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ON THE VALIDITY OF THE KOZENY EQUATION

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IF we want to describe the flow of fluids through a porous medium, we generally use for the equation of motion an empirical relationship that is usually called Darcy's law (*see* Scheidegger[(1)])—

$$g = -\frac{k}{\mu} \operatorname{grad} p$$

In the above formula, g signifies the vector of the approach (filtration) velocity, p is pressure, μ the viscosity of the fluid and k is called the 'permeability' of the porous medium.

As noted above, Darcy's law is essentially an empirical relationship, but, since the fluids in question are ordinary, viscous fluids, it should be possible to deduce it theoretically from more fundamental principles—that is, from the Navier Stokes equations for viscous fluids, if the boundary conditions characteristic for a porous medium are introduced. These boundary conditions state that the fluid must stick to the walls of the pores—that is, that the fluid velocity at the walls of the pores must be zero. Needless to say, it is not possible to state these boundary conditions in analytical form, simply because it is quite beyond present capabilities to state the equation of the pore space analytically.

The form of Darcy's law—the linear relationship between the pressure gradient and the flow (or filtration) velocity—reminds one very much of the Hagen-Poiseuille equation, which is valid for tubes. The latter states—

$$Q = \frac{\pi}{8} \frac{a^4}{\mu} \frac{\Delta p}{\Delta L}$$

where Q is the volume flow through the tube, a the radius of the tube and Δp is the (finite) pressure drop over the length ΔL of the tube. As before, μ is the viscosity of the fluid.

Since the flow through one tube follows much the same law as the flow through a porous medium, it is tempting to regard a bundle of tubes as a model of a porous medium. Let us assume that there are n tubes of radius a per unit area, then the filtration velocity g through such an array is (we write proportionalities only, neglecting any numerical factors)—

$$g \sim \frac{n}{\mu} a^4 \frac{\Delta p}{\Delta L}$$

In a porous medium, n and a are not known, but these can be expressed by other geometrical quantities. The specific internal area S (the internal area per unit bulk volume) of the model is given by—

$$S \sim na$$

and the porosity P by—

$$P \sim na^2$$

Hence, by eliminating n and a from the above three proportionalities, we obtain—

$$g \sim \frac{P^3}{S^2} \frac{1}{\mu} \frac{\Delta p}{\Delta L}$$

Comparing this with Darcy's law yields—

$$k \sim \frac{P^3}{S^2}$$

Alternatively, written as an equation in which c is a constant—

$$k = c \frac{P^3}{S^2}$$

Equations of this type are called Kozeny equations, after Kozeny,(2) who deduced the relationship from a somewhat more elaborate model. In fact, Kozeny took an arbitrary cross-section of a porous medium, assumed that all the holes are of the same shape (that is, circles) and solved the Navier Stokes equation under the assumption that the *flow is orthogonal to the cross-section.* It is easily seen that this procedure is essentially equivalent to taking a bundle of straight, parallel tubes as model for the porous medium, albeit of varying radius; for circular tubes, Kozeny found the theoretical value for the proportionality constant c to be $\frac{1}{2}$. Most theoretical values (for other shapes) fall into the same neighbourhood.

Empirical tests do not bear out the validity of the Kozeny equation. For industrial powders, the constant must be taken as $c = \frac{1}{5}$ to get even a remote resemblance with observed results. It has been attempted to explain this discrepancy by introducing a 'tortuosity' T of the porous medium, implying that the flow path is not the directly measured ΔL, but $T\Delta L$. In order to make the Kozeny equation fit the facts, T has to be of the order of 2·5, which means that the flow distance is 2·5 times larger than the geometrical distance through the porous medium. This is an unreasonably high value indeed.

Even with the 'corrected' value of the Kozeny constant (that is, $\frac{1}{5}$ instead of $\frac{1}{2}$), the experimental results are not very satisfactory. Kozeny[3] himself found discrepancies between calculated and measured surface areas of -69 to $+86$ per cent. For any experiments that test the theory at all severely, large discrepancies have been reported in the literature. The Kozeny equation seems to work fairly well in the routine determinations of the internal area of industrial powders, but this is probably due to the essential sameness of such powders. Very few of such determinations are cross-checked by other methods.

In conclusion, it may be said that the Kozeny equation represents a valid qualitative relationship that comes out from any tube-model of porous media. It can also easily be deduced by dimensional reasoning. However, its numerical implications cannot be trusted to more than with a factor of ten.

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Transcription of Discussion

DISCUSSION

MR. G. JACQUELIN: I should like to comment briefly on the paper we published three months ago and to which Robertson referred.[1]

Among factors affecting the flocculation of pulp suspensions, we have considered more specifically the action of additives. A particular case is the action of electrolytes, on which there have been relatively few publications. I was very surprised and happy to note that, in spite of the complexity of the problem, differences between techniques and experimental conditions, we agree pretty well with several conclusions of an interesting paper published recently by Andersson,[2] especially upon the action of pH conditions, as follows—

1. pH value may affect flocculation, but in a way that depends on the ions present and on their concentrations. Polyvalent cations seem to be particularly active.

2. For the same suspension, even in a region of turbulent flow, flocculation is generally higher for low pH values and lower for high pH values. In between, a maximum or a minimum may exist depending on the mineral composition of the suspension.

3. The most significant action of electrolytes occurs when there is formation or dissolution of colloidal precipitates (calcium carbonate, aluminium hydroxyde, for instance). As colloidal particles, the fines play an important role, too.

Other points are discussed in the paper such as the action of polymers and vegetable gums, as well as the influence of temperature of flocculation and I refer those interested to the paper itself.

In conclusion, I think it is useful to keep in mind that flocculation problems are not merely mechanical and hydrodynamic problems, but that the physico-chemical aspect of the properties of the fibre surface and of the colloids in the suspension have an important role in the ability of fibres to become entangled, also in the behaviour of flocs in flowing suspensions.

DR. O. ANDERSSON: Fig. 6 shows a relationship between flocculation and rate of flow and, in the turbulent region, you have found a decrease in flocculation as the rate of flow increases. I have made similar measurements

[1] *ATIP Bull.*, 1961, (4), 287–303

[2] Andersson, O. and Brunsvick, J. J., *Svensk Papperstidn.*, 1961, **64** (13), 493–499

on 1 g/litre suspensions of bleached spruce sulphite pulp, beaten to 50° S.R., but the change in flocculation in the turbulent region is very small, if it exists at all. My experiments suggest that the flocculation does not change at all in the turbulent region. I wonder whether you could offer an explanation of this. I want to point out an important difference between your experiments and mine, namely, that my measurements were carried out on photographic plates, which were subject to circular scanning, whereas yours were taken directly from the flowing suspension, using the motion of the suspension for scanning.

DR. A. A. ROBERTSON: Perhaps your own explanation has some validity and the differences in concentration may be important too. We are using 6 g/litre suspensions of a similar stock, but unbeaten. I expect as you do, that the velocity could be reached when the flocculation curve becomes flat. If curves were carried to 1 000 cm/sec, they would flatten out. Perhaps the flocculation effect could be correlated with the pressure loss data and it might be found that a maximum deflocculation is obtained when the friction losses for stock and water become nearly equal, beyond point E in Fig. 1 of the Cambridge symposium paper.(28) Earlier work(34) has shown, in fact, that the flocculation curves are flat for the more dilute suspensions in turbulent flow.

A DELEGATE: Once the flocs are broken down, it seems that the measurements always give the same answer.

DR. ROBERTSON: We are satisfied from our own work that, in the turbulent flow region and even in the latter parts of the transition region, a dynamic flocculation-deflocculation equilibrium exists, because the curve in Fig. 6 can be approached from two directions. That is to say, by very violent stirring in the head box at the entrance to the pipe, one can create a better dispersion than the equilibrium dispersion in the pipe or, by slowing down the stirring, one can introduce a rather flocculated suspension. Independent of the nature of the entry conditions, the curve is reproduced.

PROF. J. D'A. CLARK: Dried unbeaten pulp suspensions give very little flocculation. Immediately beating commences, flocculation of the resulting diluted fibres increases very markedly until the fibres are sufficiently shortened. The secret is its rapid initial fibrillation. That is a most important point, so when talking about unbeaten fibres, I would ask how unbeaten are they?—since the primary wall may or may not be present.

DR. ROBERTSON: The stock I was using was a commercial pulp dispersed from the dry condition, so initial fibrillation was presumably at a minimum. I do not know the history of the pulp, but we have run the same pulp in an apparatus for nearly a month and the characteristics of the curve changed very little during that time because of any beating action that the centrifugal pump may have given.

DR. J. A. VAN DEN AKKER: Perhaps the difference between the results obtained in the two laboratories is to be found in differences in the distance between the inlet to the duct and the point of observation.

DR. ROBERTSON: Fig. 14 of one of our earlier papers[29] has some relevance here.

THE CHAIRMAN: Dr. Andersson, do you wish to comment on this question of position?

DR. ANDERSSON: I measured the turbulence at the same time and, after that region, I could just as well make it a co-ordinate showing a root mean square fluctuation.

PROF. B. STEENBERG: Do we really have to look for the mental differences? After all, it may be that your scanning rate was provided by the rate of flow, changed while the scanning rate in the Attwood and Andersson case was independent of the rate of flow. Before that point is studied, we need not look for any more complicated explanations.

DR. ROBERTSON: If Steenberg is suggesting a lack of adequate frequency response of our instruments, this has been checked and it was flat over the frequency spectrum present.

PROF. STEENBERG (*written comment*): I do not dispute your frequency response. The scanning rate in the Attwood and Andersson case is constant; but, in the Robertson case, there is a complicated distribution of scanning rates provided by the motion of the fibres in the turbulent system. In the first case, scanning rate is an independent variable; in the latter case, it is dependent on the properties of the flowing system.

DR. N. K. BRIDGE: The origin of the 2 in streaks seen in the papers from many different machines and repeated in your paper is very interesting. Have you any explanation for them yet?

DR. ROBERTSON: We included mention of it in the manuscript in the hope that somebody else would explain this phenomenon.

DR. BRIDGE: Could they be caused by a hydrodynamic condition due to the width of the slice being very much the same on all machines?

DR. ROBERTSON: No, I would hestitate to say that. We have had several levels of comment in this symposium, starting off with quantitative relations and semi-quantitative relations, qualitative relations and semi-qualitative relations. Jayme has introduced the 'detection of trends' as being at a slightly lower level than this. In order to comment on this particular question, we would have to recognise lower levels still. I suggest that some of these levels have been occupied already. They might be called informed prejudice, semi-informed prejudice and ignorant prejudice. Any comment from me would be in the lowest order; however, I think that some studies of the stability of wide, flat jets are warranted.

DR. G. BURKHARD: Recently, some new evidence of the cause of these streaks came to our attention. I am now referring to machine A–1 in Table 2 of the paper, on which very dominant streaks have developed from time to time. It is observed that on regrinding the couch the formation of streaks disappeared for some time. Furthermore, by lifting the presser roll, the recurrence of the streaks was delayed. When streaks appeared, the wire showed a distinct streaky wear pattern corresponding to the spacing of the streaks in the paper. Since nothing else seemed to correspond to the streak spacing, attention was then focused on the couch and it was found that the couch drilling pattern—or better the couch drilling pitch—corresponded very well. In examining the couch, it was found that, at every fourth hole in the axial direction, the land between holes was slightly wider than between the other holes. This is due to backlash in the couch drilling.

It is thought that this slightly wider land at a drill spacing of $2\frac{1}{4}$ in (on that couch) could well cause a non-uniform wear on the wire, owing to uneven slip and load distribution. The observations made on that machine seem to sustain this theory.

DR. ROBERTSON: Suspicion has been cast on almost everything else, but the couch roll pattern has never been mentioned as suspect before.

MR. T. ULMANEN: Do you not think that differences in the degree of swelling of different fibres have an influence on the shape and the location differences of the similar curves as given in Fig. 5?

Very often, we speak only of the purely mechanical characteristics of a fibre—such as the length, the ratio between wall thickness and the diameter of the fibre—being very important. Very seldom, however, do we mention the degree of swelling of a fibre as an important variable when investigating those rheological phenomena in which the interaction between fibres plays a considerable role.

I thought that differences in the degree of swelling of used fibres might partly explain the discrepancies between the curves worked out at the two laboratories.

DR. ROBERTSON (*written comment*): We recognise the importance of fibre swelling in its effect on the mechanical properties of fibres and in the calculation of volume concentration of suspensions, but we have not yet been able to demonstrate its explicit influence in pipe flow.

DR. ANDERSSON: A report of our recent findings is to be published later on. We have an apparatus designed by one of my colleagues for determining the stress/strain curves of pulp suspensions. In these, the strain is very small and it looks just like the stress/strain curve of paper.

DR. VAN DEN AKKER: May I speak further about the difference between the results of Robertson and Andersson? It has ofter been pointed out that turbulence does at least two things in a fibre suspension—it tends to bring fibres together, thus creating flocs and it tends also to pull them apart or disperse them. These two effects exist simultaneously. If one thinks of the two actions in terms of curves, what one observes is the sum of two curves, one going up, the other going down; possibly all that is happening here is that in Andersson's laboratory one effect is just enough stronger than the other that an essentially horizontal line is obtained, whereas in Robertson's laboratory conditions are different and the relative importances of the two effects are such that a sloping line is obtained.

MR. D. ATTWOOD: We have experimental evidence in support of Andersson's results and we too would expect a straight line for fully developed turbulence. We are obtaining our results in a similar way, by photographing the tube.

DR. S. G. MASON: When this work was well on, we thought that the properties of pulp fibres suspension were unique. It has recently been brought very forcibly to our attention that the flow properties of human blood in the veins and arteries have a very close relationship to those of pulp suspensions as described by Robertson. The connecting link between the two systems is that, in pulp suspensions, we are dealing basically with flexible prolate-spheroidal (thread-like) particles and, in human blood, we have a suspension of flexible oblate-spheroidal (disc-like) particles. It so happens that the mathematics of the two particle systems are identical.