Spatially varied flow in open channels

J. A. FOX & I. M. GOODWILL

Dr A. van der Beken, Research Engineer (NFWO), Laboratory of Hydraulics, University of Ghent

The Authors present an interesting attempt to solve some difficult problems of spatially varied flow. Since I did much of the same research^{13,14} which was initiated for exactly the same reasons as mentioned by the Authors, a discussion will be most valuable.

45. Concerning the theory, presented by the Authors in §§ 4-8, I would like to refer to recent papers by Strelkoff¹⁵ and Yen and Wenzel¹⁶ which discuss the intrinsic difference between the momentum and energy approaches in deriving the general equation for spatially varied flow. It is not clear how the third term $(Vq/gA)(\beta+1)$ at the right hand side in equation (1) is obtained, unless an approach similar to that given by Keulegan¹⁷ is followed. To Keulegan the term Vq/A represents the frictional effect due to the mixing of the additive masses with the moving current. This effect is equivalent to an apparent stress and therefore the term is put on the right hand side of the momentum equation as a true external force. In this way, the velocity correction coefficient β does not apply to this term. However, Keulegan states immediately that in a rigorous derivation another correction coefficient β' , different from β , should be applied to this term, since the inflowing water (i.e. the falling rain) causes a disturbance of the existing velocity distribution.

46. The third term would be written then $(Vq/Ag)(\beta+\beta')$. But later in the same paper, Keulegan prefers to relate this disturbance to the roughness coefficient λ and writes (without referring to measurement) that the falling rain accentuates the velocity gradient near the bottom. It is this latter approach that has been followed by the Authors when discussing the channel roughness (§§ 33-38). I found no apparent increase in wall roughness. It should be recognized that an appropriate value of β (or β') could as well take into account the perturbation caused by the lateral inflow. Moreover, since secondary currents can develop, pressure correction coefficients should also be considered. The Authors' conclusion in § 38 certainly deserves attention.

47. A valuable contribution has been made in analysing the water profiles in spatially varied flow (§§ 9-32).¹⁸⁻²¹ In my opinion a complete systematic analysis of all possible water profiles is still missing in the literature.

48. Before commenting further, a brief description of my own research is necessary. A slope-adjustable drain made of plexiglass pipe with an inner diameter $d_0 = 5$ cm was installed over a total length of 12 m. Any flow could be regulated by the upstream and downstream reservoirs. A constant lateral inflow was introduced every 50 cm at the top of the drain through a small vertical pipe, surmounted by a funnel. This inflow was diverted along the wall of the drain. By analysing the observed water profiles for slopes $S = 0.2; 0.5$ and 1% and lateral inflow rates $q = 3; 5; 10$ and $20 \text{ ml s}^{-1} \text{ m}^{-1}$ it was found that the water surface slope dd/dx was negligible over the main part of the total flow length. It was concluded that for practical design purposes equation (6) could be used. A very appealing expression 'energy balance line' was introduced by the Authors and explained in § 14. In fact, the proposed procedure is completely equivalent to that used for uniform flow computation. By substituting:

$$\frac{\tau P}{wA} = \frac{\lambda}{4m} \frac{V^2}{2g} \quad \dots \dots \dots (19)$$

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into equation (6), the solution for V is:

$$V = \frac{-8qm + \sqrt{(64q^2m^2 + 8\lambda gmA^2S)}}{\lambda A} \quad (20)$$

A set of design charts based upon this equation have been prepared for different types of drains and gutters.²²

49. The Authors mention in § 16 the occurrence of, what they term, 'a hydraulic rise' which can only occur when the critical depth line is close to but below the energy balance line. This phenomenon is discussed later in §§ 30–32 and shown in Figs 4 and 8. Apparently there is a contradiction between the description given and the statement in § 30 that in certain cases a hydraulic jump had been expected. Furthermore, I suggest that the hydraulic rise shown in Figs 4 and 8 can be simply described as a well-known backwater profile A_1 at a mild bottom slope, tangent to the horizontal, when the downstream end of the channel is submerged to a greater depth than the normal depth. Indeed, the slope of the rise shown in Fig. 8 is near 0.02.

50. Two examples of these backwater profiles A_1 in spatially varied flow are given in Fig. 14. These profiles were observed each time the experimental drain, lying at a mild bottom slope, was running full downstream. At this point there was a relatively smooth transition from free-surface flow to full-pipe flow and the observed parabolic pressure line at full-pipe conditions was simply a smooth extension of the backwater profile. At a steep slope, however, a fully developed hydraulic jump could occur, extending directly downstream towards the pressure line for full-pipe conditions.

Fig. 14. Examples of backwater profiles A_1 at a mild bottom slope S_0 from the study of spatially varied flow in a drain pipe

Dr C. R. Robinson, Skefco Ball Bearing Company Limited

A question arises on the series of hydraulic jumps and it is suggested that the Paper shows why such a series cannot exist, except where there is a large variation of lateral inflow with length. Two approaches are proposed to surmount the absence of a second boundary condition and both implicitly assume an individual jump. Is it possible that the use of the assumptions presupposes that the series will not exist? An argument is put forward, based on Fig. 5 and a conclusion is drawn. Is not Fig. 5 itself based on the very point which the Authors wish to prove?

51. In any full-scale solution to a spatially varied flow problem the physical parameters will inevitably be assumed to vary continuously with depth. It should, therefore, not be taken as a restriction on the value of the Paper that the Authors have made a simplifying assumption of this type. In fact, in prototype cases it is likely to be assumed that physical parameters vary linearly with horizontal displacement—a rather easier condition.

52. The finite difference solution of the unsteady flow equations is another aspect which has received a great deal of attention and it has been developed into a powerful tool. It does, however, suffer from the plague of instability.

53. Fox²³ mentions that he can give no guarantee of immunity from instability in this type of solution. Am I correct in thinking that the Authors' indication of how to avoid instability only applies to this case of supercritical flow?

54. Also on the stability question, the Authors caused me some amusement by their assertion that 'the integration process tended to converge to the correct solution regardless of the initial approximation'. I can think of a number of problems where that would be a distinct advantage. The situation suggests to me that, for integration from the upstream end, the technique is highly stable. Can the Authors put forward any reasons for this effect as opposed to the more usual case of concern over instability?

55. Figures 6, 7 and 8 show how three sets of results compare in theory and practice. This accuracy of result would not be expected throughout a series of experiments although it must be remembered that some proximity is achieved by assuming values for the momentum coefficient and friction expressions which give the right order of answer. Could the Authors show the variation obtained in their results and say whether consistent momentum and friction effects could be used throughout?

56. The investigation was prompted by the development of plastic pipes as replacements for land tiles. In the section dealing with experimental apparatus the channel section used is shown to be a narrow rectangle. This is basically the equipment used by White²⁴ but with a variable flow with length facility added.

57. The majority of experimental studies at this level of complexity are by necessity carried out using rectangular channels. It seems logical to assume that, if a channel theory is going to work at all then it will certainly work for the rectangular case. It is not so logical to assume that extension to other shapes will follow automatically. The Paper does not make clear just how extensive the application of this particular theory can be. I would refer the Authors to their comment that the correlation between the theoretical and experimental curves is good. This, they say *fully* verifies the theoretical approach finally adopted. My own view is that, while I agree that the correlations shown in the Paper are excellent, I must dispute that full verification is therefore achieved.

58. In asking for comment on this point, in view of the Authors' opening remarks would they be prepared, without reservation, to claim proof for the case of a circular pipe with a free surface condition?

59. Further investigation is needed into the question of roughness. The difference between the invert and wall roughness, accentuated when the inflow is through the walls, is likely to be of no great relevance as long as an average figure can be estimated. This is possible in a research project where one can vary the prevailing conditions as an aid to estimation, but is not conceivable in practice. It is for the researchers to

supply suitable coefficients to make their theories available to the practising engineer. It must be accepted, however, that such values will not follow immediately behind the first production of the results of research. Am I correct in believing that the modification to the Powell formula will become more complex as the channel width increases relative to the water depth?

60. With the increasing use of electronic digital computers in design offices this type of analysis will become invaluable for greater understanding and realistic design.

Mr D. J. Phelps and Dr A. A. Smith, McMaster University, Hamilton, Canada

As implied in the Author's introduction, interest in the problem of spatially augmented channel flow has received some impetus in recent years due to the availability of computing facilities to handle the not inconsiderable task of numerical integration. Work has been done on just such a computational endeavour concerning the pattern of run-off from road surfaces as affected by discrete point or continuous line drainage systems.²⁵ The latter is, in practice, facilitated by the use of circular drain-block units and a not unnatural sequel to this work was a more fundamental study of the problem of spatially varied flow ($dQ/dx > 0$) in circular conduits. Initially this work was purely numerical but recently the existence of certain ambiguities in the mathematical solution made it desirable to mount an experimental programme to examine the phenomenon. In view of the Author's stated interest in pipe drainage systems, the following preliminary observations were made.

61. As pointed out by the Authors, an essential preliminary to profile integration is the location of the control section. Several methods suggest themselves including the solution of Keulegan's equation, the plotting of energy balance and critical depth lines and other variants such as those suggested by Smith.²⁶ All of these techniques are merely different ways of locating the singular point defined by a slightly modified form of the Authors' equation (6):

$$\frac{dd}{dx} = \frac{S - S_f - 2Qq/gA^2}{1 - Fr^2} = 0/0 \quad \dots \dots \dots (21)$$

Thus the distance x_0 is defined by the set of equations:

$$F(x) = S - S_f - 2Qq/gA^2 = 0 \quad \dots \dots \dots (22a)$$

$$\text{where:} \quad Q = q \cdot x \quad \dots \dots \dots (22b)$$

$$\text{and:} \quad Q^2 b/gA^3 = 1 \quad \dots \dots \dots (22c)$$

62. A polynomial in x is obtained only for certain cross-sectional shapes of simple geometry and in general a computational scheme²⁷ is necessary to treat cases of prismatic channels of arbitrary geometry with various forms of resistance law. A convenient presentation of the relationship between x_0 and the other variables is obtained by rewriting equation (22a) as:

$$S = f(x) = S_f + 2Qq/gA^2 \quad \dots \dots \dots (23)$$

63. The variables of lateral discharge, equivalent roughness height (if relevant) and distance may be expressed non-dimensionally as:

$$\bar{q} = q/\sqrt{gl}^{3/2} \quad \dots \dots \dots (24a)$$

$$\bar{k} = k/l \quad \dots \dots \dots (24b)$$

$$\bar{x} = x/l \quad \dots \dots \dots (24c)$$

where l is a characteristic dimension defining the cross section.

64. Examination of the right hand side of equation (23) shows it to consist of the sum of two energy dissipation terms, one associated with boundary friction, the other with the energy necessary to accelerate the lateral inflow. Since critical flow must be obtained at the singular point, it follows that equation (23) when evaluated for critical depth yields the critical slope.

65. Recalling the relationship between critical slope and transitional depth or discharge in non-spatially varied flow it is evident that the value of x_0 corresponds to a

Fig. 15

transitional stage in the profile. At this point in the argument, the concept of a limit slope in non-spatially varied flow is brought to mind and also the fact that one, two or even three transitional depths may exist, depending on the cross-sectional shape of the channel. Thus, stimulated, a quantitative examination of equation (23) shows a similar phenomenon and Fig. 15 illustrates the fact that for a rectangular channel there may exist two, one or zero control sections in a system of spatially varied flow. This is not to contradict the Authors' claim that only one critical section will exist in a given profile, but it suggests the interesting possibility of alternative profiles and the consequent instability which might result.

66. Thus Fig. 5 shows only one part of a rather interesting story for, if continued long enough, or plotted for a slightly flatter slope it would be seen that the energy balance line and critical depth line cross twice and not once as stated in § 17. This is true not only for rectangular channels but applies also to narrow trapezoidal cross sections and is of course of particular relevance to channels with converging sides such as part-full circular conduits.

67. Figures 16(a) and 16(b) show non-dimensional plots for a circular conduit, the former being after the style of Fig. 15 while Fig. 16(b) shows the energy balance and critical depth lines as used by the Authors.

68. We are presently engaged on an experimental programme to examine the ambiguity resulting from the circumstance of two possible control sections in spatially varied pipe flow and to quantify the criterion necessary for numerical prediction of the resulting profiles and jump positions.

69. Consideration of the transitional stage in spatially varied flow serves not only to point out the ambiguity discussed previously, but throws a little light on the phenomenon described by the Authors as a 'hydraulic rise'. It will be recalled that for non-spatially varied flow, the profile is described by the equation:

$$\frac{dd}{dx} = S \frac{1 - (Q/Q_0)^2}{1 - (Q/Q_{cr})^2} \quad \dots \dots \dots (25)$$

At the transitional depth or discharge:

$$Q_0 = Q_{cr} = Q_t \quad \dots \dots \dots (26)$$

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Fig. 16(a)

Fig. 16(b)

so that $dd/dx = S$. Thus, it is predicted theoretically and has been observed experimentally²⁸ that a profile passing through the transitional depth line does so with a horizontal water surface.

70. Now for spatially varied flow, in a similar way:

$$\frac{dd}{dx} = S \frac{1 - \left(\frac{Q}{Q_0}\right)^2 \left(1 + \frac{16m}{\lambda x}\right)}{1 - \left(\frac{Q}{Q_{cr}}\right)^2} \quad \dots \quad (27)$$

where $\lambda = 8 \cdot S_f g m / V^2$. Therefore, when passing through or close to the transitional depth line a profile of spatially varied flow will exhibit a near horizontal but slightly negative surface slope. Examination of Fig. 4 shows that over a significant distance the energy balance and critical depth lines are very close together, and consequently the transitional depth is not clearly defined. Raising of the downstream level results in a profile passing through the transitional depth with a near horizontal surface slope, a fact which is confirmed if Fig. 8 is replotted as a surface profile and not a depth profile. It would appear, therefore, that the 'hydraulic rise' is simply a near horizontal profile essentially equivalent to the profile of non-spatially varied flow which would result if the $M1$ or $S1$ profile is plotted (or observed) with a uniform flow Froude number of close to unity.

71. Mention has been made of the advantage of allowing complete generality of cross-sectional geometry in any computational routine. A similar need for flexibility exists in relation to the choice of resistance law and we have been concerned that in any computer program for the solution of problems in which flow resistance is significant, the capability should be provided of defining any functional form of resistance law. It has been suggested²⁹ that a convenient method of defining resistance equations is by the device of a normal discharge function or subroutine operating on a standard set of parameters. Assuming the roughness to be sufficiently described by a single variable, a convenient parameter list is given by the relationship:

$$Q = Q(A, P, S, \epsilon, g) \quad \dots \quad (28)$$

Here, ϵ represents any index of roughness be it a physical measure or a 'coefficient of ignorance' and indeed may be irrelevant for smooth turbulent laws. The inclusion of gravitational acceleration g serves to define the system of units employed.

72. It is of interest to see that the Authors chose to employ a variation of the Powell equation. This may be expressed as the normal discharge equation:

$$Q = -\left(\frac{42}{\sqrt{g}}\right) A \sqrt{S_f g m} \log_{10} \left[\frac{(\sqrt{g}/4) \cdot v}{\sqrt{S_f g \cdot m^{3/2}} + \frac{\epsilon}{m}} \right] \quad \dots \quad (29)$$

which is explicitly calculable by the parameter list of equation (28). A complication arises with the introduction of the additional term, since this involves V (or Q) and thus requires an iterative calculation.

73. Notwithstanding the Author's rational argument in § 36 it seems probable that the parameter $K \cdot u / \sqrt{sgm}$ could be employed with a similar degree of success eliminating the need for iteration in the calculation. The need to introduce an additional parameter would be avoided by utilizing an equivalent roughness height defined by:

$$\begin{aligned} \epsilon_{eq} &= \epsilon + K \cdot u \cdot m / V \\ \text{or:} \quad &= \epsilon + K \cdot u \sqrt{\frac{m}{s \cdot g}} \end{aligned}$$

74. Since typically the values of m/ϵ and V/u appear to be of the order of 20 and 200 respectively it would seem that the value of K should approach 10 if a significant increase in resistance is produced by the lateral inflow. Presumably, also if u is averaged over the side-wall perimeter, the value of K will depend on the proportion

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of wall area available for lateral inflow. It would be of interest to have further information on the Authors' findings on this point.

J. A. Fox

In reply I would first like to thank everyone who contributed to the discussion.

76. In §§ 45 and 49 **Dr Van der Beken** raised a query concerning the $(Vq/gA)(\beta + 1)$ term. The rate of change of momentum of the flow over a small element of length ∂x is given by:

$$F = \beta \rho A \left(V \frac{\partial V}{\partial x} + \frac{\partial V}{\partial t} \right) + \rho q \partial x (V - u)$$

where u is the longitudinal velocity of the water inflowing from the side. It will be seen that the first of these two terms quite correctly includes the β term. If the analysis is then fully developed the term $(Vq/gA)(\beta + 1)$ results. The β' term mentioned by Dr Van der Beken is interesting. The inflow in our case entered through porous channel walls and so would not set up a secondary flow circumstance such as that which occurs when entry is over the banks. I agree that in such a case some allowance would have to be made for the very strong secondary effects that would be generated and feel, with Dr Van der Beken, that a term β' of a value other than unity might be a satisfactory way of making such an allowance. However, as we did not in fact encounter such secondary flows I do not feel competent to comment upon what values of β' might be appropriate.

77. **Dr Robinson** raised the question of whether by using an energy approach to the problem of spatially varied flow in which one jump was assumed to be present we have begged the question of the possibility of more than one jump existing in the problem.

78. The energy approach can be used assuming the presence of many jumps. If it were to work the technique of locating multiple jumps would be to first assume the jumps located equidistantly along the channel. Then, by varying the position of one of the jumps the position at which energy losses in the channel were minimized could be found. Fixing the position of the first jump at this point, the next jump would be moved and its position at which total energy losses were minimized found. This process could be repeated until all jumps had been located at points which gave minimum energy losses. Obviously the process would then have to be repeated until two successive runs caused only insignificant adjustments to the jumps' positions.

79. When we attempted to do this process with only one jump present we ran into the difficulty reported in the Paper that the energy loss minimizing process could not be used because of the essential stability of the equations. To carry out the minimum energy technique involves assuming a wrong initial upstream boundary condition and integrating downstream from this to obtain a wrong surface profile. From the downstream boundary condition the surface profile from the channel outlet back upstream can be calculated. A hydraulic jump can be located at the point in the channel at which the conjugate depth line based on the erroneous upstream channel surface profile intersects the downstream channel surface profile. A solution obtained in this manner would not theoretically be correct because the initial upstream boundary condition, the depth, was wrongly chosen so it would be necessary to repeat the process many times using different initial depth values and finally finding that initial depth value that gave minimum energy loss within the channel length. In fact this idea could not be used because whatever the initial depth chosen the finite difference solution 'homed in' on the correct answer very rapidly.

80. It is thus impossible to test the single jump analysis based upon minimum energy loss concepts. If the minimum energy concept cannot be tested then it cannot be applied to the multiple jump circumstance. In any case it has been shown in the Paper that multiple jumps do not occur unless the lateral inflow or the bed slope or the channel breadth or some other geometric parameter changes considerably.

81. Referring to §§ 52 and 53 and Dr Robinson's comments concerning instability, we see no reason to change our view that the test of stability given in the Paper is valid for both supercritical and subcritical flows. This test specifies the direction in which an integration will be stable but it does not follow that integration in a direction opposite to that specified will automatically lead to instability.

82. I do not feel that the accuracy of the results that Dr Robinson comments upon in § 55 is really surprising. There was very little wave action in the experimental flume so small scale error is unlikely whilst large scale error would have demonstrated fundamental error in the theoretical approach. We had carefully assessed the frictional characteristics of the channel and had developed a semi-rational modification of the Powell formula that described it. If this modified Powell formula is correctly hypothesized then the results obtained from analysis should agree well with experiment whereas if the modification proposed had been wrong then disagreement between analytical and experimental results would follow.

83. Concerning §§ 57–59, I agree with Dr Robinson's point that much remains to be done in developing the type of approach used in the Paper so that practising engineers can use it freely and with confidence. I believe that what we have said is generally true and suitably amended could be applied to prismoidal channels of non-rectangular section without gross error. I freely admit that if secondary flows are generated the modification of the Powell formula suggested will not be accurate. Our research was an attempt to describe flow circumstances in which flow enters a river or channel by seepage and we would not suggest that our friction equation could be applied with total confidence to a circumstance in which the flow entered the river by overground run-off. It is quite clear that the complex secondary flow circumstance generated by overground run-off needs further research but I believe that although this is necessary it will be difficult to investigate the effect of secondary flows on a laboratory scale.

84. I find Messrs Phelps and Smith's comments of great interest. I accept most of their remarks but I am not happy about their comment in §§ 65 and 66 in which they state that in *rectangular* channels the energy balance line and the critical depth line may cross more than once thus indicating that in some cases there may be more than one singular point within the length of the channel. I have not found this to be true, e.g. in broad rectangular channels $d/m \rightarrow 1$ so x_c definitely has only one value in this case. Whilst I accept that in channels of circular cross section there will be more than one singular point we do not accept that the possibility exists that there may be more than one singular point in rectangular channels. I am grateful to Mr Phelps and Dr Smith for drawing my attention to this peculiarity of circular channels.

85. Both the contributions by Messrs Phelps and Smith and Dr Van der Beken make the point that the hydraulic rise may be, in some cases, a simple example of the *rather abrupt change in the surface profile that occurs as the depth increases and the surface tends to the horizontal*. This when plotted as a graph of a surface profile may not be particularly impressive but when seen in practice is a very noticeable phenomenon. The variant of the hydraulic jump that we describe as a further example of a hydraulic rise is a jump which has been extended over a length of the channel because of the turbulent mixing of the inflow from the sides.

86. Usually a jump is very abrupt and the turbulence structure is coarse but in spatially varied flow the energy dissipation occurring because of the turbulent mixing may cause a much smoother depth change and finer structure of the turbulence present. It is this phenomenon that we have classified as a hydraulic rise; of course, the normal type of hydraulic jump may also occur in spatially varied flow. As stated in the Paper both types of hydraulic rise can be predicted by the methods specified and of course this fits in exactly with Dr Van der Beken and Messrs Phelps and Smith's comments.

87. In §§ 72 and 73 Messrs Phelps and Smith have proposed a variant of the equations for friction that we proposed. I take the point of their variant form. Our

version was developed because it originated from a fundamental concept and the variant proposed is a further developed form of this basic idea. We see our proposed friction formula as a first attempt and hope that someone will develop it further, improving its accuracy and its ease of use.

88. I would like to congratulate Messrs Phelps and Smith on their contribution to the discussion and look forward to seeing the results of their work in print.

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