**D. A. Nield**
Associate Professor,
Department of Engineering Science,
University of Auckland,
Private Bag 92019, Auckland, New Zealand
e-mail: d.nield@auckland.ac.nz

# Alternative Models of Turbulence in a Porous Medium, and Related Matters

*Recently published papers involving two distinct models of turbulence in a porous medium are discussed, together with related matters including inertial effects, lateral momentum transfer and spin-up, nonlinear drag, and the detection of the onset of turbulence.*
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## 1 Introduction

After mentioning several of their papers in which turbulent transport equations for porous media were developed based on the generalized Volume Averaging Theory (VAT) for highly porous media, Travkin et al. [1] wrote (page 2), ''Antohe and Lage [2] presented a two-equation ... turbulence model for incompressible flow within a fluid saturated and rigid porous medium that is the result of incorrect procedures.'' It is regrettable that Travkin et al. did not find room in their paper to explain why they considered those procedures to be incorrect, and the reader is left to guess that any procedures that are not based on VAT must be incorrect. Travkin et al. [1] proceeded to derive their own form of the kappa-epsilon equations, displayed as their Eqs. (35) and (37). These complicated equations contain various integrals, and there is no indication in the paper of how closure is to be completed, despite the claim (page 6) that ''closure examples are given.'' In a signed review of an earlier version of this paper, Dr Travkin argues that his complicated equations are correct, and therefore that the approximate equations of other people must be wrong. It appears that he does not appreciate that in order to make practical progress it is necessary to make approximations. In his review, Dr Travkin admits that he has been unable to solve completely the closure problem for the VAT equations.

A more informative treatment of the matter is that by Nakayama and Kuwahara [3]. On page 427 they wrote:

''Recently, two distinct two equation turbulence models have been established for turbulent flows in porous media. Antohe and Lage [2] chose to carry out the Reynolds averaging over the volume-averaged macroscopic equations to derive two-equation turbulence model equations, whereas Masuoka and Takatsu [4] derived a macroscopic turbulence transport equation by spatially averaging the turbulence transport equation of the two-equation turbulence model. Antohe and Lage [2] examined their model equations for the turbulence kinetic energy and its dissipation rate, assuming a unidirectional fully-developed flow through an isotropic porous medium. Their model demonstrates that the only possible steady state solution for the case is ''zero'' macroscopic turbulence kinetic energy. This solution should be re-examined, since the macroscopic turbulence kinetic energy in a forced flow through a porous medium must stay at a certain level, as long as the presence of porous matrix keeps generating it. (The situation is analogous to that of turbulent fully-developed flow in a conduit.) Also, it should be noted that the small eddies must be modeled first, as in the case of LES (Large Eddy Simulation). Thus we must start with the Reynolds averaged set of the governing equations and integrate them over a representative control volume, to obtain the set of macroscopic turbulence model equations. Therefore, the procedure based on the Reynolds averaging of the spatially averaged continuity and momentum equations is questionable, since the eddies larger than the scale of the porous structure are not likely to survive long enough to be detected. Moreover, none of these models has been verified experimentally.''

Nakayama and Kuwahara [3] go on to describe their own work:

''The macroscopic turbulence kinetic energy and its dissipation rate are derived by spatially averaging the Reynolds-averaged transport equations along with the $k-\varepsilon$ turbulence model. For the closure problem, the unknown terms describing the production and dissipation rates inherent in porous matrix are modified collectively. In order to establish the unknown model constants, we conduct an exhaustive numerical experiment for turbulent flows though a periodic array, directly solving the microscopic governing equations, namely, the Reynolds-averaged set of continuity, Navier-Stokes, turbulence kinetic energy and dissipation rate equations. The microscopic results obtained from the numerical experiment are integrated spatially over a unit porous structure to determine the unknown model constants.

The macroscopic turbulence model, thus established, is tested for the case of macroscopically unidirectional turbulent flow. The streamwise variations of the turbulence kinetic energy and its dissipation rate predicted by the present macroscopic model are compared against those obtained from a large scale direct computation over an entire field of saturated porous medium, to substantiate the validity of the present macroscopic model.''

It is the view of the present author that Nakayama and Kuwahara [3] have presented clearly and forcefully their case that their model is superior to that of Antohe and Lage [2], and in many respects their paper is admirable. However, there are some questionable aspects of their arguments and these are discussed in the following section. Further, there are two general questions that should be raised. First, is the turbulence discussed by Nakayama and Kuwahara true macroscopic turbulence? Second, how reliable are conclusions based on volume averaging? In the remainder of the present paper, the author presents his answers to these questions and discusses related matters, such as the way in which inertial effects should be modeled, and the nature of momentum transfer and spin-up in a porous medium.

For a review of earlier papers on macroscopic turbulence in permeable media the reader is referred to Lage [5].

## 2 Some Specific Comments on the Nakayama-Kuwahara Model

Having integrated the Reynolds averaged equations over a ''control volume,'' Nakayama and Kuwahara [3] obtain their momentum equation, Eq. (11). This is unexceptional. However, they then proceed to replace the last two terms of Eq. (11) by Darcy and Dupuit-Forchheimer terms to obtain Eq. (14). This replacement is a standard procedure for laminar flows, but it appears to the present author that the replacement is highly questionable in

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the context of turbulence modeling. In the paragraph containing Eq. (14) those authors wrote, ''In the numerical study of turbulent flow through a periodic array, Kuwahara et al. [6] concluded that the Forchheimer-extended Darcy's law holds even in the turbulent flow regime in porous media.'' That too is an acceptable statement, but it does not justify the transition from Eq. (11) to Eq. (14). For one thing, Eq. (14) is substantially different from the standard Forchheimer-extended Darcy equation. Furthermore, there is a gap in the argument in proceeding from an equation for the turbulence regime in a bulk form, in which the total pressure-drop is related to the bulk fluid speed via an expression quadratic in the velocity, to an equation involving a differential expression. (The reader should note that this comment has nothing to do with whether or not differential operators are used in ''macroscopic'' or integral equations.) In summary, it seems to the present author that Nakayama and Kuwahara [3] have made an assumption of a relationship between microscopic turbulence and macroscopic drag that cannot be justified except in the gross sense that for high Reynolds number the Forchheimer term will be dominant.

Because of their assumption of periodicity when performing their numerical calculations, Nakayama and Kuwahara [3] were unable to treat eddies on a scale larger than their period length. They treated a geometry that was periodic in both the $x$- and $y$-directions with period 2H. Within a period cell, they considered solid obstacles having cross-section a square of side-length $D$, and they selected the ratio $D/H$ so that the porosity $\phi$ lay between 0.2 and 0.9. That means that $D$ and $H$ were of the same order. In other words the period length for the numerical calculations was of the same order of magnitude as the particle diameter, and so representative of the pore scale. Eddies with a diameter greater than $2H$ cannot be accommodated on their model, simply because they are larger than the period length. In other words, global eddies (those with a diameter lager than the pore scale) were filtered out because of the assumptions about periodicity. Thus global eddies were ruled out a priori. It is true that a medium does not have to be periodic to have the Nakayama-Kuwahara model applied to it, but when the periodicity imposed is that of the pore scale then this is a severe restriction.

Further, there is a fundamental difficulty with any model in which time-averaging (Reynolds averaging) is followed by volume averaging. That procedure precludes the incorporation of the interaction between fluctuating quantities and the solid matrix of the porous medium, other than the minor effect of fluctuations in pressure and shear stresses along the interfacial solid-fluid area. This aspect was clearly stated by Antohe and Lage [2].

Also, it appears that Nakayama and Kuwahara [3] may have misinterpreted the ''zero'' turbulence conclusion (for fully developed turbulent flow) of Antohe and Lage [2]. The Antohe-Lage result says nothing about the existence or otherwise of microscopic turbulence, and its failure to do so should not be used as negative criticism of the model.

Moreover, it would have been advantageous to Nakayama and Kuwahara to recognize that the turbulence kinetic energy used in their own paper defined as a volume-average of the microscopic turbulence kinetic energy) is different from that used by Antohe and Lage (defined as the time-averaging of the square of the volume-averaged fluid velocity fluctuations).

This difference has been highlighted by the work of Pedras and de Lemos [7], and de Lemos and Pedras [8]. The analysis in these papers leads to the conclusion that the two approaches, time-averaging the volume-averaged equations and volume-averaging the time-averaged equations, lead to similar equations because of a commutative property of the two averaging operations. The authors explicitly demonstrate (Eq. (50) in [7]) that there is a difference between the expressions used to denote turbulent kinetic energy used in the two classes of models. The Pedras and de Lemos approach may be regarded as more complete, in a sense, than the Antohe-Lage approach, because clearly it has regard for pore turbulence and it does say something (but not very much) about large scale turbulence.

## 3 Macroscopic Turbulence in a Porous Medium

The author has over a period of some years expressed the view (Nield [9], Nield and Bejan [10]) that it is important to distinguish between turbulence in the pores of a porous medium and turbulence on a macroscopic scale (the global scale, that of the apparatus in an experiment). For example, Nield ([9], page 271) wrote that ''A further consequence of our physical argument is that true turbulence, in which there is a cascade of energy from large eddies to smaller eddies, does not occur on a macroscopic scale in a dense porous medium.'' (The physical argument is presented again below.) The author believes that subsequent investigations have shown results that are consistent with the quoted statement. For example, as Nakayama and Kuwahara [3] highlighted in the passage quoted above, the model of Antohe and Lage [2] leads to the conclusion that the only possible steady-state solution for the case considered by them is zero macroscopic turbulence kinetic energy.

The model of Nakayama and Kuwahara [3] is concerned with the effect of turbulence within the pores, and not with true macroscopic turbulence. In particular, their numerical experiments involve a model that is periodic on the pore scale, and this means that (as I have already noted) global eddies are ruled out a priori. On the other hand, the model of Antohe and Lage [2] does deal with macroscopic turbulence in a sensible fashion. Of course, as already noted above, the Antohe-Lage model says nothing about the turbulence within the pores.

This means that we have two classes of models. One class of models (that includes the Nakayama-Kuwahara model) describes the effect of turbulence within the pores. The second class, exemplified by the Antohe-Lage model, describes turbulence on a global scale. For the case of dense porous media (characterized by small values of the Darcy number, and generally with porosities less than 0.5) the first class of models will generally be the more useful for dealing with what turbulence there is, but for the case of large Darcy number (and in particular for hyperporous materials (Nield and Lage, [11]) such as metallic foams) the second class of models will be the more appropriate.

## 4 Limitations of the Volume-Averaging Approach

At first sight, the method of volume-averaging is a rigorous procedure, as Travkin et al. [1] claimed it is. It is indeed a rigorous procedure, but only up to the stage at which the system of equations is closed. In order to make practical progress, approximations have to be made to evaluate certain integrals, and from then on the procedure is not rigorous. It is inevitable that physical information is lost at the closure stage. (See, for example, the discussion of the ''filter'' in Sections 1.3.4 and 1.6.4 in Whitaker [12].)

In performing the closure one is guided by physical experience. In other words, the closure process is a semi-empirical matter, and the usefulness of the final model is critically dependent on the skill that one employs at the closure stage. This matter is discussed further in the next section.

## 5 Modeling Inertial Effects

It is relatively simple to perform averaging over terms that are linear in the dependent variables, but nonlinear terms, such as the convective (advective) inertial term $(\mathbf{V}\cdot\nabla)\mathbf{V}$ in the Navier-Stokes equation, cause difficulties. (Here $\mathbf{V}$ denotes the fluid velocity.) Even in the case of laminar flow there has been controversy over the best way to model inertial effects in a porous medium. There is agreement that these effects lead to a quadratic drag term, usually called the Forchheimer drag term, though on historical grounds (Lage [5]) it is more accurate to call this the Dupuit-Forchheimer term. However, there has been disagreement about the coefficient of this term (Antohe and Lage [13]) and whether or not one should also include simultaneously a convective inertial term $(\mathbf{v}\cdot\nabla)\mathbf{v}$ indicated by formal volume-averaging (where $\mathbf{v}$ denotes the Darcy velocity) in the resulting momentum equation, when modeling a medium of low porosity (Nield [9]).

It was noted by Nield [14] that at least the irrotational part of this term needs to be retained in order to account for the phenomenon of choking in the high speed flow of a compressible fluid, but he suggested that the rotational part, proportional to the intrinsic vorticity, be deleted. His argument was based on the expectation that a medium of low porosity will allow scalar entities like fluid speed (the magnitude of the velocity) to be freely advected, but will inhibit the advection of vector quantities like vorticity. Nield and Bejan [10] went a step further, and suggested that even when vorticity is being continuously produced (e.g., by buoyancy) one would expect that it would be destroyed by a momentum dispersion process due to the solid obstructions.

An argument providing further support for this point of view will now be presented. There are some subtleties about the effect of the inertial terms on motion in a porous medium. The power of the total drag force (per unit volume) is equal to the rate of viscous dissipation (per unit volume); for a detailed discussion see Nield [15]. The Forchheimer drag term, although it appears to be independent of the viscosity, contributes to the viscous dissipation. The effect of inertia is mediated via a change in the pressure distribution and the velocity distribution. The flip side of the coin is that when one closes the system of equations by introducing a Forchheimer drag term one should not assume that the convective inertia term that remains in the momentum equation is identical with that obtained by formal volume-averaging. After integration, it should lead to the correct expression for the averaged kinetic energy, which involves the magnitude but not the direction of the velocity, and this means that the irrotational part of the volume-averaged convective inertial term must be unchanged, but the rotational part is not determined by the averaging process, and there is no inconsistency in setting it to zero as part of the closure process.

In the process of performing the closure after volume-averaging, it has been traditional to adjust for the contribution to the overall drag force, that includes a quadratic drag force that has a specific direction (parallel to the Darcy velocity in the case of an anisotropic medium), but to ignore the fact that one also needs to adjust for the fact that the overall moment of the force system has to be zero. It is now being suggested that an appropriate adjustment is simply to set to zero the irrotational part of the volume-averaged convective inertial term.

It has sometimes been claimed that the retention of the convective inertial term is necessary in order to account for the formation of hydrodynamic boundary layers in channel flow, and in order to estimate the entrance length, but this is not correct. The formation of such layers is primarily due to the action of viscous diffusion, and the entrance length can be estimated using the time-derivative inertial term.

## 6 The Lateral Transfer of Momentum, and Spin-Up in a Porous Medium

A related matter is the extent to which it is possible to transmit longitudinal momentum in a transverse direction in a dense porous medium. Nield [9] discussed the case of a special medium in which the pores consist of channels along the *x*-, *y*-, and *z*- directions. He pointed out that if one forces fluid to flow down a single *x*- channel, that will cause flow along intersecting *y*- and *z*- channels but will not produce any significant flow on the average in neighboring *x*-channels. A consequence is that, on physical grounds, one would expect that it should be difficult to produce significant motion in the bulk of a porous medium, with a fixed solid matrix, by moving just a rigid boundary. Rather, one would expect significant motion to be confined to a thin layer near the boundary. As Nield [16] showed, by consideration of a fixed circular cylinder of a porous medium surrounded by a rotating sleeve, that is indeed the form of motion predicted when one solves a momentum equation containing the Brinkman term but with the convective inertial term omitted. On the other hand, if one includes the convective inertial term then one has an equation similar to the usual Navier-Stokes equation, and this leads to the prediction that all the fluid within the porous medium will ultimately be set in motion. As far as the author is aware, no one has yet performed an experiment in order to test the prediction.

## 7 The Onset of Turbulence

**(i) The Relationship Between Quadratic Drag and Turbulence.** The topic of transition to turbulence in porous media is among the interesting topics reviewed by Masuoka [17]. He refers in particular to the experimental work discussed by Dybbs and Edwards [18], and to his own work reported by Masuoka and Takatsu [4] and by Takatsu and Masuoka [19]. Nield [20] and Antohe and Lage [13] have pointed out that the work of Masuoka and his colleagues is based on a misconception about the identity of the onset of turbulence and the Forchheimer drag term taking significantly large values. Antohe and Lage [13] have also emphasized the need to use a proper definition of Reynolds number in characterizing these phenomena.

**(ii) The Detection of the Onset of Turbulence.** The determination in an experiment of the critical Reynolds number at which turbulence appears is not a straightforward matter. In a personal communication to the author, Dr. J. L. Lage has pointed out that in a porous medium of conduit type, in which the pore space consists essentially of tubes of varying cross-section, there is the possibility of relaminarization, in some portions of the tubes (after divergence), of turbulence that appears in other portions (after convergence). Ideally, one would like to put probes in the narrowest part of the tubes, and of course that is difficult in practice and almost certainly has not been achieved in experiments reported to date. Also, it should be noted that the appearance of a signal chaotic in time at a single position is probably an excellent indication, but not conclusive evidence, of the onset of turbulence. One needs to observe also what is happening at a neighboring point in order to be sure that turbulence is occurring.

The Lage argument is based on the fact that, for constant volume flux through a tube, the mean velocity is inversely proportional to tube cross-section, and hence inversely proportional to the square of the tube diameter. The local Reynolds number, which involves the product of the mean velocity and the tube diameter, is thus inversely proportional to the tube diameter. That means that in the wider portions of the tube, the local Re value may drop below the critical value necessary to maintain the turbulent state. In other words, relaminarization may occur. On the same argument, the onset of turbulence is likely to occur first in those parts of the channel where the local Re is highest, namely in the narrowest part of the tubes.

A referee pointed out that while the association between high local Re and turbulence is appropriate for a tube of constant cross-section, it might not be applicable to ducts with varying cross-section. It is known that for high Re flows in aerodynamics, turbulence is seldom encountered except close to solid walls (where it can be generated). In converging sections of ducts, fluid layers close to the solid tend to run faster and flatten the time-averaged velocity profile. Consequently, less mechanical energy is converted to turbulence, because production of turbulent kinetic energy is dependent on the mean velocity gradients. In this case, in accelerated flows, relaminarization may occur. When the flow crosses an enlargement, layers close to the wall are subjected to a positive pressure gradient and tend to run slower than the core of the flow, and separation may even occur. The referee noted that as a consequence, turbulence production is enhanced and levels of turbulent kinetic energy are increased, as found experimentally by Spencer et al. [21].

However, it is not clear to the present author to what extent results for high Re aerodynamics carry over to flow in porous media, and further investigation is desirable.


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Several of the author's ideas on this subject have developed as a result of discussions/debates over the years with Dr J. L. Lage of Southern Methodist University.


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# Discussion: ''Alternative Models of Turbulence in a Porous Medium, and Related Matters'' (D. A. Nield, 2001, ASME J. Fluids Eng., 123, pp. 928–931)


**V. S. Travkin**
Mechanical and Aerospace Engineering Department, UCLA, Los Angeles, CA 90095-1597


*The paper which I authored is mentioned first by Prof. Nield [1]. I would like to make some preliminary comments about that citing in the paper by Nield, because the length of a paper which is presented to a conference like the 3rd ASME/JSME Fluids Engineering Conference in 1999, is usually restricted to 6 pages. That is the reason we could not include discussion or critics of other studies, but focused primarily on our results.*


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The paper by Antohe and Lage [2], cited by Prof. Nield, needs comments on turbulent transport in porous media. The equations derived by Antohe and Lage in their paper appeared to be based on a set of phenomenological equations that are themselves the result of assumptions and simplifications. The development of a set of equations that are rigorous does not allow one to use correlation based models developed by others that are themselves based on approximate conceptions of what the physical processes are dependent on. These models or terms in the equations already include many observed effects. After all, that was their purpose. It is inadmissible for one to include such correlations in the Navier Stokes equations, as was done by Antohe and Lage, because this results in the effects being included in the governing equations twice.

A number of serious deficiencies are found in that paper, including the following:

1 The authors initial set of equations are based on the assumption that the turbulent fluctuations and fluctuations caused by the porous medium are of the same nature. They are not, and serious error can result if they are assumed to be the same.

2 Given the above observation and other issues of development, the conclusions presented in the abstract of the paper that ''Among them, this conclusion supports the hypothesis of having microscopic turbulence, known to exist at high speed flow, damped by the volume averaging process. Therefore, turbulence models derived directly from the general (macroscopic) equations will inevitably fail to characterize accurately turbulence induced by the porous matrix in a microscopic sense,'' are not correct. Before one can reach such conclusions, the derivations of the equations upon which it is based must be valid.

Regarding the need for approximations mentioned in the first paragraphs by Prof. Nield, I appreciate and respect the desire to use approximation, but it must be a correct approximation, substantiated. When someone derives an incorrect sum of suggestions, model, governing equations and then makes an approximation of that model-it makes no sense to consider values of this ''approximation.''

In application to the VAT turbulent transport in porous media, the words ''in this review...unable to solve completely the closure problem for the VAT equations'' are not completely accurate. As a matter of fact, the papers by Shcherban et al.[3] and Primak et al. [4] were the first correct studies on VAT in turbulent transport in porous media. In these studies and others in Russian were first published two critical features: