Transport in Porous Media **56:** 113–116, 2004.

Letter to the Editor

Reply to the "Comments on 'On an Exact Analytical Solution of the Boussinesq Equation"', *Transport in Porous Media* **52**, 389–394, 2003

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(Received: 22 July 2003)

Kacimov and Serrano (2003) criticize Parlange *et al.* (2000) primarily for not referring to Sokolov's (1956) solution of the Boussinesq equation as reported by Polubarinova-Kochina (1977), both in Russian. This criticism is grossly exaggerated.

We note, first, that Polubarinova-Kochina (1977) writes the solution without discussing it whereas Barenblatt's solutions are discussed in detail. However, in her 1962 English version, Polubarinova-Kochina (1962) only discusses Barenblatt's solutions. Furthermore, Barenblatt *et al*. (1990) discuss in detail Barenblatt's solutions (linear and quadratic) only and do not even give Sokolov's result. Actually, this is quite understandable as Sokolov's solution is trivially obtained from Barenblatt's quadratic solution, by a translation (see below). Thus, if that is all there is to Sokolov's result then it would hardly be worth mentioning, but in any case we would of course have added it to our references if we had been aware of it. However, nothing else in the paper would have required any change.

Kacimov and Serrano (2003) write "Barenblatt and Sokolov interpreted their $h(x, t)$ as a decaying groundwater mound", it is indeed remarkable that all appli-

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cations have been limited to decaying mounds. In fact, it is the interpretation and application of Sokolov's solution to other flows that makes it important, otherwise it is hardly of any relevance. This was one of the two main points of our paper and Kacimov and Serrano (2003) recognize that the extension to other physical situations is a new observation. The second important and theoretical point in our paper, clearly missed by Kacimov and Serrano (2003), is the fact that the two solutions reported by Barenblatt are, in fact, not independent. The quadratic result includes the linear one and this is what makes it 'unique'. Apparently, this was not appreciated before our paper (Parlange *et al.*, 2000, p. 341) or we presume Barenblatt *et al.* (1990) would have mentioned it. This point is worth repeating here because of its importance, so that casual readers might not miss it: Starting with Barenblatt's quadratic solution with Parlange *et al.*'s notations,

$$\frac{K}{S}(h_0 - h) = \frac{X^2}{6(t+\alpha)}, \tag{1}$$

where h_0 is the value of h at $X = 0$. Then translate the X-axis so that

$$X = \left(x + 3\sqrt{\frac{K}{S}}\beta\right). \tag{2}$$

Equation (1) becomes Equation (10) of Parlange *et al.* (2000), that is,

$$\frac{K}{S}(H - h) = \sqrt{\frac{K}{S}}\frac{\beta x}{t+\alpha} + \frac{x^2}{6(t+\alpha)}, \tag{3}$$

where $H(t)$ can be written as Equation (11) of Parlange *et al.* (2000). If now we let $\beta, \alpha, \to \infty$, keeping $\beta = \alpha$, (3) yields at once Barenblatt's linear result. Hopefully, in the future the two solutions will not be presented again as being independent as purported by Kacimov and Serrano (2003).

Obviously we cannot disagree with Kacimov and Serrano's (2003) emphasis that relevant literature should be appropriately cited and the importance of a 'scientific spirit of openness and justice'. Two of their own papers which they refer to are indeed quite pertinent to this discussion. First, consider Kacimov (1997), which Kacimov and Serrano (2003) complain we did not refer to. It has two sections, the first looks at well-known results for 'mounds' which adds nothing to our existing understanding. The second linearizes the Boussinesq equation and tries to analyze the decay of a 'mound' on a dry impervious bottom layer. As is well known this approach contains unphysical features, for example, the front moves at an infinite speed! The unphysical results of the approach were not discussed. If it had been pointed out that linearization should not be used for this problem then the paper might have been worth citing. Second, the comment also praises the work by Serrano and Workman (1998) but does not point out that our comment on it (Barry *et al.*, 2000), which is also cited by Kacimov and Serrano (2003), shows in a very straightforward manner that their Boussinesq approximation is faulty. Furthermore, Barry *et al.* (2000) gives the proper formulation. In their reply, Serrano and Workman (2000) were unable to identify where Barry *et al.* (2000) failed to improperly apply their approximation. Rather, they repeated the correct solution of Barry *et al.* (2000) and somehow deduce from this repetition, and without attribution, that their approach 'produces the exact solution'. In the same paper, Serrano and Workman (1998, p. 247) state: "The polynomials A_n are generated for each non-linearity so that A_0 depends only on h_0, A_1 depends only on h_0 and h_1, A_2 depends only on h_0, h_1, h_2, etc. All of the h_n components are calculable. It is now established that the series $\sum_{n=0}^{\infty} A_n$ for $N(h)$ is equal to a generalized Taylor series for $N(h_0)$ that $\sum_{n=0}^{\infty} h_n$ is a generalized Taylor series about the function h_0, and that the series terms approach zero as $1/(mn)!$, if m is the order of the highest linear differential operator. Since the series converges and does do very rapidly, the n-term partial sum $\Phi_n = \sum_{i=0}^{n-1} h_i$ usually serves as an accurate enough and practical solution." It is noteworthy that almost a decade earlier, Adomian (1990, p. 18) states: "The polynomials A_n are generated for each nonlinearity so that A_n depends only on u_0. A_1 depends only on u_0 and u_1, A_2 depends on u_0, u_1, u_2, etc. All of the u_n components are calculable, and $u = \sum_{n=0}^{x} u_n$. It is now established that the series $\sum_{n=0}^{x} A_n$ for $N(h)$ is equal to a generalized Taylor series about $f(u_0)$, that $\sum_{n=0}^{x} u_n$ is equal to a generalized Taylor series about the function u_0 and that the series terms approach zero as $1/(mn)!$ if m is the order of the highest linear differential operator. Since the series converges and does so very rapidly, the n-term partial sum $\varphi_n = \sum_{i=0}^{n-1} u_i$ can serve as a practical solution." Serrano and Workman (1998) wrote their own statement without attribution.

In conclusion, their comment is both intemperate and without noticeable merit. As stated in the abstract of Parlange *et al.* (2000) our paper constituted an 'application and extension of Barenblatt's solutions'. Their own two papers which they cite and we discuss here leave much to be desired.

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