Technical Forum

Questions in Fluid Mechanics

Stokes' Hypothesis for a Newtonian, Isotropic Fluid

Mohamed Gad-el-Hak[1]

Is the second coefficient of viscosity equal to the negative two-third of the dynamic coefficient of viscosity?

The short answer to the above question is no, not in general. Although the issues involved remain partially open, the present brief is an attempt to clarify some of the misconceptions and misuses embroiled in the subject of bulk viscosity. Consider the genesis of the query.

To close the equations of motion for a continuum fluid, a relation between surface forces and the flow field is needed. A Newtonian fluid is that for which the stress tensor is assumed to be linearly related to the rate-of-strain tensor.[2] The constant of proportionality between these two second-order tensors is in general a fourth-rank tensor:

$$\sigma_{ij} = \sigma_{ij}^{(0)} + C_{ijkl} e_{kl} \qquad (1)$$

where σ_{ij} is the stress tensor, $\sigma_{ij}^{(0)}$ is the stress distribution that can exist in a resting fluid, C_{ijkl} are the 81 coefficients of proportionality, and e_{kl} is the rate-of-strain tensor. The residual stress term must be retained for fluids, while the corresponding term is dropped when deformable, compressible solids are considered (Eringen, 1980). In the theory of linear elasticity, the displacements are measured, by convention, from a state in which the body is at rest under zero body forces, and the residual stress term has no contribution to the equations of motion. This is justified because any process which reduces the finite motion at a point to zero would not change perceptibly the thermodynamic state. For fluids, this is not necessarily the case. The *total* stress appears in the equations of motion of fluids, and it is therefore absolutely necessary to include $\sigma_{ij}^{(0)}$.

For fluids, both $\sigma_{ij}^{(0)}$ and C_{ijkl} vary with the thermodynamic state specified by, for example, the density and temperature. However, the residual stress must be the same regardless of the fluid state of motion, otherwise the assumed linear relation between σ_{ij} and e_{kl} is violated. Since the stress force exerted across any element of a surface in a resting fluid is independent of the orientation of that element (see, for example, Batchelor, 1967), it follows that:

$$\sigma_{ij}^{(0)} = -p\delta_{ij} \qquad (2)$$

where p is a scalar, called hydrostatic pressure or simply pressure, and δ_{ik} is the Kronecker delta (the only isotropic second-rank tensor). The pressure is one more unknown in fluid prob-

lems,[3] but the continuity equation, which has no counterpart[4] in the theory of elasticity, provides an additional equation to close the problem.

The lack of microscopic surface moments ensures that the stress tensor is a symmetric one. If the fluid is further assumed to have no preferred directions, i.e., isotropic fluid, those 81 linear coefficients reduce to only two independent coefficients (for a proof, see, for example, Long, 1961; Aris, 1962): μ the dynamic coefficient of viscosity (shear), and λ the second coefficient of viscosity (dilatational). Thus for a Newtonian, isotropic fluid:

$$\sigma_{ik} = -p\delta_{ik} + \mu\left(\frac{\partial U_i}{\partial x_k} + \frac{\partial U_k}{\partial x_i}\right) + \lambda\left(\frac{\partial U_j}{\partial x_j}\right)\delta_{ik} \qquad (3)$$

where U_i is a velocity component in the x_i-direction.

Continuum mechanics does not require any fixed relationship between the two coefficients of viscosity, and one must appeal to statistical mechanics, to macroscopic thermodynamics or, as a last resort, to experiments. The precise value of the second coefficient of viscosity is not needed for inviscid flows (both μ and λ are assumed zero), for incompressible flows ($\nabla \cdot \vec{U} = 0$), or when the boundary layer approximations are invoked (normal viscous stresses $\ll$ shear stresses). On those special cases, the thrust of the present question is muted although the conceptual issues are always important.

If we define the mean pressure $\bar{P}$ as the negative one-third of the sum of the three normal stresses (a tensor invariant), Eq. (3) yields:

$$\bar{P} \equiv -\frac{1}{3}(\sigma_{11} + \sigma_{22} + \sigma_{33}) = p - \left(\lambda + \frac{2}{3}\mu\right)(\nabla \cdot \vec{U}) \qquad (4)$$

The factor $(\lambda + 2/3\mu)$ is often termed the coefficient of bulk viscosity, although some textbooks mistakenly reserve this terminology for λ itself. Physically, this factor is connected with the dissipation mechanism during a change of volume at a finite rate. In other words, the bulk viscosity provides a damp-

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[1]Aerospace and Mechanical Engineering, University of Notre Dame, Notre Dame, IN 46556-5637.

[2]The present brief does not deal with nonlinear, i.e., non-Newtonian fluids.

[3]For compressible flows, if the density and internal energy, for example, are taken as the two independent intensive properties, then the intensive state of a simple thermodynamic system is fully specified. The pressure in that case is not an independent variable and follows from the appropriate equation of state. For a strictly incompressible fluid, on the other hand, the pressure does not influence the thermodynamic state and is formally an unknown mechanical force per unit area whose gradient, but not absolute value, could be determined from the equations of motion.

[4]Of course mass is conserved whether the material is solid or liquid. The simplifications of the linear elasticity theory merely eliminate the need to consider explicitly the continuity equation.

ing of volumetric vibrations such as might occur during sound absorption. Equation (4) implies that, unless either $(\lambda + 2/3\mu)$ or $(\nabla \cdot \vec{U})$ is equal to zero, the mean (mechanical) pressure in a deforming viscous fluid is not equal to the thermodynamic pressure; more on this point later. The second law of thermodynamics requires that both μ and $(\lambda + 2/3\mu)$ be non-negative.

In 1845, Stokes simply assumed that $(\lambda + 2/3\mu) = 0$.[5] The resulting negative value of λ implies that the tension required to produce a specified proportional rate of stretching along one principal axis is reduced if the fluid is locally expanding. This could easily be seen if Eq. (3) is rewritten for $i = k = 1$, for example:

$$\sigma_{11} = -p + 2\mu \frac{\partial U_1}{\partial x_1} + \lambda \left(\frac{\partial U_1}{\partial x_1} + \frac{\partial U_2}{\partial x_2} + \frac{\partial U_3}{\partial x_3} \right) \qquad (5)$$

If λ is negative and $(\nabla \cdot \vec{U})$ is positive, σ_{11} necessary to produce a given rate of stretching $\partial U_1/\partial x_1$ becomes smaller as the fluid dilation is intensified and/or as the absolute value of λ is increased.

Unfortunately, the above implication can neither be verified nor refuted with direct measurements. The bulk viscosity can be measured, albeit not very accurately, by the attenuation and dispersion of intense ultrasonic waves (in order to generate measurable effects). In order to satisfy the quasi-equilibrium approximation that requires the sound frequency to be small compared to the inverse of the molecular relaxation time, the high-frequency data are more often than not extrapolated to zero frequency resulting in considerable scatter. The second coefficient of viscosity may not even be a thermodynamic property, since available measurements indicate that λ is frequency dependent. Nevertheless, reasonably accurate, high-frequency acoustic absorption measurements, in conjunction with the standard low-frequency theory, do indicate that the Stokes' hypothesis is correct only for monatomic gases (Prangsma et al., 1973).

Very recently, Emanuel and Argrow (1994) have proposed an alternative, still indirect, method for measuring the bulk viscosity. Their yet-to-be-demonstrated approach is particularly suited for dense polyatomic gases, where the density-based thickness of a shock wave is typically thousands of mean free paths. Emanuel and Argrow have shown analytically that the ratio $(\lambda + 2/3\mu)/\mu$ is linear with the aforementioned thickness, which could readily be measured using the optical reflectivity method or the electron-beam absorption technique.[6]

Several controversies exist in the literature regarding the second coefficient of viscosity. First, does the kinetic theory of gases *prove* that the bulk viscosity is zero for a monatomic gas? Truesdell (1954) argues that this statement is an assumption of the theory and not a proof. On the other hand, the Chapman-Enskog expansion of the Boltzmann equation does yield a zero bulk viscosity for dilute gases without internal molecular structure (Chapman and Cowling, 1970). Furthermore, acoustic attenuation measurements for inert gases support this conclusion. Second, Karim and Rosenhead (1952) report on several sound-wave-attenuation measurements that yield a large, positive λ for most liquids. Once again, Truesdell (1954) disputes the validity of these experiments (see also the extensive discussion on the general subject of bulk viscosity provided under the leadership of Rosenhead, 1954).

Should there be any significant difference between the mechanical and thermodynamic pressures? This is an unsettling question, but the kinetic theory of gases offers some guidance (Hirschfelder et al., 1954; Vincenti and Kruger, 1965; Chap-

man and Cowling, 1970). The mechanical pressure is a measure of the translational energy of the molecules. The thermodynamic pressure, on the other hand, is a measure of the total energy, which might include additionally vibrational and rotational modes and, for liquids and dense gases, intermolecular attraction. For dilute monatomic gases, the translational energy is the only mode of molecular energy. The mechanical and thermodynamic pressures are, therefore, the same state variable and the bulk viscosity is zero as indicated earlier.

For polyatomic gases, the mechanical and thermodynamic pressures are not necessarily the same. The bulk viscosity is expected to be important if the relaxation (or adjustment) time of the molecules is not small compared to the characteristic time of the flow. Consider, for example, the passage of a polyatomic gas through a shock wave. Vibrational modes of molecular energy are excited at the expense of the translational modes, and the nonzero bulk viscosity is a measure of the corresponding transfer of energy. In this case, the gas within the shock is not in thermodynamic equilibrium, the bulk viscosity is proportional to the longer relaxation time for the relevant internal (nontranslational) modes to come to equilibrium, and the mechanical pressure is no longer equal to the thermodynamic pressure.

Classical kinetic theory is, of course, not applicable to liquids and dense gases. The bulk viscosity for those situations is determined primarily from experiment and is found, despite the considerable scatter in the data, to have a finite positive value. The dense gas theory (Chapman and Cowling, 1970) provides some support for this result.

Emanuel (1990) has recently pointed out that reliance on Stokes hypothesis may not always be warranted. He cites the example of hypersonic entry into certain planets where the atmosphere consists largely of carbon dioxide.[7] In such cases, lack of knowledge of λ might be especially detrimental to the accurate computations of relevant engineering quantities such as the skin friction or heat transfer rate.

From room temperature acoustic attenuation data, Tisza (1942) has concluded that the bulk viscosity $(\lambda + 2/3\mu)$ for CO_2 is three orders of magnitude larger than its first viscosity coefficient μ. Truesdell (1953) concurs with this result. Acoustic attenuation and other processes in gases with internal molecular structure can involve thermodynamic nonequilibrium as indicated earlier, and such effects may be modeled by a finite bulk viscosity as was done by Emanuel (1990) for a relaxing polyatomic gas.

By including a nonzero bulk viscosity as a correction term in a general, nonsimilar formulation of hypersonic laminar boundary layers, Emanuel (1992) has computed (for the class of planetary problems cited earlier) a heat transfer rate well in excess of that based on Stokes' hypothesis prediction. There is also an effect on the pressure which changed measurably across the wall layer, in contradiction to a key result of classical boundary layer theory. No significant effect on the skin friction was reported.

In summary, much confusion still remains in the literature 150 years after Stokes assumed that the bulk viscosity is zero. In general, it is not. The thermodynamic and mechanical pressures differ when the fluid undergoes nonequilibrium thermodynamic processes. Considerable scatter is present in existing data, and it is hoped that future experiments would provide more accurate estimates of the bulk viscosity for liquids and polyatomic gases.

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[5]An alternative statement of Stokes' hypothesis is that the average normal *viscous* stress is zero.

[6]In an unpublished report (not cited by Emanuel and Argrow, 1994), Frederick S. Sherman might have been the first to use shock thickness data to estimate the second coefficient of viscosity of nitrogen (NACA TN 3298, 1955).

[7]In a boundary-layer formulation, the bulk viscosity has a third-order effect. In order to demonstrate a measurable effect, Emanuel chose the extreme example of hypersonic flow of a gas with a very large bulk viscosity.

how little I knew about this rich subject. My colleague at Notre Dame, Professor Joseph M. Powers, has read the manuscript at several stages of its development. The comments made by all five contributed significantly to the present version of my unanswered question in fluid mechanics.

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Comments on the Policy Statement on Numerical Accuracy

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B. P. Leonard[1]

The *Journal of Fluids Engineering* Editorial Policy on numerical accuracy has sparked some lively debate, both in and out of print. This, in itself, is a positive development, as anticipated by the editors (Freitas, 1994). One of the more controversial aspects of the Policy is the effective "banning" of largely first-order-based methods. Since I was involved in the latter stages of the drafting of the Policy, and have been advocating a halt to the use of first-order methods for about twenty years, I would like to make a few comments in support of this aspect of the Policy. In an ideal world, I would agree with Professor Ferziger (1994) that we should not mandate the exclusion of any particular technique; referees should be able to filter out the "foolishness." But first-order methods are currently so wide-spread that "drastic" measures seem to be called for. (In an ideal world, one might expect automobile drivers to act wisely; in reality, rules of the road are needed to deter foolishness.)

Twenty-odd years ago, first-order methods could be of use in giving semi-quantitative results that enhanced understanding of fluid processes. My first encounter with CFD involved a simulation of a magnetic shock tube (Schneider et al., 1971). As an initial attempt at understanding this problem, we purposely made use of the inherent artificial viscosity of a first-order convection scheme to *replace* the rather complex transport processes within the shock and magnetic piston. The results were *good enough* for the experimentalists to be able to identify what they were measuring. [We jokingly referred to this as the "good-enough" method, not realizing the close relationship to an earlier (and subsequently very well-known) first-order "shock-capturing" method of a similar sounding name (Godunov, 1959).] Today, this paper would not be acceptable for publication in the *Journal of Fluids Engineering*. But a modern simulation would use a higher-order nonoscillatory method together with some simple (e.g., constant) effective viscosity (diffusivity) model. The results might be almost identical, but the philosophy is significantly different: we (rather than the grid-dependent differencing scheme) *are controlling the transport model.*

The problem with first-order-based methods is not just that they are somewhat less accurate than higher-order methods (actually, they are incredibly less accurate). More significant is the fact that *they are not simulating the correct physical problem.* The inherent artificial viscosity (diffusivity) of first-order upwinding (proportional to the local velocity and the mesh size) is well known and well documented. Why do people continue to use first-order-based methods? Apparently because of the philosophy that "any solution is better than none." As has been pointed out (Ferziger, 1993), this is a potentially "dangerous" attitude—one that needs to be countered with some common sense "rules of the road." First-order-based methods are typically used in high-convection flow regimes—involving *very high* grid Reynolds (Péclet) numbers—for which "classical" central-difference (or equivalent finite-element or finite-volume) schemes often blow up! The first-order simulation is equivalent to a central-difference simulation of a *nonphysical* problem in which the component grid Reynolds (Péclet) numbers are artificially manipulated and never allowed to exceed 2.0. This, of course, is (fairly) well known for schemes using first-order upwinding for convection together with central methods for diffusion. It also applies, however, to the popular Hybrid scheme (Spalding, 1972), the even more popular power-law scheme (Patankar, 1980), and the exponential-differencing scheme on which they are based (Allen and Southwell, 1955), when these methods are used out of the rather restrictive context for which they are valid (steady, quasi-one-dimensional, source-free flow, closely aligned with a grid coordinate). The most common misapplication of exponential-based schemes is to multidimensional flows oblique or skew to the grid. This introduces massive artificial "cross-wind" diffusion. Typi-

[1]Center for Computational Mechanics, The University of Akron, Akron, OH 44325-3903.