, $-(\kappa_0 h)^2$; the soliton has the amplitude A , and the pulse is one soliton without any radiation. For $\eta_0 = A \text{sech}^2(\kappa_0 x/2)$, which is twice as wide, there are three solitons.*

Proof. For $\psi \in H^1(\mathbb{R})$, $\langle \psi, L\psi \rangle = \int (|\psi_\xi|^2 + u_0|\psi|^2) d\xi$. If $\eta_0 \leq 0$ then $u_0 \geq 0$, so that $\langle \psi, L\psi \rangle \geq 0$ and no eigenvalue is negative. If $\eta_0 \geq 0$ and $\eta_0 \not\equiv 0$, then $u_0 \leq 0$ and $\int u_0 d\xi < 0$. The trial function $\psi_\delta = e^{-\delta|\xi|}$ has $\int |\psi_{\delta,\xi}|^2 d\xi = \delta$ and $\int u_0 \psi_\delta^2 d\xi \rightarrow \int u_0 d\xi$ as $\delta \rightarrow 0$, so that $\langle \psi_\delta, L\psi_\delta \rangle = \delta + \int u_0 e^{-2\delta|\xi|} d\xi < 0$ for small δ . The essential spectrum of L is $[0, \infty)$, hence by the min-max principle L has a negative eigenvalue. For the third statement, $u_0 = -(3A/(2h)) \text{sech}^2(\kappa_0 h\xi)$ is a Pöschl–Teller potential, $-s(s+1)\mu^2 \text{sech}^2(\mu\xi)$ with $\mu = \kappa_0 h$ and $s(s+1) = (3\varepsilon/2)/(3\varepsilon/4) = 2$, that is $s = 1$. Its negative eigenvalues are $-\mu^2(s-n)^2$ for the integers n with $0 \leq n < s$, so there is exactly one, $-(\kappa_0 h)^2 = -3\varepsilon/4$, and the amplitude of the soliton is $(4h/3)(3\varepsilon/4) = \varepsilon h = A$. For the pulse of twice the width, $\mu = \kappa_0 h/2$ and $s(s+1) = 8$, so $s = (\sqrt{33} - 1)/2 \approx 2.37$ and $n = 0, 1, 2$. ■

The sentence on page 7 can therefore be corrected to say that a sudden elevation of the water level evolves into at least one solitary wave, followed by a dispersive tail, while a sudden depression produces a dispersive wave train only.

V. THE RUN-UP OF A SOLITARY WAVE ON A PLANE BEACH

This appendix supports Remark 14 and Remark 15. It gives the reference against which I read Figures 9 and 10, with its derivation, so that you can reproduce it in a few lines and use it to verify BASEMENT, GeoClaw and BoussClaw alike, and it describes the independent computation of the hydrostatic equations behind Figure R2. The setting is the canonical one of Synolakis [R38]. The depth is the constant d beyond the toe $x = d \cot \beta$, and $h(x) = x \tan \beta$ on the beach $0 \leq x \leq d \cot \beta$, the shoreline at rest being at $x = 0$. Lengths and elevations are measured in d and times in $\sqrt{d/g}$; $\varepsilon = A/d$, $\gamma = \sqrt{3\varepsilon/4}$ and $\mu = \gamma \cot \beta$. The solitary wave reaches the toe as $\eta_{\text{in}}(t) = \varepsilon \operatorname{sech}^2(\gamma t)$, which is its profile in time there to leading order in ε .

PROPOSITION R7. (Run-up of a solitary wave in the linear theory) *In the linear long-wave theory with the exact shoreline condition, the vertical displacement of the shoreline is*

$$R(s) = 2\varepsilon \operatorname{Re} \int_0^\infty \frac{v e^{-ivs}}{\sinh(\pi v/2)(J_0(2\mu v) - iJ_1(2\mu v))} dv, \quad s = \gamma t, \quad (\text{R11})$$

where J_0 and J_1 are the Bessel functions, so that the maximum run-up is $R_{\text{max}} = \varepsilon \Phi(\mu)$ with a function Φ of $\mu = \sqrt{3\varepsilon/4} \cot \beta$ alone. Moreover $\Phi(0) = 2$, and as $\mu \rightarrow \infty$

$$\Phi(\mu) = c_\infty \sqrt{\mu} + O(\mu^{-1/2}), \quad (\text{R12})$$

where

$$c_\infty = 2\sqrt{\pi} \max_\sigma \int_0^\infty \frac{v^{3/2} \cos(v\sigma - \pi/4)}{\sinh(\pi v/2)} dv = 3.0427.$$

This is the law $R_{\text{max}} = 2.831 \sqrt{\cot \beta} \varepsilon^{5/4} d$ of Synolakis [R38]: the number 2.831 is $c_\infty (3/4)^{1/4}$.

Proof. In these units the linear long-wave equations are $\eta_t + (hu)_x = 0$ and $u_t + \eta_x = 0$, so that $\eta_{tt} = (h\eta_x)_x$, with $h = x/X_0$ on the beach, $X_0 = \cot \beta$, and $h = 1$ beyond the toe $x = X_0$. For a wave of frequency ω put $\eta = \operatorname{Re}(\hat{\eta}(x)e^{-i\omega t})$. On the beach $(x\hat{\eta}')' + X_0\omega^2\hat{\eta} = 0$, and the substitution $\xi = 2\omega\sqrt{X_0x}$ turns it into Bessel's equation of order zero,

$$\hat{\eta}_{\xi\xi} + \hat{\eta}_\xi/\xi + \hat{\eta} = 0,$$

whose solution bounded at the shoreline $\xi = 0$ is $\hat{\eta} = C J_0(\xi)$; the other solution has a logarithmic singularity. Beyond the toe $\hat{\eta} = a e^{-i\omega(x-X_0)} + b e^{i\omega(x-X_0)}$, with an incident wave of amplitude a and a reflected wave of amplitude b . The depth is continuous at the toe, so that $\hat{\eta}$ and $\hat{\eta}'$ are continuous there, and $d\xi/dx = \omega$ at the toe; hence

$$a + b = C J_0(2\omega X_0), \quad i(b - a) = -C J_1(2\omega X_0),$$

and $C = 2a/(J_0(2\omega X_0) - iJ_1(2\omega X_0))$. In the linear theory the vertical displacement of the shoreline is η at $x = 0$, where $J_0 = 1$, so that a harmonic incident wave a produces the run-up amplitude $2a/(J_0 - iJ_1)$.

The Fourier transform of the incident wave is the classical

$$\hat{a}(\omega) = \int \eta_{\text{in}}(t) e^{i\omega t} dt = \frac{\varepsilon \pi \omega}{\gamma^2 \sinh(\pi \omega / (2\gamma))}.$$

Since J_0 is even and J_1 is odd, the integrand takes conjugate values at ω and $-\omega$, and the superposition of the harmonic waves gives

$$R(t) = \frac{1}{\pi} \operatorname{Re} \int_0^\infty \frac{2\hat{a}(\omega)e^{-i\omega t}}{J_0(2\omega X_0) - iJ_1(2\omega X_0)} d\omega.$$

The substitution $\omega = \gamma v$ turns this into equation (R11), where $2\omega X_0 = 2\mu v$, so that μ is the only parameter.

For $\mu = 0$ one has $J_0 = 1$ and $J_1 = 0$, and equation (R11) is $2\varepsilon \operatorname{sech}^2 s$, by inversion of the transform above: the wall reflects the wave and doubles it, $\Phi(0) = 2$. For large μ use Hankel's expansion, $J_0(\xi) - iJ_1(\xi) = \sqrt{2/(\pi\xi)}e^{-i(\xi-\pi/4)}(1 + O(1/\xi))$. The function $1/(J_0 - iJ_1)$ is continuous on $[0, \infty)$, because J_0 and J_1 have no common zero, and by the expansion it differs from $\sqrt{\pi\xi/2}e^{i(\xi-\pi/4)}$ by $O(\xi^{-1/2})$. Since $v^{-1/2}v/\sinh(\pi v/2)$ is integrable on $(0, \infty)$, the error made in equation (R11) by this replacement is $O(\mu^{-1/2})$, uniformly in s . With $\sigma = 2\mu - s$ the leading term is

$$2\sqrt{\pi\mu} \int_0^\infty \frac{v^{3/2} \cos(v\sigma - \pi/4)}{\sinh(\pi v/2)} dv.$$

Its maximum over s is its maximum over $\sigma \in \mathbb{R}$, which I computed: it is attained at $\sigma = 0.3657$ and the integral is 0.8583, whence $c_\infty = 3.0427$ and $c_\infty(3/4)^{1/4} = 2.8315$, since $R_{\max}/d = \varepsilon c_\infty \sqrt{\mu} = c_\infty(3/4)^{1/4} \varepsilon^{5/4} \sqrt{\cot \beta}$. ■

For a wave that does not break, the maximum run-up of the nonlinear shallow-water equations is the same number, as Synolakis [R38] uses for the solitary wave, following the exact solution of the nonlinear equations on the plane beach by the hodograph transformation of Carrier and Greenspan [R47]. I checked the statement on the exact nonlinear standing wave of Carrier and Greenspan, whose maximum run-up the solver described below reproduces to 1 percent. The criterion of Synolakis for the onset of breaking in the nonlinear shallow-water equations, $\varepsilon > 0.818(\tan \beta)^{10/9}$, gives 0.14, 0.30 and 0.52 on your three slopes. For the full potential flow, Grilli, Svendsen and Subramanya [R48] find that no wave breaks on slopes steeper than 12 degrees and, on the slope 1:5, that breaking needs $\varepsilon > 0.68$; their criterion predicts no breaking in any of your cases, so that the deficit of the laboratory on the gentle slope is not due to breaking. Because Φ is a function of μ alone, the amplification R/A is the same for all pairs (ε, β) with the same μ , and in your tests μ lies between 0.58 and 2.37, where neither limit of the proposition is accurate; this is why I evaluate equation (R11) and do not use the law. Table R4 gives μ , Φ and the run-up for the seven cases of your Figure 10, and Figure R7 compares the theory with the laboratory and with BASEMENT for all ε . Two checks of equation (R11): at the slope 1:19.85 of the laboratory tests of Synolakis, a benchmark of Synolakis et al. [R37], it gives 0.0883 and 0.2295 (in units of d) for $\varepsilon = 0.0185$ and 0.04, against 0.0861 and 0.2256 from the law; and if the width of the incident wave is taken from the solitary wave of equations (9)–(10), $\gamma = \sqrt{3\varepsilon/(4(1+\varepsilon))}$, the values of the table are lower by 2 to 6 percent.

slope	ε	linear theory			your Figure 10		hydrostatic equations	
		μ	Φ	r [m]	laboratory	BASEMENT	no friction	friction
1:5 11°	0.1	1.37	3.72	0.074	0.065	0.068	0.083	0.074
	0.2	1.94	4.37	0.175	0.132	0.124	0.196	0.134
	0.3	2.37	4.80	0.288	0.197	0.167	0.257	0.170
1:2.5 22°	0.2	0.97	3.20	0.128	0.120	0.160	0.187	0.147
	0.3	1.19	3.49	0.209	0.180	0.190	0.252	0.191
1:1.5 34°	0.2	0.58	2.62	0.105	0.100	0.157	0.177	0.152
	0.3	0.71	2.82	0.169	0.155	0.228	0.244	0.202

Table R4. Run-up r in metres for $h_{\text{ref}} = 0.2$ m. Linear theory: $\mu = \sqrt{3\varepsilon/4} \cot \beta$, $\Phi = r/(\varepsilon h_{\text{ref}})$ and r from Proposition R7. Laboratory and BASEMENT: the values of your Figure 10. Hydrostatic equations: my independent solution with the crest $0.5L_w$ from the left boundary, without friction and with the friction of page 9 ($k_b = 0.5$ mm); behind a bore the frictionless values are lower bounds, since they still grow when the cells are refined.

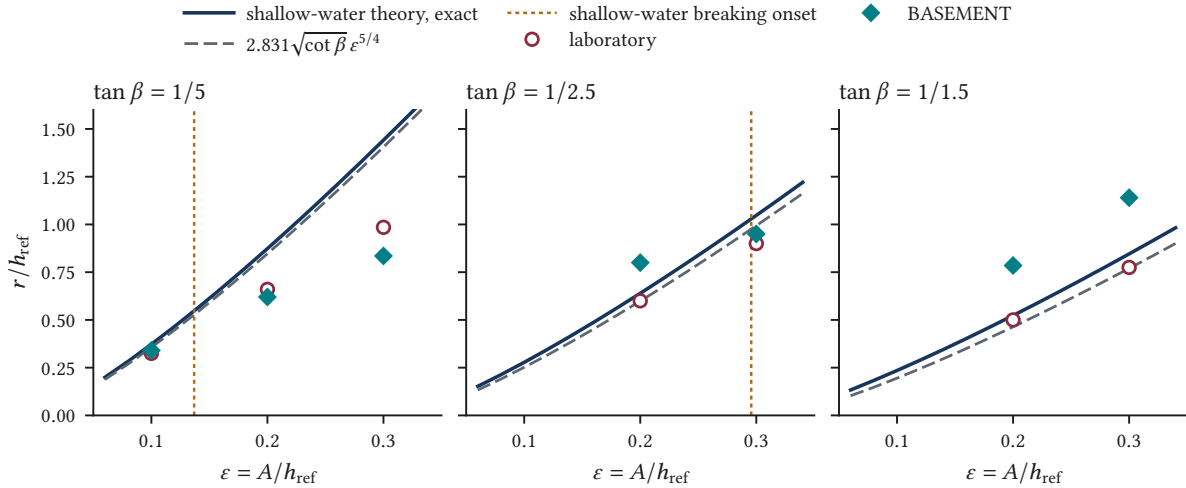


Figure R7. Run-up divided by h_{ref} against ε on the three slopes of your Figure 10. Navy: the exact linear theory of Proposition R7, which is also the run-up of the nonlinear equations as long as the wave does not break; dashed: the law of Synolakis [R38]; dotted: the onset of breaking in the nonlinear shallow-water equations (beyond the range for the slope 1:1.5); circles: the laboratory; diamonds: BASEMENT. The laboratory follows the theory, below it by 4 to 32 percent, with a deficit that grows with the length of the swash, while BASEMENT leaves it on the two steep slopes.

The computation behind Figure R2 and Table R4 solves the hydrostatic equations with the friction law of page 9,

$$h_t + q_x = 0, \quad q_t + (q^2/h + gh^2/2)_x = -ghz_x - q|q|/(c_f^2 h^2),$$

$$c_f = 5.75 \log_{10}(12h/k_b) \quad \text{for } h > k_b, \quad c_f = 5.75 \log_{10} 12 \quad \text{otherwise.}$$

The scheme is of second order: minmod reconstruction of h , u and $h + z$, the hydrostatic reconstruction of Audusse et al. [R14] with a centred source for the bed, an HLL flux with wave speeds adapted to dry cells, Heun's time stepping at a Courant number of 0.45, and implicit friction. The run-up is the maximum over time of the elevation above still water of the last cell with a depth above 10^{-5} m. It was validated before it was used. The lake at rest with a wet-dry front is preserved to round-off, and the volume of a closed basin is conserved to round-off while a solitary wave climbs and descends. The dam break of Ritter converges at rate one in L^1 . For the exact nonlinear standing wave of Carrier and Greenspan [R47] with amplitude parameter 0.8, where 1 is the limit of breaking, the maximum run-up is 0.2010 against the exact 0.2000. For solitary waves on the 1:19.85 beach it gives 0.0881 and 0.2241 against the exact linear 0.0883 and 0.2295. With friction the run-ups of your set-up change by less than 1.5 percent when the cells are halved from 2 to 1 mm. Finally, the first jump of the pulse appears where Appendix III puts it, a little after it (3.85 and 2.49 m against 3.48 and 2.27 m for $\varepsilon = 0.2$ and 0.3), since a jump needs a few cells to be resolved.

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