LETTERS TO THE EDITOR

To the Editor:

The recent work of Mackie et al. (*AIChE J.*, **33**, 1761, Nov., 1987) is concerned with the formulation of a new model for the transient behavior of granular filtration of hydrosols. Their work is a compromise between the traditional approach of modeling deep-bed filtration, largely empirical, and the more rigorous but tedious stochastic simulation, which requires the tracking of individual particle trajectories. It is, therefore, an important piece of study of deep bed filtration since it represents a new method of assessing the effect of deposition on filter performance.

In formulating their model, Mackie et al. advanced certain arguments and statements which, in our opinion, are incorrect or require further clarification. The purpose of this letter is to outline these issues with the hope of eliciting comments from the authors. These issues are:

1. Concerning the relationship between the filter coefficient, λ, and the efficiency of the unit collector, the authors claim that the correct relationship is given by Eq. 12, and not by Eq. 11. In fact, both expressions may be considered correct, as well as others. This multiplicity of relationships between λ and η arises from the variety of choices one has regarding the shape of the unit bed element (UBE). Corresponding to the assumption that an UBE is cylindrical, spherical or cubical in shape, the following relationships may be obtained:

cylindrical	$l = 4a/3(1 - f_0)$
spherical	$l = 2a/(1 - f_0)^{1/3}$
cubical	$l = 2a\left[\dfrac{\pi}{6(1 - f_0)}\right]^{1/3}$

The last expression was used by Mackie et al. in relating λ and η, which led to Eq. 12. On the other hand, Rajagopalan and Tien applied the first expression in obtaining Eq. 11.

This difference in the definition between λ and η is indeed a nuisance. In practical terms, the difference between these definitions is insignificant (for example, if $f = 0.4$, the first and third definitions differ by 17%) as compared with the experimental accuracy of λ.

2. The statement prior to Eq. 8, that surface forces are unimportant in determining the magnitude of λ, certainly is incorrect. What Rajagopalan and Tien concluded was that in case of favorable interactions, the difference in attractive forces in systems of practical interest is insufficient to make any difference in the value of λ. There is sufficient experimental evidence which indicates significant decline in λ as the surface interactions become more repulsive.

3. The hypothesis regarding the effect of interestitial velocity is certainly correct. The expression used to quantify this hypothesis, i.e., Eq. 17, is totally incorrect, as it relates a London force acting in the normal direction with a hydrodynamic drag force acting on the tangental direction.

Correct formulations for the adhesion of impacting particles have been given previously (Gimbel and Sontheimer, 1978; Vaidyanathan and Tien, 1988). These earlier studies have shown that in determining particle adhesion, one should consider not only the forces involved, but also the surface structures. This last point was apparently overlooked by the authors.

4. To account for certain experimental observations that λ may decline with the increase in σ, the author postulates the presence of a no-deposition region ($\theta_{1_i} < \theta < \theta_{2_i}$) over filter grains. Accepting this hypothesis and the use of Happel's flow field, or even the expression given in Eq. 54, it can be seen that θ_{2_i} must extend to $\pi/2$ since v_θ is a monotonically increasing function of θ. As a result, among the three possibilities suggested by the authors, only the first one is possible, significantly simplifying the subsequent analysis.

5. The model proposed, has two adjustable parameters, χ_{ij} and v^*. Based on the results shown in Table 3, which give $\chi_{ij} = 1$ for the majority of cases, the effect of deposited particles on the flow field appear negligible. On the other hand, the v^* value used was 0.02 m/s, which is at least one order of magnitude smaller than values obtained in particle adhesion measurements. Consequently, v^* has no physical significance and must be decided upon on a case-by-case basis.

6. One should perhaps be more cautious in claiming model validation through agreement between predictions and experiments. First, the authors did not use all the data they collected but rather used only those which were considered as accurate (presumably on the basis of their better agreement with model prediction). Even for those selected data, they exhibit considerable scattering and even some inconsistency. This last point can be seen clearly in Figures 8–11 by comparing data points obtained from the top layer and those from the second layer of their experimental filter. In fact, one may even argue that those two data groups actually display opposite behavior.

In sum, we find that the model advanced by Mackie et al. is interesting and useful because it combines features of both conventional modeling approach as well as those of stochastic simulation. On the other hand, even with further refinement and modification, to overcome the objections listed above, the model, in *all* probability, will still contain some empirical parameters. This is not really a reflection of any "modeler's" ability, but a direct result of the complexities of granular filtration.

Literature cited

Gimbel, R., and H. Sontheimer, "Recent Results on Particle Deposition in Sand Filters," *Deposition and Filtration of Particles from Gases and Liquids*, The Society of Chemical Industry, London, (1978).

Vaidyanathan, R., and C. Tien, "Hydrosol Deposition in Granular Beds," *Chem. Eng. Sci.*, **43**, 289 (1988).

Chi Tien and Rajasekar Vaidyanathan
Dept. of Chemical Engineering
and Materials Science
Syracuse University
Syracuse, NY 13244

Reply:

I welcome the comments of Tien and Vaidyanathan about our model of the effect of deposition on removal efficiency in deep bed filtration. As they point out, deep-bed filtration is an extremely complex process and, with the present state of knowledge, any model is bound to contain a large number of simplifications. Therefore, it is essential that we recognize the approximations made in any model, and understand their significance. In view of this, their letter is particularly welcome, and I will now reply to each of the points that they raise.

1. I still believe that the relationship used in our paper, between λ and η (the third one mentioned by Tien and Vaidyanathan), is the best one to use. This is based on the principle that η is the collection efficiency of a single grain in a filter, and that the relationship between λ and η should be independent of the type of collector model used. Moreover, while it is possible to divide a filter into small cubes, dividing it into small cylinders will inevitably leave gaps between the cylinders. The same is true for spheres. Admittedly, with the present accuracy of data collected from filtration experiments, arguing about a difference of about 15% in the calculation of λ is of little immediate practical importance. However, the same amount of computational effort is involved in calculating λ, whichever relationship is used, so we should avoid introducing unnecessary errors.

2. The model is designed to deal only with the case where surface forces are favorable, and the authors recognize that the model is therefore not valid when significant repulsive forces are present.

3. The authors accept that particle-grain adhesion is much more complex than the derivation of Eq. 17, and that values for v^* derived from this equation will be subject to error. However, although the London force does act normally, it is still the case that it plays a major role in determining the value of v^*. Also, as pointed out in the original paper, Visser (1976) did find experimental evidence for equating the London force to the tangential drag force.

The authors recognize that surface structure may also play an important role in determining v^*. This will be particularly true when the grain is covered with deposit, as the collector surface will be much rougher. The effect of surface roughness will mean that v^* is not independent of size, as surface roughness will have a greater effect on smaller particles than on larger ones.

4. The authors do not agree with Tien and Vaidyanathan that θ_2 must extend to $\pi/2$. The part of the model that deals with regions of no-deposition is there to model the decrease in λ *after* the initial increase, not an initial decrease in λ. Consider the case where, initially, particles can deposit anywhere on the collector. Under the assumptions made in the model, the deposit will form a dome shape on the collector, and therefore, the relative increase in tangential interstitial velocity caused by deposition will be greater near $\theta = 0$ than near $\theta = \pi/2$. So, although initially the tangential velocity is greatest at $\theta = \pi/2$, after deposition has occurred, this is not necessarily the case, as the deposit may have caused the tangential velocity to be greatest at some point other than $\theta = \pi/2$. Indeed, under the assumed relationship for the change in grain shape, Eq. 36, there is no increase in tangential velocity at $\theta = \pi/2$, so θ_2 will be less than $\pi/2$ for a time. In practice, deposition extends beyond $\pi/2$, and the model is a crude approximation, but the possibility that a region of no-deposition may develop in the middle of the original deposition area still needs to be considered.

5. As it currently stands, the quantities, χ_{ij}, and v^*, are, in effect, empirical parameters. However, this is not because they have no physical significance, but because mathematical models of adhesion and of the effect of deposited particles are inadequate. If ever a near perfect model of filtration is developed, I believe that the quantities, χ_{ij}, and v^* (or something similar to them), will play a part in the model.

6. In the authors' opinion, the major value of the model is that it highlights the importance of polydispersity on filtration performance. The qualitative behavior displayed in Figures 5 and 6 was consistently observed in all experiments performed, reported and unreported. The general principles upon which the model is based (outlined on p. 1764) can all be supported on purely physical grounds. Therefore, the authors maintain that the model is valid qualitatively.

As far as quantitative agreement is concerned, we only claimed "fair" agreement. The model is not intended to be definitive, but it demonstrates that it is possible to develop a model that is not as complicated as stochastic simulation models, while still giving reasonable results. The model contains many simplifications, and does not include a number of physical factors that may be important. Several of these were noted at the end of the paper, and Tien and Vaidyanathan pointed out another in their third point. It is especially worth noting that, to the best of the authors' knowledge, when the model was developed, no models existed that would predict the difference in the change in filtration efficiency for particles of different sizes. Moreover, the model successfully predicts this behavior because of the physical principles incorporated in the model, not because of any empirical parameters used.

In conclusion, I hope that our work has shown that particle size distribution must be accounted for in the development of any theory of filtration, and that the underlying physical principles in the model are major contributory factors in explaining the effect of size distribution.

Literature cited

Visser, J., "The Adhesion of Colloidal Polystyrene Particles to Cellophane as a Function of pH and Ionic Strength," *J. Coll. Int. Sci.*, **55**, 664 (1976).

R. I. Mackie
Dept. of Civil Engineering
University of Dundee
Dundee, Scotland DD1 4HN