**Correspondence between de Saint Venant and Boussinesq**

**1: Birth of the Shallow Water Equations**

Willi H. Hager1, Oscar Castro-Orgaz2, Kolumban Hutter1

**Complementary Files**

These files contain material also relevant in the main paper, but presented here for reasons of space.

Letter 17, written by dSV on July 8, 1871, continues with the velocity correction coefficient: “Your last letter dated July 5 was written by you so as to convince me. It was a pleasure to read it. However, please do not repeat your arguments stated in all previous letters related to the establishment of the governing equations. Your ‘simple force’ corresponds to a work that I cannot simply accept, because I do no longer have here the ‘Note on the permanent movement of water’. I would be obliged to reconsider it to finally understand your text, yet I have neither time, nor the mental condition for this. You can only convince me by a Note or a letter if you support me fully, where you define all your notations, or you even explain it how you do it with your students.”

“Be sure to say without explanation as in your letter dated May 31, whether the pressures on the sections AB, A′B′ are *P* and [*P*+*ρgω*(d*V*/d*x*)d*x*]. As for me, the pressure on a section depends on the flow depth above the gravity center, which is equal to +(1/2)*ρgω*(d*V*/d*x*)d*x* (ah, I understand now that the pressures on all elements of A′B′ are these on the elements of AB plus *ρg*B′c = *ρg*d*h*, but you should say so). Define *i*, is this the bottom slope or the free surface slope? I cannot follow without an explanation that the driving unit force due to gravity is *ρgω*d*x*⋅*i*, because this depends on the trajectory of the gravity center when decomposing the gravity force. Explain.”

“And if you consider a single streamline, the weight is decomposed following its own slope, but not the line of slope *i*. I start to understand. You accept certainly the bottom as the axis of the *x*-coordinate, and you decompose the average quantities following *x*. And the bottom, under these circumstances, does it have a variable slope?”

“What is now *α*′? And what is d*u*/d*x*, or is it dc*u*/d*t* = (∂*u*/∂*t*)+(∂*u*/∂*x*)(d*x*/d*t*) in which dc represents the total differential? I also admit as for the permanent motion d*u*/d*t*=0, one only has (d*u*/d*x*)(d*x*/d*t*) = *u*(d*u*/d*x*). However, this should be stated.”

“Do no longer state «as various times demonstrated in my letters». One simply has (d*u*2d*ω*/d*x*)=2*u*d*ω*(d*u*/d*x*); repeat the reason, which I think is =(d*u*d*ω*/d*t*)*u*+ *u*d*ω*(d*u*/d*x*), and that (d*u*d*ω*/d*x*) = 0. Define the ∂ and the ‘d’, and their differences, and omit all ∂’s reminding me of the use of variations [Sic.: From then onward, JB only used ‘d’ and ‘∂’ for the ordinary and partial derivatives, respectively]. The *u*′, *h*′ etc. are certainly good abbreviations, but not when you should convince readers of a statement so far not considered. Use their language, if notations as *u*′, *h*′ do not define the exact derivative taken of the variable.”

“To reject an argument, late Coriolis made it all differently: He used his approach, included his reasoning, accepted what he thought was reasonable, and showed what he had missed. Did he [Sic.: Coriolis] use the kinetic energies? If he was correct, after having demonstrated that the use of the momentum theorem leads to *α*′, he demonstrated that the use of the energy equation leads to the same result, noting that multiplication involves only *u*d*x* instead of multiplying by d*x* before integrating all terms of the equation. This would lead to negate the ‘equation of energy and work’.”

“In revising the text, particularly with new additions, please remain calm and patient, to avoid the impatience of readers who are not always ready to accept all findings, leaving the author alone with his work. Exercise charity for the good-willed readers if there are any. By this approach, you will neither be rejected nor be fully appreciated, mainly by members of another Section [Sic.: of the *Académie*]. God does not only like my proposals, of which I do not guarantee the perfect goodness. I support your inventive spirit taking you without effort to new findings.”

“D. Bernoulli, Euler etc. also had imagination. They also had clarity, patience in terms of writing even in a foreign language, and to set down everything during revision in a scholarly way, using theorems, corollaries. And Fourier?, what a master of passionate clarity! He also acquired fame with his book ‘On the theory of heat’, even though it is less profound than those of Poisson or Cauchy with respect to partial differential equations. If you prepare in future what you did on the intumescence, and its multiple waves, please add a sketch [Sic.: as dSV did, see Fig. 16].”



**Fig. 16.** Sketches of positive and negative surges propagating into stillwater (caption provided by the authors)

“God will be pleased to see that I engage you to overlook the charlatanism entering the smallest things and allowing for a simple entrance to the *Institut* with the lightest scientific baggage. You have better things to do. Yet you must assert of what you do, and look how you can guide others in developing positive and useful results for science and its applications. The supplements of your Note will be published, yet modifications as you would finally like to add would ask for additional expenses. Yours good and affectionate dSV.”

“P.S. I have finally understood you, and I think you are correct. I will always send you your letters and please do what you like with my general recommendations.”

“I think that it would be good to represent the internal friction as *ρgf* instead of *f*, and similarly for wall friction *ρgF*. This would drop in the Coriolis equation the denominator *ρg*. Wouldn’t it also be good to introduce the streamwise coordinate *x* such that the integrals become ∫0*h* d*z* instead of the embarrassing ∫*H*−*hH* d*z*, except you adopt in your ‘innovation’ of deriving definite integrals such as ∫(*h*) ∫(*ω*) all over *h* or *ω* [Sic.: *H* is the flow depth, and *h* either the bottom elevation or the flow depth]. Accordingly, the error of Coriolis and those having followed him, such as Bélanger and Bresse, is that the total work done by the fluid flow due to internal and external friction, is the work of the entire friction corresponding to *ρgF* (along the bottom) for the average velocity *V*, namely *ρgFV*d*t* within the time d*t*, but not ∫0*h* (d*f*/d*s*)*u* d*z*. This is identical to uniform movement; I indeed demonstrated this in 1836 (and said it to Coriolis with a vivid voice, who repeated it to Caligny and his students; it was published in 1845 in the *Société philomathique*). Bélanger (who did not know what I had said) did not prove it except for uniform flow. If the motion is variable, I admit that this is another matter. The difference between the two quantities of work per unit time would then be



.

This could be applied for the largest part of your researches. I have indeed had the chance to seek whether the work of the total internal friction, plus that of *Fu*od*t* due to bottom friction, is identical to *FV*d*t*, namely the work due to friction on the bottom involving the average cross-sectional velocity. If this would apply to both the uniform and the gradually varied motions, the above would solve this embarrassing question.”

“I regret that various things that you found are of small interest to practitioners. The merit remains the same, but it would be more difficult to find support for your results in the Section of Mechanics when compared with results of relevance. From this perspective, it is often more convenient to use the momentum instead of the energy conservation theorems. Yes, the use of the momentum instead of the energy theorem depends on the corrective coefficients, and knowledge, whether they can be dropped. I will possibly present a Note on *α*′ to be substituted for *α*, in which I will cite you.” Note that dSV at once became interested in the velocity correction coefficients, and in the differences between the two mechanical conservation theorems.

Letter 38, dated December 4, 1871, reads: “I inform you with pleasure, dear Sir, that Phillips, as cause of his course at *Ecole Polytechnique*, starting in February, will not be able until then to examine the works and lists of the candidates replacing Piobert. The election will therefore be only in March.”

“I am not less in charge to finish the revisions of various works, including yours ‘On the theory of light’. The memoir of Mr. Kleitz, which I thought would occupy me for some 300 days, has taken me only a fortnight without counting the months during which I was occupied by him two years ago. Finally, it is done! I say that his analysis on the variable coefficient *ε* [Sic.: of viscosity], was it complete, or does it exclude also powers of the expression d(*u*, *v*, *w*)/d(*x*, *y*, *z*)? If the knowledge on the molecular law would allow for its numerical evaluation, as a quantity of the order of *ε* = 1/7478 which you proposed from Poiseuille’s experiments, which would not be 1% or 0.1% to be attributed to this coefficient, supposed constant in a water course. It thereby satisfied both the formula of Tadini, *V*=*k*(*RI*)1/2, and the ratio *ξ*/*A* between the maximum and average velocities. For a rectangular channel of 1 m flow depth, 6 m width and an average velocity of 1 m/s, assuming the velocity distribution following a parabolic ellipse, and the maximum velocity located at 80% above the channel bottom, I obtain *ε*=1/4, i.e. far from *ε* = 1/7478 of water flows with a regular movement as in Poiseuille tubes. In your Note on permanent water flow, published on July 3, 1870, you find *ε* = 0.00064*ρgu*oφ(*R*) in which *u*o=1, *ϕ*=1, *ρg*=1000, *ε*=0.64. This is of similar magnitude.”

“As you say, by keeping note of your last two letters, I adjourn the continuation of my work ‘On the unsteady movement and the tidal rivers’ trying to raise the objection of M. Partiot. What made me find the absurd was that *x* = (*t*−*τ*)[3(*gy*)1/2−2(*gy*o)1/2] becomes negative, even for *t*−*τ* positive, as the value *y*o of *y*, for horizontal stillwater in the canal into which the tide is discharged, if this value of *y*o is between *y* values for the low and high tides. One has certainly to add a velocity +*V*o to the results, to be studied. Yours well affectionate dSV.”

In Letter 30, dated Sept. 17, 1872, dSV continues his discussion: “I did not yet entirely go through your new memoir. I will probably agree with you as you look into the third yet incomplete approximation where you assume a linear variation of streamline inclination between the bottom and the surface, i.e. the relation *w*/*u*=(d*h*/d*s*)(*z*/*h*). I am inclined to look at the computations of this third approximation as being useful to indicate a limit beyond which the 2nd approximation would be insufficient. In this third approximation, I have corrected all errors from the supposition to drop all products of d*h*/d*x* and d2/d*x*2(d*h*/*h*d*x*), because, in differentiating the following expression in *x*, I account for the two, starting from the surface,

1. *p*/(*ρg*) = *p*1/(*ρg*)+*z*−(*h*2/*g*)d/d*x*[(d*h*/*h*d*x*) ∫0*z* *u*2(*z*/*h*)(d*z*/*h*)],

instead of differentiating only the first factor

1. (*h*2/*g*)d/d*x*[d*h*/(*u*d*x*)].

Of its last term, one would remove the product of this factor from d/d*x* [∫0*z* *u*2(*z*/*h*)(d*z*/*h*)] and delete the product of two first order quantities, so that

1. (*h*2*V* 2/*g*)d/d*x*[d*h*/(*h*d*x*)] ∫0*z* (*u*2/*V* 2)(*z*/*h*)(d*z*/*h*).

However, the differential d/d*x* of ∫0*z* (*u*2/*V*2)(*z*/*h*)(d*z*/*h*) appears to me to be much smaller than that of ∫0*z* *u*2(*z*/*h*)(d*z*/*h*). It can be considered of second order since the ratio *u*/*V* varies much less with *x* than *u* or *V*. If, therefore, in differentiating (c), the term ∫0*z* is neglected, this only results in a mistake of some sort of third order.”

“Because (*h*2*V* 2/*g*) is constant, differentiate d/d*x*[d*h*/(*h*d*x*)], and the result is

1/(*ρg*)(d*p*/d*x*) = −(*h*2/*g*)d2/d*x*2(d*h*/*h*d*x*) [∫0*z* *u*2(*z*/*h*)(d*z*/*h*)],

providing as a type of computation simply the expression

1/(*ρg*)(d*p*/d*x*) = −(*h*/*g*)d3/d*x*3 [∫0*z* *u*2(*z*/*h*)(d*z*/*h*)]

by dropping products of d*h*/d*x*, d2*h*/d*x*2, d/d*x* [∫0*z* *u*2(*z*/*h*)(d*z*/*h*)]. Moreover, if the limits d*h*/d*x*=0.005, d2/d*x*2(d*h*/*h*d*x*)=…, etc. of the previous approximation are exceeded in my formulas of subsequent approximation, then, I think, there remains at least the correction of a part, perhaps the strongest part of the error. This is, indeed, something practical here. The remainder, I do not want to change.”

“Continue, dear Sir, to elucidate this question, by considering that all what we did is nothing compared to what we have to do. However, of course, do not exhaust yourself and manage well your forces.”

“I checked several times whether my equations are dimensionally homogeneous, which often they are not after having them slightly amended and in so doing forgotten a factor *h* or 1/*h* in one of the terms; I had the opportunity to notice that your coefficients *A* and *B* have dimension −*1* [Sic.: length-1], so that 1/*A*, 1/*B* are lengths. As you do not have to print your memoir yet, would it not be simpler calling *A* and *B* for clarity 1/*A*, 1/*B*? Similarly, you called *b* what you call now 1/*b*1, that for uniform flow is given by *hi*=*V* 2/*b*, with *b* as a length. This would better be changed because we wouldn't have, as is done in England for example, to express *A*, *B*, *b* in weight rather than in meters.”

Letter 2, dated January 30, 1873, is a further idea of dSV, reading: “My dear Professor [Sic.: JB was since 1873 professor at Lille University], the issue of the application, either for internal tides, or flood flows, based on the two equations of unsteady water flow, presents difficulties for me that I did not realize. They probably appeared also to you, since you seem to require to resolve several facts: one example is the first application (to tides), occurring at the mouth of a river subjected by the tides; the concern is not only the relation between the water depths *h* and time *t*, but also the law of water entry or exit at velocity *V*. These two arbitrary functions *h*=f(*t*), *V*=F(*t*) are needed, you say in your letter of January 23, to determine the water level *h* and *V* at a time Δ*t* later, at a distance Δ*s* of the mouth, then in the same way at distances 2Δ*s*, etc. It is only by means of these two eqs. (256), leading, with *i* as the bottom slope, to

 ,

 .

If you know, at point *s*=0, not only at the initial time *t*=0, but for an arbitrary time *t*, the values of *h*, *V*, d*h*/d*t*, d*V*/d*t*, one knows that these two equations provide d*h*/d*s*, d*V*/d*s*, so that we compute for Δ*s* and at all instants *Vs*=Δ*s*=*Vs*=0+(d*V*/d*s*)Δ*s*, *hs*=Δ*s*=*hs*=0+(d*h*/d*s*)Δ*s*, from where you pass to *Vs*=2Δ*s*, *hs*=2Δ*s*, etc. and further to *V* for any *s*, at any time. This is correct. However, it seems to me that nature does not allow for these operations without indetermination. The law of *h* [Sic.: as a function of time *t*] at the river mouth should be sufficient for *s*=0; in addition, the flow depths *h* and the velocities *V*, or only the variation of *h* at *s*=0, *s*=Δ*s* (in addition to *q*=*Vh*) of the current whose width and bottom slope *i* should be known at time *t*=0. If the initial motion were steady, the movement could be determined with this information, and the tide will not have two modes of behavior in entering the river. It would be in vain to impose a law *hζ*=0=f(*t*) as proposed by you in addition to a second law *Vζ*=0=F(*t*). This second function is not arbitrary once the first and the initial discharge are given. This function *Vζ*=0=F(*t*) would be incompatible with the first *hζ*=0=f(*t*) as well as with the initial conditions.”

“The problem of the non-permanent flow is without difficulty if the state of a stream at a certain moment is given; the remainder of its subsequent states, either assumed indefinite in both directions, or closed by a domain (I would say even given at two ends) has to be deduced because, if you know both *h* and *V* at *s*=Δ*s*, 2Δ*s*, 3Δ*s*, etc., you also know both d*h*/d*s*, d*V*/d*s*, and the two above equations result equally in values of d*h*/d*t*, d*V*/d*t* and consequently for *t*=Δ*t*, in all details previously known at *t*=0.”

At this point dSV speculates on how the two differential equations for *h* and *V* could be integrated with different boundary conditions, e.g. when continuous and temporally variable lateral inflow from a rising sea is acting, but seems to give up in view of the unsurmountable difficulties he is confronted with. He then returns to his problem of finite wave propagation, asking: “But, can we, for tides of finite height, comparable to the original water depth, and even greater, operate by such a kind of super-imposition of small effects? Well, I guess yes, and that's what I offer to your thoughts. Assuming that the sea at the river mouth increases first by an extremely small height Δ*h*, we determine the reaction in the channel. Then, without waiting until the generated intumescence will have traveled along the entire channel length, we will after a certain time generate a second intumescence, etc. Each of these will be small, so that we approximately will obtain also their effects given that the formulas apply perfectly to wave heights much smaller than the original water depth. This means to go back to the method of Mr. Partiot, but operating with accurate formulas, accounting for friction and differences of velocities along streamlines, the bottom slope and the cross-sectional area, leading to the gradual height decrease of the intumescence, by putting its concave surface upward. This means that instead of using my ‘exact formulas’ *V*=2(*gy*)1/2−2(*gh*)1/2 and *k*=3(*gy*)1/2−2(*gh*)1/2, the solution will be designed based on the results of your recent researches.”

It seems that dSV suggests here to split in a numerical integration a growing intumescence into several sub-intumescences, each of which may be handled as being small and such conditioned that his equations are applicable for each sub-system.

“Will these results give all what has to be achieved? I am led to believe, the problem of river tides will be solved, if the water level rises slowly (as in the problem of slow floods), based on the law of velocity distribution, and the internal friction, following the governing equations. And do you think that your latest research, or § XIX added to your memoir, will replace the celerity formula of Lagrange and mine, to furnish the results on which we can count? Is it the proof of my future memoir relating to practical procedures, based on the material of your § XXIX after having devoted sufficient time to study it? Is there a way to determine the celerity *ω*0 and velocity *V* according to this §, but assuming first a uniform velocity distribution (as you do when determining the effect of streamline curvature), given that the effects of the coefficients accounting for this do hardly change the result? And, by employing the simplified celerity and velocity formulas, accounting thereby for wall friction and bottom slope, would there be some probability to solve approximately the two operational problems of river tides and floods? Would this involve a numerical and graphical approach of superimposed waves, following Partiot? That, my dear Professor, I submit for your consideration, as the only thing that I imagine to achieve solutions. Yours very affectionate dSV.”

In the above letter, dSV for the first time refers to a paper to be written in the future, which was finally published only after his death, see the Complimentary File on the 1887 paper published in the *Annales des Ponts et Chaussées*.

In Letter 3, dated February 2, 1873, dSV continues his input to the generalized solution of the SWEs. It reads: “My dear Professor, I hasten to tell you that what I found difficult yesterday I find easy today. Let's take the equations

 ,

 .

Recalling that for an indefinite current and given the values of *h* and *V* for *t*=0 and *s*=0, *s*=Δ*s*, *s*=2Δ*s*, *s*=3Δ*s* etc., one easily determines with the two equations all subsequent states, because if one considers *h*0′=(d*h*/d*s*)*s*=0, *h*1′=(d*h*/d*s*)*s*=Δ*s*, *h*2′=(d*h*/d*s*)*s*=2Δ*s*, *h*0=(*h*)*s*=0, *h*1=(*h*)*s*=Δ*s*, etc., the knowledge supposed to have at instant *t*=0, the values of *h*0, *h*1, *h*2, etc. lead to those of *h*′ based on the formulas of polygonal interpolation

 *h*0′=(*h*1−*h*0)/Δ*s*, *h*1′=(*h*2−*h*1)/Δ*s*, *h*2′=(*h*3−*h*2)/Δ*s*, etc.

This improves, even by using parabolic interpolations, to

*h*0′=[3(*h*1−*h*0)−(*h*2−*h*1)]/2Δ*x*, *h*1′=(*h*2−*h*0)/2Δ*x*,… *hn*−1′=(*hn*−*hn*−2)/2Δ*x*, *hn*′=[3(*hn*−*hn*−2)−(*hn*−1−*hn*−2)]/2Δ*x*,

and one similarly has *V*0′, *V*1′, *V*2′ based on *V*. Substituting these *h*′, *V*′ values for d*h*/d*t*, d*V*/d*t* into the equations, one finds d*h*/d*t*, d*V*/d*t* serving to obtain both *h* and *V* at *t*=Δ*t*, then at *t*=2Δ*t*, etc. based on analogous interpolation formulas as

 (*h*)*t*=0=*h*0, (*h*)*t*=Δ*t*=*h*1, (*h*)*t*=2Δ*t*=*h*2,

 (d*h*/d*t*)*t*=0=*h*0′, (d*h*/d*t*)*t*=Δ*t*=*h*1′, etc.

or using the polygonals

 *h*1=*h*0+*h*0′Δ*t*, *h*2=*h*1+*h*1′Δ*t*, etc.

or even better by using parabolic interpolation, resulting at first approximation in

 *h*1=*h*0+(1/2)[*h*0′+*h*1′ (1st approx.)]Δ*t*, *h*2=*h*0+[*h*1′(2nd approx.)]2Δ*t*.

The same procedure applies for *V*.”

“Thus, there is no difficulty for these transformations to determine an undefined flow by assuming the completely given initial state. However, suppose a current which runs into a lake whose level is constant. We will see that the difficulty is no greater. Indeed, if the initial state is completely given, it must be such that the nearest part of the mouth remains nearly horizontal so that *h* remains constant; if we substitute for d*h*/d*x*, d*V*/d*x*, in the two differential equations, their values taken from the knowledge of *h*, *V* for that portion, they had to be nearly equal to d*h*/d*t*=0. If you find for d*h*/d*t* an approximation based on the computations, and the values of d*h*/d*t* will give for *s*=0 numbers only slightly different from 0, one may correct the result so that you have (d*h*/d*t*)*s*=0=0. We will then use these first values of (d*h*/d*t*), and all those following for *s*=(3 or 4)Δ*s* as in the previous case. We then have the state of the current at *t*=Δ*t*, deduced identically as we did for the states at *t*=2Δ*t*, 3Δ*t*, etc. in doing each time the same small corrections for the points located close to the river mouth so that the value (d*h*/d*t*)*s*=0 is obtained at any time *t*. This done, we will use these first values of (d*h*/d*t*), and all those that follow for *s*=(3 or 4)Δ*s* as previously. With the given state at *t*=Δ*t*, we deduce analogously its states at *t*=2Δ*t*, 3Δ*t*, etc. by doing each time, if required, these small corrections for the points located close to the river mouth such that (d*h*/d*t*)*s*=0 will be known at any time *t*.”

“Instead of a mouth at fixed level, take that of variable level following (*h*)*s*=0=f(*t*) such as the sine law of the tide. The difficulty will be no larger. Indeed, if one knows well and completely the initial state of the current, it should be such that in the part closest to the mouth, the values of *h*, *V*, d*h*/d*x*, d*V*/d*x* substituted in the differential equations will give (d*h*/d*t*)*s*=0= f′(*t*)=f′(0), or the known values of the tide will increase the velocity at a certain instant. If the equations result in a slightly different value of f′(0), correct it, along with small corrections for the numerically found values of (d*h*/d*t*)*s*=Δ*s* and (d*h*/d*t*)*s*=2Δ*s*, with Δ*s* sufficiently small at the start to guarantee continuity, and smoothness with the following values (d*h*/d*t*)*s*=3Δ*s*, which are left as found from the numerical computations. By this procedure, and by doing small corrections, if required, for the following instants *t*=2Δ*t*, 3Δ*t*, etc., requiring (d*h*/d*t*)*t*=2Δ*t*, 3Δ*t*= f′(2Δ*t*, 3Δ*t*), one obtains the successive states of the tidal wave. It will be its states responding to a given initial state. They will neither be the periodic standing nor the regulated states, in fact rather an expansion of the initial state as established within a few days. However, this state (always requiring enough time and work of office workers) will result in extending the computation for several periods, until we find an initial state within 12 hours and 24 minutes [Sic.: in agreement with the currently adopted period in Southern France].”

“If the first initial state corresponds to the low tide, as the sea remains for some time stagnant at that point of minimum, the solution during the first moments will be identical to that of the river discharging into a lake. In the following moments, (d*h*/d*t*)*s*=0 will take its value and we operate as I just said. For that initial moment, we can suppose uniform motion, or stagnant water. Moreover, instead of correcting as I said for the successive computational results, i.e. to return them exactly to the given law (d*h*/d*t*)*s*=0, it seems more rational to correct the data during the initial state close to the river mouth by using calculations in advance, such that this condition is satisfied, and then do computations without successive corrections. In all problems involving approximations, one is even obliged to correct a part of these to render them compatible with each other.”

“The problem of the fluvial tide thus does no longer seem to be a subject of theoretical difficulty. This does not prevent me to always think of solving the other process which I explained to you in my letter of Jan. 30th, because it would probably be a more expeditious process to compute first by using your formulas, and accounting for the bottom slope and friction; the remainder of the values of the wave celerity and the advance of the decreasing heights of an intumescence are produced on the river by raising (d*h*/d*t*)*s*=0Δ*t* of the sea at the river mouth. This first intumescence would then be overtaken by the second wave, which would be covered by a third, etc. We should have time to compare the results of these two processes with an example. This is my kind of patience, perhaps also yours, yet, certainly, the time left for me in this world is too short. Floods embarrass me more than tides. In this respect, one does not have *h*=f(*t*) at a given point, but (*hV*)=F(*t*), and [*h*(d*V*/d*t*)+*V*(d*h*/d*t*)]*s*=0=F′(*t*). Can we obtain a numerical solution as for *a*(*h*)=F(*t*)? Perhaps it would be as easy as for the tide.”

“There is a problem related to the successive states of a watercourse interrupted by a closing lock. If closure is abrupt no solution emerges, except it is supposed to be gradual. One should solve the problem at section AB by gradually reducing its width to finally clog. It is a special case of variable cross-sectional flow (Fig. 17).”

**Fig. 17.** Effect of local flow contraction at section AB (caption provided by the authors)

“I hope to send you your memoir that you will be glad to have for eight days to change some parts. I engaged you some time ago, to change the start of the § related to the analysis of streamline curvature in assuming all velocities equal to *V*, so that the reader is not forced to study painfully and at length, as I did, the previous § on the considerations that you abandoned as too complicated, and thus finds in this § what he has to know. However, if the draft of this amendment is not ready when you receive your manuscript, you may join it at the time of printing. Yours very affectionate dSV.”

In Letter 25, dated July 10, 1875, JB readdresses the question of the velocity correction coefficients as follows: “Sir and dear master, the discovery I made, subsequent to your report, that *β*=2(*α*−1−*η*) does not change anything, as said by you or that would not have previously been in my memory. There exists, it is true, in addition to the *α*′ in my equation and the *α* by Coriolis, the following relation

 *α*′ = 1+(5/3)(*α*−1) or, more accurately, *α*′ = 1+(2−1/2.925)(*α*−1).

I do not recall that we ever stated the contrary. We have clearly noted that, *β*=3.85*η* and (1+*η*+*β*)=*α*′=1+4.1*η*, whereas Coriolis’ *α* is nearly equal to 1+3*η*. This does not exclude that *α*′ takes nearly the value 1.1, which is attributed to *α* by many engineers, setting *α* somewhat too large: That’s all! It is *α*′, and not *α*, which is close to 1.1. One should however add to the errata on p. 113, line 21, instead of «extensively» only «partially».” [Sic.: JB here refers to the Errata in [34]].

“By the way, I recall that in my previously announced letter, you find the numerical value 0.84 for the average ratio of the cross-sectional average to the maximum superficial velocities (p. 87, line 11) a bit large. However, I see a large number of values in the tables of Mr. Bazin that are superior to 0.84. Thus, on p. 309 [Sic.: of [10]], in the experiments on Suze River, this ratio varied from 0.836 to 0.903; in the experiments on the Marseille Canal at the Oran Aqueduct bridge, p. 355, it varied from 0.802 to 0.890; p. 358, from 0.796 to 0.831; p. 377 from 0.795 to 0.849; p. 384, from 0.805 to 0.838, etc. There are, admittedly, some rare sets of data where the ratio is on average hardly more than 0.75; but these relate only to experiences made in a water course with extremely resistant walls and minimum flow depths. Small values of less than 0.84 in the first or second digits at most correspond also to small depths. I do not think that it is possible to give a general average better acceptable than 0.84, especially in view of the river shallowness. However, it is because we took this ratio of the average velocity to the maximum velocity to be too small, resulting in a too large value of *α*, attributed (in my opinion) to *α* which effectively should be *α*′.”

“From one end of my memoir to the other, I consider fluids as heterogeneous from the point of view of the coefficient of internal friction *ε*; but this is not exactly what I call roughness of a fluid. At the few places where I discuss heterogeneous fluids (p. 370-374 related to the energy evaluation of a complex oscillatory movement), I talk about heterogeneous liquids in terms of density, for it might be necessary to assume, when breaking waves are studied, that the sea water density slightly increases down to the sea bottom, this due to the huge compression.”

“I send you your draft report including marginal additions. Thank you very much to accept this new trouble for their insertion. Only, since you are talking of diffusion, I judge that this refers to my memoir of 1868; I want to replace this by the improved draft added on the slip attached. You may stick it onto p. 2 so that it covers exactly the old version (from the paragraph reproduced at the top of the page). Would you also, please, paste at the bottom of p. 9, at the end of the addition related to periodic waves, the small addition of 20 lines also attached. It is intended to show that long waves are the most viable in the deep portion, whilst short waves are, in turn, at small depths. This obviously demonstrates, that both internal friction and friction from the bottom must be account for in the assessment of the wave extinction coefficient; each of these is accounted for in one or two cases considered at the end of this addition.”

“I hope that you find the division of the addition into two parts satisfactory. I wished to insert these in § XXXIX: the first, much shorter part, should remain in § XXXIX, and the second part should be attached to the errata. If you fear difficulties in printing the footnote at the end of No. 221, you could subsequently just stick it onto the erratum. In addition, you could remove the first two small additions (one to § XXXVII, the other to § XXXVIII, which would probably arrive too late at the printer). So, what remains is too small for the headmaster to make a case of it. I do not understand what you mean if there is place to change the titles of these proof sheets. It seemed to me that these slips had no titles. But, do what you deem suitable about these titles. What you feel appropriate will be the best. I wish that you could leave as a result of the 10 pages of major additions the two pages that include the errata, and will appear with the title changes (only accepted to printing what you have written to me). The name does not matter, but these two pages of changes and clarifications do not seem to be displaced as a result of additions, since they will be found naturally also at the end of the memoir, where they have to be. There is no need to send them back to me; I have no more changes. If I would discover later misprints or other errors, I will add them at the time when they send me the galley proofs.” JB then continues to clarify details in solving partial differential equations, yet these are not in focus here.

The letters exchanged until the end of 1875 do not contain topics of hydraulics. In Letter 1, dated January 23, 1876, however, JB returns to this topic that is related to the contribution of Airy on waves. In Letter 14, dated May 3, 1876, dSV comes back to the topic of SWEs. It reads: “Dear Sir, you may recall better than me (because I do not find the letter) the serious objection of Mr. Partiot against my theory of fluvial tides published in 1871? You assigned the difference between his theory (which you presented since then, that Airy’s 2nd approximation was consistent, and that you presented in a different way) and mine, in which I exclude the effect of streamline curvature. Yet, since you found the same results by a different approach, I see that you added this research in the chapter of long waves whose curvature is negligible. Also, by considering the effects of bottom slope, friction, and non-constancy of the velocity along a vertical within a section, you added to this theory essential changes. Well, doesn’t the objection of Mr. Partiot substitute this, at least to a good part, despite these changes? Or, better said, doesn’t your theory, the most comprehensive currently available, only apply to tides, you could almost say tides in the Mediterranean, involving small wave heights compared to the average flow depths? You told me, it is true, that the formulas seem to continue to be strongly approached to tides for which BH [Sic.: Fig. 18], or the total height of the tide, match BF, or the height of the low water level above the river bottom. (Is it this what you said and you think?)”

**Fig. 18.** Sketch illustrating the effect of tidal height on flow movement (caption provided by the authors)

“However, there are tides, even in the average state, for which BH equals several times BF, and this takes place especially under turbulent flows, where it matters most to know the laws of motion, because the ebb then exercises more action to regroove the river in changes of submersible contractions forming passages for ships under large flow depth. Well, must we go back to the problem of high river tides? It seems to me that this problem is posed, and mathematically formulated. The equation is not analytically solvable, i.e. cannot be integrated analytically, not even by successive approximations, meaning that we cannot, in dropping first nonlinear terms that prevent the analytical integration, restore them and find the first higher order approximation based on this simplification. All evidence of successive approximation assumes that the terms removed first are small compared to the others. If these terms preventing the integration are large, however, their removal followed by their recovery would only result in a divergent solution or it would deviate more and more from the real solution.”

“However, if approximate solutions are not possible to obtain, don’t we have, for any problem posed in equation form, explicit or implicit solutions applicable to the numerical systems? Can’t we replace the ‘d’ by some Δ of finite size, go first from the initial state to a state that is moderately different, then take this state for initiation, and go thus gradually but not in the manner of successive analytical approximations, but numerically or graphically, with time and patience, by using graph paper, or calculation work? Isn’t this what you have suggested for the earth pressure in your Note resulting in the translation of Rankine’s memoir, and what I made at the end of my Note? Or, isn’t this something like in astronomy under certain circumstances where we do not have this missing information, for example in cases when the analytical solution of a question would require the impossible solution to the three-body problem? I submit this to your consideration though I do not see as great a possibility that you could write a new addition to your memoir of flowing waters. Yours very affectionate dSV.”

It is too obvious that dSV tried [Sic.: in vain] another time to push JB to find a solution to the integration of the full SWEs. In Letter 15, dated May 27, 1876, he again comes back to this point, stating: “Dear Sir, after many interruptions I want to complete my work on the unsteady wave motion and river tides, using your Memoir, your letters and developments. I intend to present your equation (392),

,

starting directly from the equation of unsteady motion taken in its simplest form, or by substituting 1 for the coefficients *α*′, *α*″, 1+2*η*, *f* etc. without introducing these as a result of non-uniform velocity distribution equal to their average *V* or *Vo* and the coefficient *b* in *bV*2 versus *H*, etc.”

“And even I would arrive at this eq. (392) describing tides or floods of low flow depth taking into account friction and the bottom slope without talking of wave propagation; so, there is *V*o+(*gH*)1/2 in this equation instead of *ω*o, and (*gi*/2*V*o)[(2(*gH*)1/2−*V*o)]/(*gH*)1/2 instead of f′.These simplifications seem to be the only way to accept the analysis of engineers who do not indulge the same long work as me. In the sheet attached are my calculations. I obtained several ideas, disagreeing with one another, however. So, I recourse to you, and ask you to set comments in the margins that you might have about the causes of these inconsistencies, and required certifications.”

“I have not yet studied your means to resume by digital error the problem of high fluvial tides, but I thank you. I can see that practice will be difficult, but I am not as scared as you are by the length of the calculations: It is for engineering work, the case of employees, and a routine, perhaps partly graphical and operated with graphical paper. Similarly, I unfortunately find your solution expressed by an integral that you qualify as complicated, relatively straightforward and very elegant. Yours well affectionate dSV.”

In Letter 19, dated July 10, 1876, dSV returns to his mission, stating: “Dear Sir, I do not see why you will not solve with equal ease, but with lots of complications, the case of tides propagating over a uniform slope of a tidal river (of small height) where the motion would be steady despite its uniform bottom slope, i.e. in a river or prismatic rectangular channel in which the water surface would be bounded, not by the periodic tidal movement at the river mouth, but in a river controlled by a dam. In this way, the general case of tides controlled by backwater would be embraced, which seems necessary. No doubt, to inhibit that the water volume does not increase indefinitely in time over a wave period, there would always be a medium change to *H* =*a*[1+(3/8)*a*′]; but there would not be an obliged advance, it seems to me, because the data must be independent. If you develop this clearly, I do not see a reason for not inserting it either in the same volume, or in volume 24 of the Additions and clarifications. I would write that it would be necessary, because this cleverly determined solution seems to me, I fear, will not be applicable. Yours very affectionate dSV.”

In Letter 31, dated Dec. 20, 1876, JB comes back to the problem of waves, stating: “Sir and dear Master, I might, when you would be in Paris on January 8, send you a manuscript of a dozen pages, and would ask you to recommend it to Mr. Résal for insertion in the *Journal de mathématiques*. Its central part is concerned with the regime of gradually varied fluid flow where movements are continuous, and to waves affected by friction, if motions are continuous and the velocity is zero at the bottom, varying rapidly in a layer of extremely small thickness up to a layer parallel and very close to the bottom, where the velocity is roughly as if the external friction were absent. I recognized that, in this case, it is not completely correct to assume the external friction per unit area to be equal to the product of a constant coefficient *ε* times the velocity *uo* produced at a small distance from the bottom, as I admitted in my second addition of the *Essai* where I tried to account for this frictional effect. I have dealt with the problem in a general and exact way, recognizing that the coefficient *ε* makes invalid what I said at the end of the Addition relative to the Theory of flowing waters. In this, I admitted *ε* to be constant since, for a small flow depth for which *ω*=(*gH*)1/2 and *L*=*T*(*gH*)1/2, the wave extinction coefficient *α*=*ε*1/(2*ρH*) becomes *α*=1/(2*lH*)[(π*lε*/(8*lL*)(*g*/*H*)1/2]1/2 instead of being independent of *L*. This coefficient then varies inversely with the square root of the wave length, while at greater depths, it would be *α*=2π2*ε*/(*ρLo*2). The issue of gradually varied flow is only of purely academic interest, because I have shown more than a year ago, that gradually varied flow is quasi-uniform, so that we can simply apply with sufficient accuracy the formula of the uniform regime *RI*/*V* 2= const. I did this in my Addition to the *Essai* as well as in the large two Notes of the main memoir ‘On the phenomena of filtration’. However, the gradually varied regime for smooth flows offers no less an interesting feature, consisting of two mistakes originating from opposite directions, committed by Coriolis, which are exactly neutralizing each other. The exact equation of the supposed permanent flow is *I*=(external friction)/*R*+*α*d(*V* 2/2*g*)/d*s*. Yours respectfully and devoted disciple JB.”

In Letter 10, dated May 10, 1880, JB returns to the SWEs, stating after having detailed similitudes between the mechanics of terrestrial and celestial problems: “There is much more parity between celestial and terrestrial mechanics than seems at first. The only difference that I found (and again, this is not absolute and relative to our era), is that the dominant phenomenon is more obvious in the planetary system than it is in terrestrial mechanics accessible to us. In the planetary system, the predominant action of the Sun is natural and sufficiently evident, as soon as one inquires the relative sizes of the various bodies of the system. In translation waves propagated in water at rest, this character involves the major motions in the horizontal direction under almost negligible friction. In gradually varied flows, this character is to realize that the mutual streamline inclinations are small enough to have no impact on the velocity distribution at various points of a section; and their curvatures are quite insensitive and even have less influence. However, all my efforts were to clear these properties, which should not be easy, because others had failed; to see the starting point of a series of approximations, as well as in the mechanics of the planetary system, the dominant action of the Sun provides a starting point for a study by successive approximation of its motion. So, I got the idea to apply this method to problems that you had not noticed but could be applicable to them and indeed, I realized this idea. That's all of the credit which I deserve. However, I see no fundamental difference in this respect between celestial and terrestrial mechanics. Moreover, celestial mechanics also neither lacks complications in its own way.”

**Saint Venant’s 1887 paper**

It was since 1872 that dSV wished to publish, as expressed in the preface to [31], the governing equations of gradually varied open channel flows. These should be the basis for the solution of problems dealt with both in theory and practice. Of relevance in this problem is the description of the internal friction, which should be more general than proposed by Navier [1], who assumed a constant value for viscosity. Starting in 1843, and continuing in the 1850s, dSV had tried to advance the enigma of ‘vortex agitation’, as expressed by him, today’s question of fluid turbulence. The experimental and theoretical findings of Darcy and Bazin [10] and Boussinesq [31] are then mentioned as the first having provided evidence of the relevance of fluid turbulence.

After a review of the then available expressions dealing with gradually varied flows, including those of Bélanger [17], Vauthier [30] and Coriolis [18], of Bresse [19] and Boussinesq [31], as explained in the Correspondence, a simplified equation is proposed in which the corrective coefficients are set equal either to unity, or zero, to simplify computations. As to the internal friction, the local ‘turbulent agitation’ has to be considered. The proposal of JB is adopted, according to whom its intensity increases as the flow boundaries are approached. After long computations, dSV finally proposes for steady flows the equation

 .

Here, *I=i*−(d*h*/d*x*) is the difference between the bottom slope *i* and the free surface slope, *χ* is the wetted perimeter, *ω* the cross-sectional flow area, thus *ω*/*χ* the hydraulic radius, *Fu*/(*ρg*) the average bottom friction expressed either according to Prony [13] or Darcy-Bazin [10] for uniform flow, *x* is the streamwise coordinate, *V* the cross-sectional average velocity and *g* the gravity acceleration. The constant *α=*1.111 is stated to be an average between the two extremes 1.0851 for the wide rectangular, and 1.1380 for the semi-circular sections. Clearly, the above statement is based on the principle of energy conservation. At this stage, it has to be emphasized that the paper considered had been brought to its final form by JB, based on various fragments sent to him by dSV, who felt unable to submit a final version due to his advanced age. The name of Boussinesq, though, is never stated in the long paper, given that JB completed this long project as a colleague of his Master, as explained above in the Correspondence. When comparing the above equation for steady gradually varied flows with the current formulation, nothing has really changed, except for the adoption of the so-called friction slope *Sf*= *Fu*/(*ρg*), and usually by setting *α*=1, resulting with *So*=*i* as the bottom slope and *H*=*h*+*V*2/(2*g*) as the energy head in

 , or .

**Kleitz and the SWEs**

Once JB [31] had published his *Essai*, Kleitz [37] published a paper in which the SWEs are described. Kleitz even required priority of his equations as the true SWEs. In Letter 14 to JB, dated December 31, 1873, dSV writes: “Dear Sir, in the supplement to your Essay, related to flood flows based on the rough hypothesis *σ*=f(*q*, *S*) [Sic.: Cross-sectional area depends only on the unit discharge and the bottom slope], this will certainly be one of the most useful parts of your work, I urge you to provide as clearly as possible your thinking, and your equations. Otherwise you will not be understood and engineers will close your book.”

“You say in your two letters of Nov. 13 and Dec. 27, 1873 [Sic.: Both are not contained in the Correspondence] that the integral of f′(*Q*, *S*)d*Q*/d*t*+d*Q*/d*s*=0 is *t*−∫0*s* f′(*Q*, *S*)d*s*=*ϕ*(*Q*). I searched in Navier’s lecture notes (he, who talked about it more clearly and that it is a good model), in Duhamel, and in my notes of Ampere’s course held in 1815, how the known integration process can lead to this, and I did not find it. Do not give the integral without examining the process which leads to it, or at least, without checking by differentiation and elimination of the arbitrary function, that it agrees with the differential equation. I couldn't be sure with a particular function, as f′(*Q*, *S*) =*AQ*2+*BQS*+*CS*2. Then I recognized that there are joint locations of derivative products and that your assertion was correct. But I had the patience, others won't have it. I never saw in the solution of a differential equation that in addition of a full *S* taken without varying a function *Q*(*S*). Ask for an explanation, because of the novelty.”

“There are other passages of your letter of the 27th that I don't understand. What is this speed *k* that if *t* increases by d*t*, and *s* by *k*d*t*, the function *Q*(*S*, *t*) does not vary? Is this calculated speed equal to the celerity of the flood? Is *k* the speed of the main wave constituting a flood?”

“You do well to directly prove the formula *k*=d*Q*/d*σ* of Mr. Graeff rendering it more acceptable to his reasoning, or substituting another case, showing that this formula is not as general as d*σ*/d*t*+d*Q*/d*s*=0, but that it is essentially founded on the assumption that the discharge *Q* is a function of the cross-sectional area *σ* at each point. What does his ‘propagation velocity’ of a flood element mean?”

After discussing other problems, dSV returns to the above topic: “But would you be so kind to do for me a job that I intended to do based on your new findings on the flood. It is to apply the relation *t*−∫0*s* f′(*q*, *σ*)d*s* = *ϕ*(*q*) for a particular example, to finally determine the law of the floods. Also, like I did on page 5 of my Note of July 17, 1871, on the ‘Non-permanent movement’, to assume that an inflow of water *Q* provided by a brook into a river leads to the start of a flood slowly reaching sinusoidally a maximum *q*, so at point *s*=0 one had *Q*=*q*sin(π*t*/*T*). Adopting the above equation *σ*=*KQ*2/3 gives *t*=(*T*/π)arcsin(*Q*/*q*).”

“Well, when successively giving values of *Q*=0, 1, 2, *q*minimals=3, 2, 1, 0, one determines the values of corresponding times *t* resulting for all *t* in linear values of *S*. One could also determine the propagation of a flood, whether it decreases while propagating. You do well to prepare a sketch to ease the understanding of the relevant processes by the graphical representation (you may buy at Lille ‘gehäuseltes’ [Sic.: checkered] paper, which is inexpensive). Then, you determine the propagation velocity of the wave crest which, it seems to me, had to be a distorted sinusoidal. Would the speed not be equal to *k*? This work would certainly elucidate lots of things. It is true that this should be my duty, because this is a work of enlightenment and development as I did following my Reports made for you, for Lévy, Lucas, Partiot and Kleitz. Well, this is precisely because I think that I ask you, Sir and good friend, to undertake this project for me.”

“Does the formula of Graeff, either *h*′ or d*s*/d*t*=d*Q*/d*σ* suffice, with or without that of Kleitz, to determine the propagation speed of wave crests, to predict the arrival time at a certain point, if the values of d*Q*/d*σ* have been observed at various locations along a river? For a tidal river, it is not the discharge *Q* given as a function of time *t* at a certain location *s*=0. In turn, it is the cross-sectional area *σ* given at the river mouth versus time *t*. And, if the river is rectangular-shaped, *σ* is proportional to the flow depth denoted *y* on page 1 of my Report in the Memoir of Mr. Partiot. With *a* as the river width, *h* the value of *y* at *t*=0 or the depth of the sea at low ebb, and *H* the tidal height, so that *σ*(*S*=0) = *ay*=*ah*+*aH* against π*t*/*T*, with *T*=12 hours or ≅42,300 s. For given *σ*=f(*t*), from which results *t*=F(*σ*), I think that the equation you posed is integrated as easily as if you know *Q*=f(*t*), and you will straightforwardly solve either numerically or graphically, using a sufficiently approximated approach, the problem of tidal waves, thereby satisfying Mr. Partiot and the other experimenters. Your Memoir on this subject would certainly be worthy a report to the Academy. However, because I will not be able to write it, you better submit a Note of four pages to the *Comptes-rendus*. This will not exclude to submit a full paper prepared for the *Savants étrangers*, given that it would constitute a portion of accepted work.”

“We are highly sensitive to your wishes and in favor for you, Madame Boussinesq and your dearest, Yours highly affected dSV.”

An answer of JB on this letter is not available, so that no further explanations to this topic can be given.

**Short biographical indications**

Names of main persons mentioned in the Correspondence, together with life years and fields of work are mentioned below.

Airy, George Biddell, 1801-1892, Astronomer Royal, Greenwich UK

Bazin, Henry-Emile, 1829-1917, French hydraulic experimenter

Bélanger, Jean-Baptiste, 1790-1874, French hydraulician

Bernoulli, Daniel, 1700-1782, Swiss physicist and mathematician

Boileau, Pierre, 1811-1891, French mechanical engineer

Borda, Jean-Charles, 1733-1799, French hydraulician

Boudin, Emmanuel Joseph, 1820-1893, Professor of hydraulics at Ghent

Bresse, Charles, 1822-1883, French hydraulician

Caligny, Anatole, 1811-1892, French hydraulic engineer

Cauchy, Augustin Louis, 1789-1857, French mathematician

Clebsch, Rudolf Friedrich Alfred, 1833-1872, German mathematician/scientist

Collignon, Edouard, 1831-1913, French hydraulician

Coriolis, Gaspard-Gustave, 1792-1843, French engineer

Coulomb, Charles, 1736-1806, French hydraulician

d’Alembert, Jean-Baptiste le Rond, 1717-1783, French physicist

Darboux, Gaston, 1842-1917, French mathematician

Darcy, Henry, 1803-1858, French hydraulician

Du Buat, Pierre, 1734-1809, French hydraulician

Dupuit, Jules, 1804-1866, French hydraulician

Euler, Leonhard, 1707-1783, Swiss mathematician and physicist

Eytelwein, Johann Albert, 1764-1848, German hydraulician

Fourier, Jean Baptiste Joseph, 1768-1830, French mathematician

Gauckler, Philippe, 1826-1905, French hydraulician

Gerstner, Franz-Joseph, 1756-1832, Austrian mechanical engineer

Girard, Pierre, 1765-1836, French hydraulician

Graeff, Auguste, 1812-1884, French hydraulic engineer

Hagenbach, Eduard, 1833-1910, Swiss physicist

Kelland, Philip, 1808-1879, British mathematician

Kleitz, Charles 1808-1886, French hydraulician

Lagrange, Joseph-Louis, 1736-1813, French mathematician

Lahmeyer, Johann Wilhelm, 1818-1859, German hydraulician

Laplace, Pierre-Simon de, 1749-1827, French mathematician

Lesbros, Joseph, 1790-1860, French hydraulic engineer

Lévy, Maurice 1838-1910, French engineer

Liouville, Joseph, 1809-1882, French mathematician, editor of the *Journal de Liouville*

Navier, Henri, 1785-1836, French hydraulician

Partiot, Henri-Léon, 1825-1904, French hydraulician

Phillips, Edouard, 1821-1889, French mathematician and mining engineer

Piobert, Guillaume, 1793-1871, French engineer and scientist

Poiseuille, Jean, 1797-1869, French engineer and medical doctor

Poisson, Siméon-Denis, 1781-1840, French mathematician

Poncelet, Jean-Victor, 1788-1867, French civil and mechanical engineer

Prony, Gaspard Riche de, 1755-1839, French hydraulic engineer

Rankine, William John M., 1820-1872, British civil and mechanical engineer

Résal, Aimé-Henry, 1828-1896, French engineer

Russell, John Scott, 1808-1882, British naval engineer

Stapfer, Charles Louis, 1799-1880, Swiss engineer in Paris

Stokes, George Gabriel, 1819-1903, British physicist

Tadini, Antonio, 1754-1830, Italian mathematician and hydraulician

Vauthier, Pierre, 1784-1847, French hydraulician and engineer