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 embodied 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. The constant of proportionality between these two second-order tensors is in general a fourth-rank tensor:

\[ \sigma_{ij} = \eta \partial e_{ij} / \partial t + \eta D_{ij} \]  

where \( \sigma_{ij} \) is the stress tensor, \( D_{ij} \) is the stress distribution that can exist in a resting fluid, \( \eta \) is the fluid's coefficient of proportionality, and \( e_{ij} \) is the rate-of-strain tensor. The residual stress tensor 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 tensor 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 model stress appears in final equations of motion of fluids, and it is therefore absolutely necessary to include \( \sigma^{(0)} \).

For solids, both \( \sigma^{(0)} \) 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 \( \sigma_{ij} \) and \( e_{ij} \) is violated. Since the stress force exerted across any element of a surface in a resting fluid is independent of the configuration of that element (see, for example, Batchelor, 1967), it follows that:

\[ \sigma^{(0)} = -p \delta_{ij} \]  

where \( p \) is the scalar, called hydrostatic pressure or simply pressure, and \( \delta_{ij} \) is the Kronecker delta (the only isotropic second-rank tensor). The pressure is one more unknown in fluid problems, but the continuity equation, which has no counterpart 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; Arts, 1962). \( \mu \) the dynamic coefficient of viscosity (shear), and \( \lambda \) the second constant of viscosity (dilatational). Thus for a Newtonian, isotropic fluid:

\[ \sigma_{ij} = \mu \partial e_{ij} / \partial t + \frac{\mu}{2} \partial \epsilon / \partial t + \frac{\lambda}{2} \partial 2 \]  

where \( \epsilon \) is a velocity component in the \( x \)-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 \( \mu \) and \( \lambda \) are assumed zero), for incompressible flows \( (\nabla \cdot \mathbf{u} = 0) \), or when the boundary layer approximations are invalid (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 \( P \) as the negative one-third of the sum of the three normal stresses (a tensor invariant), Eq. (3) yields:

\[ P = -\frac{1}{3} (\sigma_{xx} + \sigma_{yy} + \sigma_{zz}) = p - \lambda / \mu (\nabla \cdot \mathbf{u}) \]  

The factor \( -2/3 \) is often termed the coefficient of bulk viscosity, although some textbooks mistakenly reserve this terminology for \( \lambda \) 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 damping mechanism.

\footnote{Acoustics and Mechanical Engineering, University of Notre Dame, Notre Dame, Ind. 46556-057. The present brief does not deal with non-linear, i.e., one-Newtonian fluids.}

\footnote{For compressible fluids, if the density and sound energy, for example, are taken as two independent intensive properties, then the condition of a single thermodynamic system is fully specified. The pressure in that case is not an independent variable, but it is determined by the appropriate equation of state. For a strictly incompressible fluid, on the other hand, the pressure does not influence the thermodynamic state and it is formally an additional mechanical force per unit area whose gradient, but not absolute value, could be determined from the equations of motion.}

\footnote{Of course, water is considered to be the material of solid or liquid. The simplification of the linear elasticity theory merely eliminates 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 \( (\lambda + \mu) \) is equal to zero, the mean (mechanical) pressure in a deforming viscous fluid is not equal to the thermodynamic pressure, and vice versa from this point on. The second law of ther-
modynamics requires that both \( \mu \) and \( (\lambda + 2/3\mu) \) be non-
egative.

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

\[
\sigma_{\gamma_1} = \rho \frac{\partial u_1}{\partial t} + \frac{1}{2} \left( \frac{\partial u_1}{\partial x_1} + \frac{\partial u_1}{\partial x_1} \right) + \frac{\partial u_1}{\partial x_1}
\]

If \( \sigma_{\gamma_1} \) is negative and \( \rho \) is positive, \( \sigma_{\gamma_1} \) is neces-

Unfortunately, the above implication can neither be verified nor refuted with direct measurements. The bulk viscosity can be measured, although 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 \( \eta \) may not even be a thermodynamic property, since available measurements indicate that \( \lambda \) is fre-

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 partic-
ularly 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 thick-
ness, which could readily be measured using the optical reflec-
flectivity method or the electron-beam absorption technique.

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 monoatomic gas? Truesdell (1954) argues that this statement is an assump-
tion of absolute equilibrium. On the other hand, the Chapman-Enskog exponent of the Boltzmann equation does yield a zero bulk viscosity for dilute gases without internal molecular structure (Chapman and Cowling, 1970). Further-
more, acoustic attenuation measurements for inert gases sup-
port this conclusion. Second, Ramo and Bonehead (1952) re-
port on several sound-wave-attenuation measurements that yield a large, positive \( \lambda \) for most liquids. Once again, Truesdell (1954) disputes the validity of these experiments (see also the exten-
sive discussion on the subject of bulk viscosity pro-
vided under the heading of Rouse and Koros, 1954).

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

An alternative statement of Sines' hypothesis is the mean average normal stress is zero.

An unpublished report (see cited by Emanuel and Argrow, 1994), Fred-

Stick S. Sheenan might have been the first to use shock thickness data to es-
timate the second coefficient of viscosity of air (NACA TN 2026, 1935).

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Comments on the Policy Statement on Numerical Accuracy
B. P. Leonard*

The Journal of Fluids Engineering Editorial Policy on nu-
merical accuracy has sparked some lively debate, both in and out of print. This, in itself, is a positive development, as an-
ticipated by the editors (Freitas, 1994). One of the more con-
traversial 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 ad-
vocating a bit 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 Ferrizge (1994) that we should not mandate the ex-
clusion of any particular technique; referees should be able to filter out the “foolishness.” But first-order methods are cur-
rently so widespread that “foolishness” seems apt 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 pur-
posely made use of the inherent artificial viscosity of a first-
order convective scheme to replace the rather complex trans-
port processes within the shock and magnetic piston. The results were good enough for the experimentalists to be able to iden-
tify what they were measuring. (We jokingly referred to this as the “good-enough” method, not realizing the close rela-
tionship 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 not look identically, but the philosophy is significantly different: we (rather than the grid-dependent differencing scheme) are controlling the numerical model.

The problem with first-order-based methods is not that they are somehow less accurate than higher-order methods (actually, they are incredibly less accurate). More significant in 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 con-
tinue to use first-order-based methods? Apparently because of the philosophy that “any solution is better than none.” As has been pointed out (Ferzigger, 1993), this is a potentially dan-
gerous attitude—one that needs to be countered with some commonsense 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 “clas-
sical” 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) num-
bers are artificially manipulated and never allowed to exceed 2.0. This, of course, is clearly well known for schemes using first-order upwinding for connection together with central methods for diffusion. It also applies, however, to the popular Hybrid scheme (Spalding, 1972), the even more popular power-
less scheme (Patankar, 1980), and the exponential-differencing scheme on which they are based (Allen and Southwell, 1950). When these methods are used out of the rather restrictive con-
text 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 skewed to the grid. This introduces massive artificial “cross-wind” diffusion. Typi-

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