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

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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:

1 basics for turbulent fields decomposition and treatment in VAT averaging [3];
2 fundamentals of averaging of nonlinear operators of class $\nabla\cdot(\nu_T\nabla\{U\}_f)$.

Since those two papers with experimental closure of VAT turbulent equations, I have published a number of papers with exact, rigorous closures (in a general interest morphologies-as capillary or globular) [5–12], as well as with approximate closures using experimental data [6,10,13–17]. In addition, we recently performed experimental studies on micro-heat exchangers, doing for the first time a data reduction methodology based completely on VAT [18]. All this is done by using the closure methods.

Over many years I've been watching the situation where people from different countries have proposed developing some "new" theoretical construction for modeling of turbulent transport in porou media. Their papers are published because there is a lack of advancement and lack of experts in this area. The mathematics and equations of VAT have yet to become part of a convenient university knowledge base and are too unfamiliar to understand or to make a close consideration. It seems one needs to blame the lack of university education plus not enough volume of solved problems.

These thoughts were behind the reason and idea of the second of our presentations at the 3rd ASME/JSME Fluids Engineering conference, "A Critique of Theoretical Models of Transport in Heterogeneous Media," [19]. This specifically addressed those issues mostly by criticizing various developments in heterogeneous and multiphase transport theories and modeling. Returning to the paper by Prof. Nield, I have no choice but to talk about those papers and studies that already were addressed in the presentation. One of them is the previous work by F. Kuwahara and A. Nakayama [20].

In this work, the Detailed Micro-Modeling Direct Numerical Modeling\(DMM-DNM)-solution of 2D problem of uniformly located quadratic rods with equal spacing in both directions is delivered and analyzed. There the Forchheimer and post-Forchheimer flow regimes were studied. This work is a good example of how DMM-DNM goals cannot be accomplished even if the solution on a microlevel is obtained completely, when the proper volume averaging theory (VAT) scaling procedures basics are not known and applied. The one structural unit periodic cell in the medium was taken for DMM-DNM. Equations were taken with constant coefficients and in phase one the VAT equation was written for the laminar regime as

$$\langle m\rangle(\rho c_p)_f\frac{\partial\tilde{T}_f}{\partial t}+\langle m\rangle(\rho c_p)_f\tilde{U}_i\nabla\tilde{T}_f$$
$$=(\rho c_p)_f\nabla\cdot\langle-\hat{T}_f\hat{u}_i\rangle_f+k_f\nabla\nabla(\langle m\rangle\tilde{T}_f)$$
$$+k_f\nabla\cdot\left[\frac{1}{\Delta\Omega}\int_{\partial S_w}T_f\vec{d}s\right]+\frac{k_f}{\Delta\Omega}\int_{\partial S_w}\nabla T_f\cdot\vec{d}s. \tag{1}$$

Adding this equation to the VAT solid phase (second phase) two temperature equation gives

$$\langle m\rangle(\rho c_p)_f\frac{\partial\tilde{T}_f}{\partial t}+\langle s_2\rangle(\rho c_p)_2\frac{\partial\tilde{T}_2}{\partial t}+\langle m\rangle(\rho c_p)_f\tilde{U}_i\nabla\tilde{T}_f$$
$$=(\rho c_p)_f\nabla\cdot\langle-\hat{T}_f\hat{u}_i\rangle_f+\nabla\cdot(k_f\nabla(\langle m\rangle\tilde{T}_f)+k_2\nabla(\langle s_2\rangle\tilde{T}_2))$$
$$+\nabla\cdot\left[\frac{k_f}{\Delta\Omega}\int_{\partial S_{12}}T_f\vec{d}s_1+\frac{k_2}{\Delta\Omega}\int_{\partial S_{12}}T_2\vec{d}s_2\right]$$
$$+\frac{k_f}{\Delta\Omega}\int_{\partial S_{12}}\nabla T_f\cdot\vec{d}s_1+\frac{k_2}{\Delta\Omega}\int_{\partial S_{12}}\nabla T_2\cdot\vec{d}s_2 \tag{2}$$

which reduces due to interface fluxes equality to

$$\langle m\rangle(\rho c_p)_f\frac{\partial\tilde{T}_f}{\partial t}+\langle s_2\rangle(\rho c_p)_2\frac{\partial\tilde{T}_2}{\partial t}+\langle m\rangle(\rho c_p)_f\tilde{U}_i\nabla\tilde{T}_f$$
$$=\nabla\cdot(k_f\nabla(\langle m\rangle\tilde{T}_f)+k_2\nabla(\langle s_2\rangle\tilde{T}_2))+(\rho c_p)_f\nabla$$
$$\cdot\langle-\hat{T}_f\hat{u}_i\rangle_f+(k_f-k_2)\nabla\cdot\left[\frac{1}{\Delta\Omega}\int_{\partial S_{12}}T_f\vec{d}s_1\right] \tag{3}$$

which has two averaged temperatures $\tilde{T}_f$ and $\tilde{T}_2$, interface surface integrated temperature $T_f(\partial S_{12})$, and two fields of fluctuations $\hat{T}_f(x)$ and $\hat{u}_i(x)$, assuming that the velocity field is also computed and known. One will write further the effective conductivity coefficients for Eq. (3) and for the one-temperature equation when the temperature equilibrium is assumed.

In the first case for the weighted temperature

$$\langle T^w\rangle=(\langle m\rangle(\rho c_p)_f\tilde{T}_f+\langle s_2\rangle(\rho c_p)_2\tilde{T}_2)/w_T, \tag{4}$$

$$w_T=\langle m\rangle(\rho c_p)_f+\langle s_2\rangle(\rho c_p)_2=\text{const}, \tag{5}$$

equation can be written as

$$w_T\frac{\partial\langle T^w\rangle}{\partial t}+\langle m\rangle(\rho c_p)_f\tilde{U}_i\nabla\tilde{T}_f=\nabla\cdot(k_f\nabla(\langle m\rangle\tilde{T}_f)+k_2\nabla(\langle s_2\rangle\tilde{T}_2))$$
$$+(\rho c_p)_f\nabla\cdot\langle-\hat{T}_f\hat{u}_i\rangle_f+(k_f$$
$$-k_2)\nabla\cdot\left[\frac{1}{\Delta\Omega}\int_{\partial S_{12}}T_f\vec{d}s_1\right], \tag{6}$$

where 3 temperatures are unknown $\langle T^w\rangle,\tilde{T}_f,\tilde{T}_2$, plus interface surface temperature integral $T_f(\partial S_{12})$, and fluctuation fields $\hat{T}_f(x)$ and $\hat{u}_i(x)$. The effective coefficient of conductivity can be looked for is

$$k_{\text{eff}}^o\langle\nabla T^w\rangle=(k_f\nabla(\langle m\rangle\tilde{T}_f)+k_2\nabla(\langle s\rangle\tilde{T}_2))+(\rho c_p)_f\langle-\hat{T}_f\hat{u}_i\rangle_f$$
$$+(k_f-k_2)\left[\frac{1}{\Delta\Omega}\int_{\partial S_{12}}T_f\vec{d}s_1\right]. \tag{7}$$

In order to avoid the complicated problem with effective conductivity coefficient definition in a multi-temperature environment Kuwahara and Nakayama [20], while performing the DMM-DNM for problem of laminar regime transport in porous medium, decided to justify the local thermal equilibrium condition

$$\langle T\rangle=\langle m\rangle\tilde{T}_f+\langle s_2\rangle\tilde{T}_2=T^*=\tilde{T}_f=\tilde{T}_2 \tag{8}$$

which greatly changes the one effective temperature equation. This equation becomes simpler with only one unknown temperature T^* and variable field $\hat{T}_f$ and writes as

$$(\langle m\rangle(\rho c)_f+\langle s_2\rangle(\rho c_p)_2)\frac{\partial T^*}{\partial t}+\langle m\rangle(\rho c_p)_f\tilde{U}_i\nabla T^*$$
$$=\nabla\cdot(k_f\nabla(\langle m\rangle T^*)+k_2\nabla(\langle s_2\rangle T^*))+(\rho c_p)_f\nabla\langle-\hat{T}_f\hat{u}_i\rangle_f$$
$$+(k_f-k_2)\nabla\cdot\left[\frac{1}{\Delta\Omega}\int_{\partial S_{12}}T_f\vec{d}s_1\right], \tag{9}$$

as the variable temperature and velocity fluctuation fields $\hat{T}_f$ and $\hat{u}_i$ should be known, although this is a problem. As long as the definition of effective conductivity coefficient is

$$k_{\text{eff}}^*\langle\nabla T^*\rangle=k_f\nabla(\langle m\rangle T^*)+k_2\nabla(\langle s_2\rangle T^*)+(\rho c_p)_f\langle-\hat{T}_f\hat{u}_i\rangle_f$$
$$+(k_f-k_2)\left[\frac{1}{\Delta\Omega}\int_{\partial S_{12}}T_f\vec{d}s_1\right], \tag{10}$$

then the effective conductivity can be calculated subject to known T^*, $\hat{T}_f$, T_f, and $\hat{u}_i$. One important issue here also is how is the temperature fluctuation $\hat{T}_f$ being calculated? By using the correct phase temperature T_f of calculating the difference with T^*? At the same time, an important issue is that in DMM-DNM, the assumption of thermal equilibrium has no sense at all, as long as the problem has been already calculated as a two temperature problem. In this situation it is not clear what kind of temperature was used in the calculations of VAT terms and assessments of characteristics. If one temperature was used it was used instead of the already calculated two temperatures, which would be preferred. To further perform the correct estimation or calculation of effective characteristics, one needs to know what are those characteristics, in terms of definition and mathematical description of the model?

This is one more place where the DMM-DNM as it is performed now is in trouble if it does not comply with the hierarchical theory derivations and conclusions as the VAT. As shown above, only the requirement of thermal equilibrium warrants the equality of steady-state and transient effective conductivities in a two-phase medium. Consequently, if taken correctly the two-temperature model will cause more trouble in treatment and even interpretation of the needed bulk, averaged temperature (as long as this problem is already known and treated in nonlinear and temperature dependent situations), and corresponding effective conductivity coefficient(s).

Thus, comparing the two effective conductivity coefficients Eq. (7) and Eq. (10), one can assess the difference in the second term form and consequently, the value of the computed coefficients. Comparing the expressions for one equilibrium temperature and one effective weighted temperature, as well as for their effective conductivity coefficients, one can also observe the great imbalance and inequality in their definitions and computations.

Summarizing the application of DMM-DNM laminar regime approach by Kuwahara and Nakayama [20], it can be said that it is a questionable procedure to make an assumption of equilibrium temperatures when the problem was stated and computed as via DNM for two temperatures. In the calculation of the effective coefficients of conductivity, stagnant thermal conductivity k_e; tortuosity molecular diffusion k_{tor}; and thermal dispersion k_{dis}, Kuwahara and Nakayama used the questionable procedure for calculation of the latter two coefficients. They used a one cell (REV) computation for surface and fluctuation temperatures for the periodic morphology of the medium, and at the same time they used the infinite REV definition for the effective temperature gradient for their calculation. That results in the mixture of two different scale variables in one expression for effective characteristics. This is an inconsistency and if used consciously it should be stated explicitly, because it alters the results.

Returning to the question of the correct approach to turbulent transport modeling in a heterogeneous environment, I would point out that the first to really address the problem were not workers in fluid mechanics (those primarily diagnosed the problem), but in meteorology and particularly in agro-meteorology [3,4,8]. One who is interested in the field should also read first the book by Monin and Yaglom [21] who are primarily meteorologists. In that book, substantial discussion is provided on the subject of equations of kinetic energy and dissipation rate when a group of obstacles are encountered in a flow.

Nield states, ''It is the view of the present author that Nakayma and Kuwahara [22] 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.'' I just want to agree with this remark by Nield about the paper of Antohe and Lage, and at the same time I am not surprised by the comment of Prof. Nield on the paper by Nakayma and Kuwahara [22]. The reason is still the same. There is no understanding of the correct averaging theory and the correct VAT equations for nonlinear problems to which those averaging operators have been applied.

Furthermore, I need to make some shorter remarks on this paper [22]. One of the biggest problems with this kind of model and a similar one by Masuoka and Takatsu [23], is that the authors cannot do the correct averaging of the right-hand side of the equations. Those equations are highly nonlinear, on both sides. The theory of half-linear conservative equations averaging is well developed in the studies by Prof. S. Whitaker, yet these authors do not refer to his work, in spite of the fact that most of their equations were developed by Whitaker many years ago and are known to a reading public.

Nakayama and Kuwahara [22] do averaging which we find questionable even in the brackets of laminar equations averaging technique. For example, they apply intrinsic averaging to vectorial functions equations. Further, in the averaged momentum equation (11) on p. 428 they incorrectly define the second

$$\frac{\partial}{\partial x_j}\left[(\nu+\nu_t)\left(\frac{\partial\{\overline{u_i}\}_f}{\partial x_j}+\frac{\partial\{\overline{u_j}\}_f}{\partial x_i}\right)\right],$$

and the third terms in r.h.s.

In the averaged equation of fluid temperature (15) they incorrectly derived the first

$$\frac{\partial}{\partial x_j}\left[\left(k_f+\frac{\rho_f c_{pf}\nu_t}{\sigma_T}\right)\left(\frac{\partial\{\overline{T}\}_f}{\partial x_j}\right)\right],$$

the second and third r.h.s. terms; in the equation of solid temperature (16) the second r.h.s. term is incorrect; in the averaged kinetic energy $\{k\}_f$ equation (20) incorrect are-the first, second, fourth and fifth r.h.s. terms and in the dissipation rate $\{\varepsilon\}_f$ equation (21)-the first, second, third, fourth and fifth r.h.s. terms are incorrect.

In their further development, these authors used those equations for numerical solution through the DMM-DNM to assess the value of turbulent viscosity of the 2D square rod medium in a cross-flow. Also, they applied a thermal equilibrium condition.

Prof. Nield is right when he speaks out about the frivolous change of a few additional terms in the right-hand side for the Darcy-Forchheimer like terms. This action is so often undertaken in many papers, and is one which is incorrect.

When Nield is writing about the interaction between fluctuating quantities and the solid matrix of the porous medium, he should be referring only to models by Masuoka-Takatsu [23] and Nakayama-Kuwahara [22], because this aspect is naturally dropped from their models. That is not relevant to the theory published by Travkin and co-authors. It is necessary to note that the statement about ''global eddies are ruled out a priori'' is inaccurate with respect to the theory published by Travkin and co-authors. There is no limitation by periodicity in this theory, but vice versa. Both scale fluctuations are allowed to exist: fluctuations for turbulence within the pore which is controlled by the pore and fluctuations of the upper scale which are controlled by the whole morphology of the medium. That means that the models of Nakayama and Kuwahara, Masuoka and Takatsu and those by Travkin and co-authors are not of the same type, as Prof. Nield writes.

In Section 4 we read about closure of the averaged equations. Prof. Nield seems to have knowledge that this closure happened to be provided only on the basis of substitution by experimental data (which is still highly reliable method as long as the data reduction is done correctly). This statement is not credible. When the closure is provided on a rigorous basis, which is actually done in some of our works [[5,7,15,24], etc.], there is no question at all about the correct form of the final governing equations. It is not to say that the closure problem for the VAT equations is solved completely. Of course, it is not even close to a final determination, but the ways and means already have substantial progress.

I question the statement ''It is inevitable that physical information is lost at the closure stage.'' I would say that, on the contrary, every detail of the microscale process can be preserved and serve as an input to the upper scale model if the closure is provided correctly, which we addressed in a few papers. In VAT closure modeling, there exists the situation when there are few methodologies to close the additional integral, integro-differential and differential terms in VAT equations. The most widely known and used is the method of linear approximation of fluctuation variables, introduced first and developed by S. Whitaker. The methods used in studies by Travkin and co-authors [3,4,7,10,13,24–28] are those one can use with either the two-scale modeling or experimentally obtained coefficients, for example, of resistance and heat transfer. I would not say that it is a "semi-empirical matter." Some of the considerations and analysis by Prof. Nield on closure problems seem to be from guessing, because the process of closure of VAT equations, and even the existence of some terms which should be closed in nonlinear and turbulent transport equations, is unknown to most of authors whose papers we are discussing here.

As to the developments by Pedras, de Lemos and co-authors mentioned by D. Nield, I would like to make a few comments on the question of the transposition (commutativeness) of Volume-Time averaging or Time-Volume averaging. It appears from the papers co-authored by de Lemos that this question is settled in a favor of the transposability of the components of two sequences. Many factors would contradict this, for example:

1 REV (volume) averaging of Navier-Stokes equations with the large $\mathrm{Re} \geqslant 5 \cdot 10^2$ in porous media makes no sense (turbulence already appeared).

2 The closure, the solution of those lower scale turbulent equations in basic much more simple geometries is a huge area of research at the present time in the field of turbulence theory. Tens (if not hundreds) of people throughout the world are dying to find some meaningful clues. And this would be only a fraction of the problem in scaled VAT description.

3 The time averaging in turbulence modeling is connected to domain averaging. When the consequence of time→volume averaging changed for volume→time averaging, then to what domain and to what field should Reynolds averaging be applied? It is like mixing the heat and mass transport in one "physical" process-saying "we have the heat-mass transport averaging (T+C)." Mathematically it is possible, but what is the sense of that physics averaging? It means also that the enormous amount of knowledge and advancements obtained in turbulence theory could not be used because they would not be applicable to problems formulated with (volume→time) averaging in porous medium.

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Closure to "Discussion of 'Alternative Models of Turbulence in a Porous Medium, and Related Matters'" (2001, ASME J. Fluids Eng., 123, p. 931)

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I thank Dr. Travkin for his thoughtful discussion. I am happy to leave it to readers to decide which of his claims have merit, and I hope that these publications will inspire increased interest in the topic throughout the fluids engineering community.