Unsteady unconfined flow to gravity wells

J. A. FOX & I. ALI

Dr T. R. E. Chidley, University of Aston in Birmingham

There appears to have been a recent surge of interest in the problem of flow around wells both in this country and abroad. I have just returned from the Middle East where I have been engaged in the design and analysis of multi-well irrigation schemes. This involvement with the problem has further shown the real need for more research on it; in meeting this need the Authors are to be congratulated. Certain aspects described in the Paper may be of considerable importance in the determination of well losses. The assumptions made in the development of the theory expressed in the Paper do not explicitly state that the flow is assumed to be horizontal throughout the vertical plane, even at the free surface; perhaps this point could be clarified.

30. The Authors have taken a case in which the boundary conditions are particularly straightforward. I feel that it would be more helpful to the practising engineer if much more were to be said about the methods of approximating to the conditions obtaining at the effective boundaries of a real aquifer than about solutions obtainable within the domain defined by the boundaries.

31. An alternative way of deriving the characteristic equations for this problem is given in reference 5 and has been applied elsewhere by Chidley and Fox.^{6,7} Rewriting the basic equations we obtain

$$-h \frac{\partial v}{\partial r} + 0 \frac{\partial v}{\partial t} - v \frac{\partial h}{\partial r} - \frac{\partial h}{\partial t} k = \frac{vh}{r} \quad (16)$$

$$\frac{v}{gk^2} \frac{\partial v}{\partial r} + \frac{1}{kg} \frac{\partial v}{\partial t} + \frac{\partial h}{\partial r} + \frac{\partial h}{\partial t} 0 = -S_t \quad (17)$$

Now, if there is some line given in the (r, t) plane along which h and v are specified, do the differential equations determine the first derivatives of h and v along the line? If this is true,

$$dr \frac{\partial v}{\partial r} + dt \frac{\partial v}{\partial t} = dv \quad (18)$$

$$dr \frac{\partial h}{\partial r} + dt \frac{\partial h}{\partial t} = dh \quad (19)$$

must be satisfied. We can rewrite equations (16)–(19) in the matrix notation:

$$\begin{vmatrix} -h & 0 & -v & -k \\ v/gk^2 & 1/kg & 1 & 0 \\ dr & dt & 0 & 0 \\ 0 & 0 & dr & dt \end{vmatrix} \times \begin{vmatrix} \partial v/\partial r \\ \partial v/\partial t \\ \partial h/\partial r \\ \partial h/\partial t \end{vmatrix} = \begin{vmatrix} vh/r \\ -S_t \\ dv \\ dh \end{vmatrix} \quad (20)$$

32. In general there is a unique solution to these four equations in the unknowns $\partial v/\partial r$ etc. If, however, the relation

$$\begin{vmatrix} -h & 0 & -v & -k \\ v/gk^2 & 1/kg & 1 & 0 \\ dr & dt & 0 & 0 \\ 0 & 0 & dr & dt \end{vmatrix} = 0 \quad (21)$$

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is satisfied, the equations will not have a solution unless the rank of the matrix

$$\begin{vmatrix} -h & 0 & -v & -k & vh/r \\ v/gk^2 & 1/kg & 1 & 0 & -S_t \\ dr & dt & 0 & 0 & dv \\ 0 & 0 & dr & dt & dh \end{vmatrix} \quad \dots \quad (22)$$

is 3. From this it follows that any one of the four equivalent determinants similar to

$$\begin{vmatrix} -h & 0 & -v & vh/r \\ v/gk^2 & 1/kg & 1 & -S_t \\ dr & dt & 0 & dv \\ 0 & 0 & dr & dh \end{vmatrix} = 0 \quad \dots \quad (23)$$

is zero.

33. In effect, we are seeking the characteristic equation of the matrix from which the eigenvalues can be determined. From equation (21) it follows that

$$\left(\frac{dr}{dt}\right)^2 - \frac{2v}{k} \frac{dr}{dt} - gh + \frac{v^2}{k^2} = 0 \quad \dots \quad (24)$$

the two roots of which are

$$\frac{dr}{dt} = \frac{v}{k} \pm \sqrt{gh} \quad \dots \quad (25)$$

which is identical to the result obtained in the Paper. Equation (23) can be shown to yield the equations in § 9. The advantage of this method of solution of the basic equations is that it requires very little ingenuity and is completely automatic. It also permits a test to be applied which will indicate which type of differential equation one is dealing with. If equation (24) is examined it is seen to be a quadratic; if the roots of this equation are imaginary the original differential equations are said to be elliptic; if there is only a single real root the equations are parabolic, and if they are both real the equations are said to be hyperbolic. The characteristic method of solution only applies where the equations are hyperbolic.

34. Although I have not seen it applied, there seems to be little theoretical objection to applying this method of solution to the two- or three-dimensional case, rather than the single dimension used by the Authors. In this case the matrix method of deriving the characteristic equations would become very powerful indeed. For the two-dimensional case one would expect to obtain characteristic surfaces rather than lines. The computational difficulties would increase, but with modern electronic computers these problems are not insurmountable.

35. I should also like to ask what are the advantages of interpolating onto a regular solution grid as the calculations proceed, as in the Paper, rather than completing all the calculations and then interpolating the results? If this were done the computer output could be monitored at a graph-plotter showing the intersections of the characteristics. While this is not so critical for seepage flows, it would enable one to detect the shocks and surges which occur in other problems associated with this method of solution. This is important because the method of solution is likely to break down in the neighbourhood of such phenomena.

36. As a matter of interest it was found from the work I have done recently in Jordan that this type of problem could best be approached by making a mathematical model of the aquifer using a finite element approximation. It permits two-dimensional solutions and extremely variable boundary conditions including the location of major barriers and fault zones within the model area.

Emeritus Professor N. S. Boulton

The Paper presents an interesting and original application of modern wave theory to obtain water-table profiles near a pumped well under unsteady flow conditions. Moreover, the method of analysis follows recent trends in hydraulics research. However, for laminar flow conditions, the Authors give no adequate justification for analysing unsteady unconfined flow to a well using their differential equations (1) and (2) in preference to previously published equations⁸ based on Darcy's Law and the incompressibility of the water and aquifer. For turbulent flow beyond the range of Darcy's Law, their method may prove useful, although such flow is unusual in uniform aquifers to which theoretical analysis is applicable, except perhaps in regions very near to the well.

38. A fundamental shortcoming of equations (1) and (2) is that they do not explicitly determine the pressure heads (and levels in observation wells) at points in the seepage flow other than on the free surface.

39. As in a theory put forward in 1954,⁸ the Authors' theory assumes the water and aquifer to be incompressible, which is justified for unconfined flow, except for very short times of perhaps a few minutes after pumping begins, when in most cases the data from a practical pumping test are unreliable. After this short time interval, the yield from the drawdown of the free surface is large compared with the yield from compressibility so that the effect of compressibility is negligible.

40. In § 2 the Authors correctly draw attention to a source of error in existing theories which assume an abrupt change from zero to complete saturation at the free surface (constant pressure surface). The error is unlikely to be important in coarse-grained aquifers, but in fine-grained aquifers it has long been known that a delayed yield of water may occur from storage above the lowered free surface which considerably affects the drawdown, especially during the earlier stages of pumping. Methods of allowing for the effect of this delayed yield on the drawdown and storage coefficient are available. No allowance for delayed yield is however included in the Paper. The use of an alternative parameter to that of effective porosity does not, as the Authors imply in §§ 27 and 28, eliminate the physical phenomenon of variable porosity.

41. It is important not to overlook the inaccuracies in the new theory when practical applications are contemplated. In §§ 23 and 24 the Authors recognize that the larger drawdowns in the immediate vicinity of the well are in error, but they claim good agreement between theory and experiment when the drawdowns are small. In § 26, on the other hand, they admit that the error in the small drawdowns is too large.

42. The columns headed 'disagreement %' in Tables 1 and 2 are misleading since they are based on total aquifer thickness; hence the percentage disagreements are inevitably small for small drawdowns. If the difference in drawdown between theory and experiment is expressed as a percentage of the experimental drawdown, the discrepancies are very large. For example, from Table 2, for a distance from well of 25.5 in. and time after start of 6 s, the disagreement is 97%, which is large as compared with the disagreements between existing theories—as for example the Theis and V-function values.^{8,9}

43. Another inaccuracy of the new theory is that it assumes that the free surface radii of curvature are large (§ 23). Hence the true surface condition for constant effective porosity (equation (3), reference 8) is not in general satisfied.

44. It would be of interest to know if the usual condition of constant discharge to the well was approximately satisfied during the flow tests. If so, the statements in §§ 23 and 24, regarding the surface of discontinuity (seepage surface) at the well, are at variance with theory and published analogue tests.¹⁰ These show that the discontinuity reaches its maximum value a very short time after pumping (at constant discharge) commences.

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45. With reference to the statement in § 28 that the seepage surface is located by the critical depth, it has been shown by relaxation¹¹ that, for the steady state, the critical depth theory as given by Jaeger may give 100% error in the height of the seepage surface.

46. The new theory as expounded in the Paper cannot be regarded as an advance, from a practical viewpoint, on previous methods of analysing pumping tests in cases where turbulent seepage flow is absent or unimportant. There exists an extensive literature relating to theoretical and practical aspects of groundwater flow, including unconfined flow. It is regrettable that the new theory lacks the simplicity of (for example) the Theis equation while containing quite as crude approximations.

Mr N. M. Awan, Imperial College of Science and Technology

The Authors have presented an interesting analysis of unsteady flow to gravity wells. They have solved the continuity and dynamic equations by the method of characteristics and have found that for low drawdowns their theory agrees well with their sand model results. However, for large drawdowns, the experimental free surface curves lag behind the theoretical ones. This conclusion is reasonable in view of the limitations of the theory.

48. In the derivation of the continuity and dynamic equations, it is assumed that the pressure distribution is hydrostatic. This assumption has been used extensively in the study of unsteady flow in open channels and the free surface flow in porous media. Henderson¹² has shown that in order to conform to the hydrostatic condition along a vertical section, the acceleration term in Euler's equation should be zero. This implies that the vertical velocity everywhere must also be zero. It is, therefore, clear that the flow should substantially be horizontal—an assumption commonly known as the Dupuit–Forchheimer (DF) assumption in ground water flow. In spite of its limitations it has found wide acceptance in the study of free surface flow to gravity wells or horizontal drains.

49. The theory based on DF assumptions disregards seepage surface in the well and the head loss through the gravel pack and the well screen. It can easily be shown that the vertical velocity component is approximately proportional to the square of the hydraulic gradient.¹³ It is owing to this effect that there is a large divergence between the experimental and theoretical curves for large drawdowns.

50. The free surface according to the DF theory is given by

$$h^2 = h_v^2 - (h_v^2 - h_u^2) \frac{\ln(R_2/r)}{\ln(R_2/R_1)} \quad \dots \quad (26)$$

where h represents the height of free surface at a distance r from the well centre. Other notations are as in the Paper. If the depth of water h_u in the well at time t is known, equation (26) will give the free surface heights at that time. I have compared the free surface computed by equation (26) with the free surface obtained by the method of characteristics. Table 3 shows the comparison between the Authors' theory and the DF theory. It may be pointed out that in equation (26) h_u is the actual depth of water in the well. The Authors, however, have taken it to the point above the impermeable bed where the experimental free surface intersects the well casing. It is observed that there is practically no difference between the heights of the free surface as computed by the rather laborious method of characteristics and those given by the simple DF equation (26). In contrast to Weber's equation, as criticized by the Authors, there is no similar vague term in this equation. Another interesting point that may be observed from Table 3 is that the free surface at a distance greater than 8 in. from the well centre is the same in both the theories, even though the Authors have taken $h_u = 13.15$ in. instead of the actual value of 11.94 in. Free surface heights with $h_u = 13.15$ in. are also shown in Table 3. It is observed that the DF theory agrees more closely with the experimental results than the Authors' theory.

Table 3. Comparison of Authors' theory with DF theory

SI No.	Distance from well centre, in.	Height of drawdown curve, in.			
		Expt.	Authors' theory $h_u = 13.15$	DF theory $h_u = 11.94$	DF theory $h_u = 13.15$
1	2.00	11.94	13.15	11.94	13.15
2	8.00	16.20	15.02	15.02	15.70
3	14.00	17.08	16.14	16.15	16.60
4	20.00	17.75	16.83	16.82	17.10
5	25.50	18.00	17.34	17.25	17.50
6	31.50	18.30	17.71	17.65	17.80
7	37.00	18.58	18.00	17.90	18.10
8	43.00	18.70	18.24	18.20	18.25
9	49.00	18.80	18.44	18.40	18.45
10	55.00	18.85	18.61	18.55	18.60
11	61.00	18.90	18.75	18.70	18.75
12	67.00	18.95	18.88	18.90	18.90
13	73.00	19.00	19.00	19.00	19.00

Values taken from Table 2 for 38% drawdown and 10 s after start.

51. Equation (26) has been criticized by several investigators to whom some references have already been made in the Paper. Because of closer agreement with the DF theory, the Authors' theory is subject to the same limitations. I agree with the Authors that their theory can be applied with a reasonable amount of certainty for small drawdowns only. This condition conforms to DF assumptions which are implied in the theory.

52. There appears to be some confusion in the notations used in the Paper. In § 6, the Authors have taken V as the velocity of flow in an open channel without porous medium, whereas in § 7 they have denoted the same velocity by v . There are two types of velocities that are considered in the flow through porous media. The first is the pore velocity, which is the fluid velocity in a cross section of the soil pores. Since the pore distribution within the soil is very irregular, the pore velocity is taken to be equal to the discharge divided by the overall area of the pores. The second velocity is the seepage velocity, obtained by replacing the real flow of a fluid in the soil pores by an imaginary seepage flow filling in a continuous manner the volume of both the pores and the soil skeleton. The seepage velocity is equal to the discharge divided by the overall area of the soil mass including the pores. In the context of these definitions, V and v are pore and seepage velocities respectively.

53. In order to compare the experimental and theoretical drawdown curves, the Authors have neglected the existence of the seepage surface. Equation (14) shows that any increase in h_u will result in a corresponding decrease in the well discharge if the value of K obtained from the experimental data be used. The Authors have, therefore, underestimated the well discharge. I believe that K should be modified keeping the well discharge constant. An approximate relation between h_u and the modified value of K can be found from the experimental data and then used in the computer program.

54. I should like to refer to a similar work by Mahdaviani.¹⁶ There are, however, two distinct differences between Mahdaviani's dynamic equation and equation (4). The first is that in the former the velocity head has not been considered. The second is that S_r is taken equal to $v/\text{permeability}$, which can easily be proved¹⁷ for the ground water flow conforming to Darcy's Law.

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Dr P. A. Johnson, Sir M. MacDonald & Partners

As the Authors will know, I investigated the possibilities of using electrical resistance gauges in 1961 when at the University of Leeds. At the time I was deterred by lack of suitable recording equipment, but am pleased to see that this has now been overcome. More details of the gauges would be of interest.

56. What is the calibration curve for these gauges? I seem to remember a log-log plot of resistance and depth gave a straight line but the form of the present gauges may have altered this. It is assumed that recalibration was necessary before each run to allow for changes in electrical conductivity of the water. If this is the case have the Authors considered if it is possible to use a 'dummy' gauge in the circuit to dispense with the routine calibration procedure?

57. It is assumed that high frequency voltages were used to reduce polarization effects; were these very noticeable using direct current?

Mr Fox and Mr Ali

We were pleased to see the interest taken in our Paper and are grateful for the valuable comments made by the various contributors. Replying to the contributions in sequence we would like to make the following observations.

59. In § 29 Dr Chidley asks if we have assumed flow to be horizontal throughout any vertical plane. We have not explicitly made this statement, but it is implied in the comment made in § 5 that the pressure distribution must be hydrostatic as this effectively infers that vertical accelerations must be negligible if the theory is to hold. In § 30 Dr Chidley raises the question of the simplicity of the boundary conditions selected and suggests that in field problems such simplicity is not usually encountered. We would like to answer this point by saying that in most theoretical studies of this nature such assumptions as we have made concerning the boundary conditions are usual. However, we acknowledge that more complex boundary conditions may occur in nature, but we would like to point out that, provided that they can be defined in an algebraic expression of some sort, it is relatively easy to insert any boundary condition into the program and obtain a solution. In this sense this theory may be thought to have an advantage over *more* analytic solutions, in which it would be difficult, if not impossible, to deal with complex boundary conditions.

60. In § 31 Dr Chidley presents an alternative way of developing the characteristic equations. This method is of course known to us^{6,7} and we completely endorse it. It may be that as a technique, Dr Chidley's presentation in matrix notation is superior, but we feel that there is a case to be made for the method used in the Paper, as we believe it gives a better physical insight into the nature of the solution. The method used is due to Stoker but the reader is recommended to see a method given by Mary Lister (quoted in our Bibliography).

61. In § 34 Dr Chidley raises the question of the case of three-dimensional characteristics. The problem that has been investigated in the Paper is axis-symmetric and does not require the use of three-dimensional characteristics. In more complex situations such as asymmetric flow to a single well or flow to a multiple well system, we have found (in later work as yet unpublished) that the simple approach advanced in the Paper can be successfully used.

62. Referring to § 35, we would like to make the observation that in all probability the variation of the method he suggests would work. We do not feel able to assess the relative merits of the two interpolation techniques, but can envisage more difficulty in programming the scheme he advances than in the scheme we used. His suggestion has the merit that it will detect the onset of shock conditions, but we think it unlikely that these will arise in this context. Also we believe that it would not be very difficult to build checks into the program as at present written to test routinely for shock development.

63. We would like to thank Professor Boulton for his critical appraisal of the

Paper, and although we do not fully agree with some points in his assessment of it we are grateful for his detailed analysis of the defects of the theory we have advanced. In reply we should like to make the following points. In § 37 Professor Boulton says that he does not feel that we have 'adequately justified the application of the theory to laminar flow conditions in preference to previously published equations'. In our reply to § 34 we have pointed out that our approach will handle multiple-well problems and single-well problems in which the flow is not axi-symmetric. We hope that this will be seen as a justification for our method. Also we do not have to treat the porous medium as being isotropic. Professor Boulton's paper⁸ dealt with the case of differing vertical and horizontal permeability. Our method has to assume that in any vertical plane the coefficients of permeability in the horizontal and vertical direction are the same, but we can comparatively easily deal with a problem in which permeability varies throughout the aquifer in the horizontal direction.

64. In § 38 Professor Boulton raises the question of pressure heads. We fully acknowledge that our theory assumes the pressure to be hydrostatically distributed over a vertical surface (§ 33). This is a basic assumption that has to be made if shallow water wave theory is to be applicable. We accept this as a limitation but suggest that the advantages of the theory in dealing with complex situations as pointed out above may outweigh this drawback.

65. We are in full agreement with Professor Boulton when he says in § 40 that the theory cannot deal with delayed yield effects. We were, of course, aware of this defect when we wrote the Paper (§ 3), but up till now we have not been able to think of a way of overcoming this inadequacy in the theory. Almost certainly the presence of incompletely saturated porous media above the phreatic surface invalidates the assumption of hydrostatic pressure distribution near the surface. It should prove to be a fruitful topic for further research.

66. We would like to deal with the points raised in §§ 41–44 together.

67. The main issue here is the question of accuracy. We would like to defend our choice of flow depth as opposed to drawdown when we compared theoretical and experimental results. First, the parameter in the theory is flow depth. It seems logical therefore to compare flow depths rather than drawdowns. It is well known that the difference of two large quantities can possess a large percentage error unless the two large quantities have been obtained with very great accuracy. We acknowledge that we have not succeeded in obtaining the time varying flow depths in the experiment to a better accuracy than 0.1 in. Certain electronic limitations in the instrumentation imposed this figure, and up to the moment of writing we have not been able to improve it. We would point out that 0.1 in. in 20 in. is only 0.5%, which would appear to be well within normally accepted accuracy tolerances. In many places, however, the drawdown is of the same order of magnitude as our residual instrumental error, which accounts for the discrepancies noted by Professor Boulton. A possible way of avoiding the difficulty mentioned above would be to use a fine porous medium. This would give large drawdowns. However, as the model is small the dr increments used in the integration must of necessity be very small. To stay in the domain of dependency the dt values must also be very small (in the reported analysis $dr = 6$ in. and $dt = 0.05$ s).

68. If fine material is used the duration of an unsteady run would be great and the computing time needed to produce results for comparison purposes would be prohibitively long. For this reason fine material could not be used. This may at first sight be considered a major drawback to the proposed theory. However, in nature, dx could be in feet, possibly in hundreds of feet at points at a large distance from the well, and dt values could then be much larger. Thus significant information could be obtained from a computer run of reasonable duration and therefore at reasonable cost. The discharge referred to in § 15 was not constant.

69. The boundary condition at the well is fed in to the program as data and the need for constancy of the pumping rate is thus avoided.

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70. We regret that Professor Boulton does not see a future in practice for the theory we advanced (§ 46). Its application to asymmetric circumstances and multiple-well problems would suggest, in our view, that it has some merit in the field of practical engineering. The solution of the multiple-well problem has been obtained at Leeds since the submission of the Paper for publication and the results are, if anything, in better agreement with experiment than they are for the single well.

71. We accept Mr Awan's comments in §§ 47–48 with reservations about the cause of error in the case of large drawdowns. Although we cannot be certain we think that there may well be inescapable experimental error in depth measurement caused by the presence of the depth gauges themselves. These depth gauges although made as pervious as possible (see Fig. 9) must have caused some obstructions to flow and the gauges near the well were operating in spatially restricted circumstances. If this hypothesis is correct it would explain the fact that experimentally obtained depths were always larger than theoretically obtained depths.

72. A fact that supports the hypothesis is that in further work on the multiple-well problem where gauges similar to these were employed, but in unrestricted conditions, the results are significantly better.

73. In § 50 Mr Awan gives the steady-state Dupuit equation and applies it to unsteady conditions. The point that he makes that it describes the unsteady conditions of our experiment quite well is interesting but, perhaps, not very surprising.

74. In this problem terms such as $\partial v/\partial t$ and $\partial h/\partial t$ are not large except close to the well, so one would expect that at significant distances from the well the steady and unsteady theories will not be greatly different.

75. However, we would again like to make the point that the approach given is capable of very great extension to deal with multiple-well and other problems involving complex boundary conditions to which the analytic approaches for either steady or unsteady states are not well suited.

76. We agree of course with Mr Awan's comments in § 51 with reservations.

77. In § 52 Mr Awan has amplified what we intended to say concerning pore velocities and notional seepage velocities in § 7. We thank him for his clear statement of the difference between these velocities, but would like to comment that we intended to express the same concept.

78. Mr Awan's comment in § 53 is not fully clear to us. The value of K (§ 20) was only established to confirm the value of b determined in § 19 of the Paper. The level of the surface in the well was not used in evaluating this, so a seepage surface, if present, would not affect the result.

79. Mr Awan mentions in § 54 the paper Mahdavian published. This paper appeared after ours had been submitted to the Institution of Civil Engineers. It bears many points of similarity to ours but differs in some aspects. In particular, Mahdavian investigated the case of unsteady flow through a finer porous material than the one we used, and as a consequence he only deals with the laminar flow case. His drawdowns are larger but on the whole his results are similar to ours and the two papers can be regarded as supporting one another.

80. Dr Johnson raises the question of depth gauges. He has investigated the possibility of using electrical resistance-type depth gauges. This idea has been followed up by R. White, D. J. N. Currie and I. Ali and the type in current use is the result of the efforts of all four workers.

81. The type used in this investigation is illustrated in Figs 9 and 10, the skeletal construction being used to limit, as far as possible, the obstructing effect of the gauge. In conditions where the gauge is located well away from any boundary the gauge is fitted with a copper tube in place of the wire skeleton that has been used in this application. This is structurally stronger but a $\frac{1}{4}$ in. dia. hole must be drilled in the top of the tube to permit air to flow in and out as surface levels change. The bottom $\frac{1}{2}$ in. of the tube is cut away to permit the entry of water. The frequency response of the gauge has proved more than adequate in this application and it can detect approxi-

mately 0.05 in. of water surface level change taken over the lower ranges of depth. The gauge is fed with a 30 kHz signal from a power oscillator and the calibration curve of the system is nearly straight and of sigmoid type. Because it is not perfectly straight the calibration curve must always be used when interpreting results. The sigmoid shape of the calibration curve limits the accuracy to approximately 0.1 in. when working at large depths.

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