Poisson was *not* a geophysicist!

By LEON THOMSEN
Amoco Production Company
Tulsa, Oklahoma

The purpose of this Round Table essay is to argue that Poisson's ratio is not relevant to any problem in seismology, and that we should clear our minds of it, and of the confusion that it entrains. The reason for its irrelevance is that Poisson's ratio is defined in terms of an experiment (axial compression or tension of a thin bar) which is not relevant to wave propagation (although it may be a common situation in engineering mechanics). I then extend the argument (with less certitude) to argue that Poisson's ratio is not useful anywhere in exploration geophysics.

First, the seismological argument. Because most of our formal training in elasticity was heavily influenced by mechanics, we were all introduced to Poisson's ratio early, and immediately we were shown that it was a function of the nondimensional ratio μ/E (shear modulus/Young's modulus). Thus, in recent years, as exploration seismologists have broadened their paradigm beyond normally incident *P*-waves, recognizing that the shear properties of rocks were an aid to exploration, it seemed natural to dust off those old mechanics texts and resurrect Poisson's ratio to describe the relative strength of shear and compressive stiffness.

Of course, shear properties *are* important in many seismological contexts and, of course, the relative magnitude of shear and compressive stiffness is a useful notion. But, in seismology, Poisson's ratio is not a good way to express this relative magnitude. There is not a *single* seismological equation where Poisson's ratio enters in a natural way. That is, *any* seismological equation which appears to involve Poisson's ratio can be written more compactly, yielding greater physical insight, in terms of *other* measures of μ/E. Most often, the obvious measure is the velocity ratio, V_P/V_S. The underlying reason, of course, is that seismic waves do not perform Poisson's experiment.

Let us return to basics for a moment, in order to clarify concepts, and to see how Poisson's ratio can lead to misconceptions. Any introductory mechanics text will show you that, when a thin cylindrical bar of homogeneous, isotropic, linearly elastic material is axially squeezed (with free cylindrical surface), then Poisson's ratio, defined as the ratio of radial strain to (negative) longitudinal strain,

$$\nu \equiv -\varepsilon_r/\varepsilon_l,$$

may be expressed in terms of μ, E, the bulk modulus K, and the velocities by any of the following expressions:

$$\nu = \frac{E}{2\mu} - 1 = \frac{3K - 2\mu}{6K + 2\mu} = \frac{(V_p/V_s)^2 - 2}{2(V_p/V_s)^2 - 2}$$

Poisson was the first to discuss these ideas (in *Memoire sur l'Equilibre et le Mouvement des Corps Elastiques,* published in 1829).

It seems obvious from the original expression, above, that ν could not be less than zero, and everybody knows that it cannot be greater than 1/2. But where do these "limits" come from? It has been known since Poisson's epoch that the fundamental requirement for stability of an elastic material is that both μ and K should be nonnegative. This seems reasonable enough, since only a positive modulus yields a positive restoring force. A liquid has zero shear modulus μ and hence has Poisson's ratio 1/2 (easily derived from the above equation involving μ and K). The other limit of this expression, zero K, corresponds to $\nu = -1$, not to 0! So, materials with negative Poisson's ratios are theoretically possible and, in fact, are observed experimentally (see the article by R. Lakes in 1987). The very small Poisson's ratio of cork is responsible for its utility as a bottle-stopper for wine (note the application of axial stress in this engineering context). Certain dry rocks, with anomalously low values of V_p (relative to V_s) also produce negative values of ν as calculated from the expression above involving the velocity ratio (although this negative value may be due to anisotropy). So much for the intuitive appeal of Poisson's ratio!

By contrast, the velocity ratio V_p/V_s (or its inverse) plays no nasty tricks on us. Our intuition is valid: the velocity ratio is normally a number close to 2, less for rocks which are quartz-rich,

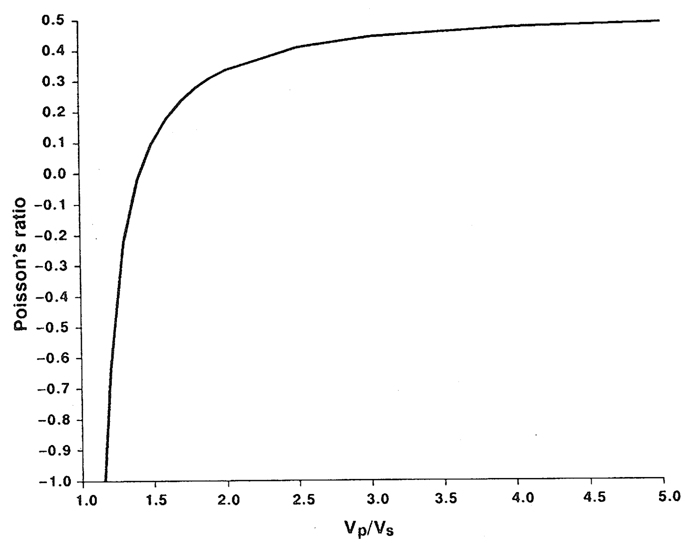

Figure 1. Poisson's ratio versus velocity ratio.

or undersaturated, more for rocks which are carbonate-rich or unconsolidated. Furthermore, because the equation relating Poisson's ratio and V_p/V_s is nonlinear, it is not easy to deduce V_p/V_s (or V_s itself) from ν, especially if ν is large (see Figure 1). Why should one prefer, over such a friendly quantity for understanding seismic phenomena, a nonlinear function of it, like Poisson's ratio?

One reason may be the historical precedent furnished by an unlucky pioneer. Koefoed (in *On the effect of Poisson's ratios of rock strata on the reflection coefficients of plane waves, Geophysical Prospecting,* 1955) first investigated the angle-dependence of *P*-wave reflection amplitude (a major component of the Amplitude-Variation-with-Offset effect) through a systematic application of the exact *P*-wave reflectivity formulas. He did so by exploring the six-dimensional space ($V_{p1}, \nu_1, \rho_1, V_{p2}, \nu_2, \rho_2$) of a planar contact between two elastic media with densities ρ_1, ρ_2, etc. He produced many curves and some general "rules" which did not lead at the time to significant exploration applications. I will use this episode to illustrate the point above, that when seismological equations are written without the use of Poisson's ratio, greater physical insight is possible.

The subject of AVO was reborn in the early '80s and has led to many applications. Insight into the effect was made possible by replacing the exact expressions of *P*-wave reflectivity with appropriate approximations, based on the assertion that for most geophysical media,

$$\frac{V_{p2}-V_{p1}}{(V_{p2}+V_{p1})/2} \equiv \frac{\Delta V_p}{\overline{V}_p} << 1$$

$$\frac{\Delta V_s}{\overline{V}_s} << 1$$

$$\frac{\Delta \rho}{\overline{\rho}} << 1$$

where the bars indicate arithmetric averages.

Under these approximations, P.G. Richards and C.W. Frasier showed (in 1976) that $R_p(\theta)$ could be written (neglecting terms involving the squares of the small quantities above) as

$$R_p(\theta) = \frac{1}{2\cos^2\theta}\frac{\Delta V_p}{\overline{V}_p} + \frac{1}{2}\frac{\Delta\rho}{\overline{\rho}} - \left(\frac{2\overline{V}_s}{\overline{V}_p}\right)^2 \left(\frac{\Delta V_s}{\overline{V}_s} + \frac{1}{2}\frac{\Delta\rho}{\overline{\rho}}\right) \sin^2\theta$$

The initial angular variation, critical to AVO analysis, is buried in the first and third terms of this expression.

In 1985, R. T. Shuey (wishing to shed light on Koefoed's "rules") rewrote the Richards-Frasier expression exactly as

$$R_p(\theta) = R_0\,[1 + A\sin^2\theta + B\tan^2\theta\sin^2\theta]$$

where the normal-incident reflection coefficient is

$$R_0 = \frac{\Delta z_p}{2\overline{z}_p} = \frac{1}{2}\left(\frac{\Delta V_p}{\overline{V}_p} + \frac{\Delta\rho}{\overline{\rho}}\right)$$

with $z_p = \rho V_p$ = *P*-wave impedance, and

$$A \equiv A_o + \frac{1}{R_0}\frac{\Delta\nu}{(1-\nu)^2}$$

$$A_o \equiv B - 2(1+B)\frac{1-2\nu}{1-\nu}$$

$$B \equiv \frac{1}{2R_o}\frac{\Delta V_p}{\overline{V}_p}$$

Combining these, we have

$$R_p(\theta) = R_o + \left(-2R_o - \frac{1-3\nu}{1-\nu}\frac{\Delta V_p}{2\overline{V}_p} + \frac{\Delta\nu}{(1-\nu)^2}\right)\sin^2\theta + \frac{1}{2}\frac{\Delta V_p}{\overline{V}_p}\sin^2\theta\tan^2\theta$$

It is clear that the introduction of Poisson's ratio in this expression has complicated the form of the equation to the point where physical intuition is threatened. The second (initial slope) term is a combination of three terms, whose relative size (and sign) is not obvious without the detailed explanation provided by Shuey.

If, instead of insisting upon the introduction of ν, we look for the simplest, most intuitively revealing version of this expression, we are led to one implied by J. Wright in 1986:

$$R_p(\theta) = R_o + \frac{1}{2}\left(\frac{\Delta V_p}{\overline{V}_p} - \left(\frac{2\overline{V}_s}{\overline{V}_p}\right)^2 \frac{\Delta\mu}{\overline{\mu}}\right)\sin^2\theta + \frac{1}{2}\frac{\Delta V_p}{\overline{V}_p}\tan^2\theta\sin^2\theta$$

The second term now clearly reveals the essential physics behind the use of AVO to identify hydrocarbon reservoirs. For ordinary lithologic reflections, the $\Delta\mu/\overline{\mu}$ part is usually bigger than the $\Delta V_p/\overline{V}_p$ part (for either polarity), hence (with the minus sign) the slope of $R_p(\theta)$ has algebraic sign *opposite* to the first (normal-incidence) term and $|R_p(\theta)|$ *de*creases initially. However, in the special case of a pure contact-event, the $\Delta\mu$ part is identically zero (see M.A. Biot's classic 1941 article), so that the first and second terms have the *same* sign and $|R_p(\theta)|$ *in*creases initially. Cases with both a lithologic contrast and a fluids contrast, or cases with exotic lithologies (e.g., anhydrite), may show either behavior.

Notice that, in this argument, the shear-compression comparison appears transparently in the trade-off between $\Delta V_p/\overline{V}_p$ versus $\Delta\mu/\overline{\mu}$, whereas it is murky in the previous expression. Also notice the natural appearance of the velocity ratio in this expression, where it does not complicate the argument above, but does shed light on other special cases. Approximations (e.g., those assuming that $V_p/V_s = 2$ or that θ is small or that density is related to velocity) can sometimes lead to costly errors; because of the simplicity of the last expression, such approximations are not needed.

My point has been to show, by example, how forcing ν into seismological equations invariably complicates them, and conversely how their simplest versions usually contain the velocity ratio. This is a happy circumstance, since few of us have an intuitive feel for the meaning of ν, while most of us intuitively understand V_P/V_S. Let us leave Poisson's ratio to the mechanists, for whom it was designed, and keep it out of seismology. At the very least, this policy would save many of us the repeated embarrassment of mispronouncing the name of one of the great figures of the French Enlightenment!

But exploration geophysics is not all seismology. Another common context in our business where Poisson's ratio often appears is in the estimation of horizontal stress. A homogeneous isotropic linearly elastic medium, axially strained, has the ratio of horizontal to vertical effective stress

$$\frac{\sigma_H}{\sigma_V} = \frac{\nu}{1-\nu} = 1 - 2\left(\frac{V_s}{V_p}\right)^2$$

This expression gives only a rough approximation to actual in-situ stresses, possibly because rocks are not, in fact, linearly elastic when subjected to such large nonhydrostatic stresses. The use of the second form above, rather than the first, is helpful in this regard, because the explicit appearance of the dynamical quantities (velocities)—in a context of static stress—should shock us into the realization that we are stretching (so to speak) the limits of the assumed elastic rheology. In this static context, the rejection of Poisson is based upon the inadequacy of the assumption of linear elasticity (when applied in real-earth contexts on real rocks) rather than upon the wave equation.

Furthermore, the appearance of the velocities may also remind us that, since real rocks have anisotropic seismic velocities, this expression needs to be generalized for anisotropy. (In 1986, I showed that in fact the neglected anisotropy correction may plausibly be comparable in magnitude to the isotropic term given above).

I conclude that Poisson's ratio is rarely a useful concept in any area of exploration geophysics, and invite readers to offer a valid counter-example.

Suggestions for further reading. *General theory of three-dimensional consolidation* by M.A. Biot (*Journal of Applied Physics*, 1941). *Scattering of elastic waves from depth-dependent inhomogeneities* by P.G. Richards and C.W. Frasier (GEOPHYSICS, 1976). *A simplification of the Zoeppritz equations* by R.T. Shuey (GEOPHYSICS, 1985). *Reflections coefficients at pore-fluid contacts as a function of offset* by J. Wright (GEOPHYSICS, 1986). *Weak elastic anisotropy* by L. Thomsen (GEOPHYSICS, 1986). *Foam structures with negative Poisson's ratio* by R. Lakes (*Science*, 1987).

Leon Thomsen received a bachelor's degree (1964) from the California Institute of Technology and a doctorate (1969) from Columbia University. He taught at the State University of New York at Binghamton for eight years, and studied the physics of the rocks and minerals of the earth's deep interior. He joined Amoco in 1980. His principal research interest is rock physics and wave propagation in anisotropic and anelastic media.

Poisson's ratio revisited

C. Payson Todd
Houston, Texas

I would like to comment on *Poisson was* not *a geophysicist* by Leon Thomsen (*TLE*, December 1990). Thomsen may have a point that the ratio V_P/V_S is a slightly more intuitive constant than Poisson's ratio. However, the relationship between V_P/V_S and Poisson's ratio is straightforward and, in practice, I've found it possible to move back and forward between the two with relative ease. I have also found that the most intuitively obvious expression for reflectivity versus angle is one which uses Poisson's ratio. This equation is Fred Hilterman's approximation of the equation first published by R.T. Shuey:

$$R(\theta) = R_O \cos^2(\theta) + 2.25\,\Delta\nu \sin^2(\theta)$$

where R_O is the normal incidence reflectivity and $\Delta\nu$ is the difference in Poisson's ratio across the interface.

Although inaccurate at *larger* angles, this approximation has the advantage of expressing $R(\theta)$ in two physically intuitive terms. The first term depends solely on R_O and decreases in importance with increasing angle. The second depends only on $\Delta\nu$ and increases in importance with increasing angle. I have had more success explaining AVO to the uninitiated with this expression than with any other.

Interested readers should consult Hilterman's article *Is AVO the seismic signature of lithology?* (*TLE*, June 1990) and *A simplification of the Zoeppritz equations* by Shuey (GEOPHYSICS, 1985).

Finally, a comment on the combative tone of the article. The purpose of scientific papers in general, and *TLE* articles specifically, is to educate and enlighten, not condemn and belittle. Unfortunately, *Poisson was* not *a geophysicist* tends more towards the latter than the former. When you write for *TLE*, you write for an educated and well-intentioned audience. Perhaps, in the future, authors who are overcome by zeal in their opinions could be reminded of this by the editors.

Leon Thomsen
Tulsa, Oklahoma

I am happy to see that the "combative tone" of my Round Table essay has had the desired effect of stimulating discussion among SEG members. I take the position that such combat, in such a forum, does indeed serve to "educate and enlighten" so long as the combat is between ideas, rather than personalities. I thank Todd for his response, and hope the following remarks will interest others as well.

So, to proceed with the combat of ideas: This particular battle concerns not the general proposition but the specific example of AVO. First, I take this opportunity to correct my representation of Shuey's work; the leading (small-angle) terms should read:

$$R_P(\theta) = R_O + \left[-2\left(1 - \frac{\nu}{1-\nu}\right) R_O - \frac{1-3\nu}{1-\nu}\frac{\Delta V_P}{2\,\overline{V}_P} + \frac{\Delta\nu}{(1-\nu)^2} \right] \sin^2\theta$$

My previous discussion is unaffected by this correction. Rearranging without further approximation, we have

$$R_P(\theta) = R_O \cos^2\theta + 2.25\,\Delta\nu \sin^2\theta$$
$$-\frac{(1-3\nu)}{(1-\nu)} \left[\frac{5-3\nu}{4(1-\nu)}\Delta\nu + R_O + \frac{\Delta V_P}{2\,\overline{V}_P} \right] \sin^2\theta$$

Now it is clear that if $\nu = 1/3$ ($V_P/V_S = 2$), then the final term vanishes, and we have Hilterman's approximation to Shuey's result.

But if, instead, $\nu = 1/4$ ($V_P/V_S = 1.732$), plausible for quartz-rich compacted sediments, then the correction terms is

$$-\frac{1}{3} \left[\frac{17}{12}\Delta\nu + R_o + \frac{\Delta V_P}{2\,\overline{V}_P} \right] \sin^2\theta$$

Or if, instead, $\theta = .437$ ($V_P/V_S = 3$), plausible for young, undercompacted and/or overpressured sediments (such as those in Hilterman's Figures 19 and 20), then the correction term is

$$+\,.555 \left[2.03\,\Delta\nu + R_o + \frac{\Delta V_P}{2\,\overline{V}_P} \right] \sin^2\theta$$

In all such cases, the correction to Hilterman's approximation may be quite comparable to the retained terms, and neglect of it may lead to puzzling results, of the type reported by Hilterman.

In assessing the size of the correction, it is necessary to evaluate the relative size of $\Delta\nu$, R_O, and $\Delta V_P/V_P$. This evaluation, of course, depends upon the lithologic contrast across the boundary, the porosity contrast, and the pore-fluid contrast. I have been able to find a clean argument for the interpretation of the AVO slope, in terms of these lithologic features, only along the lines (involving the shear modulus contrast $\Delta\mu/\mu$ instead of $\Delta\nu$) indicated by my Round Table essay. I have been able to explain this to the "uninitiated" by pointing out that this is the same argument, familiar to them, that distinguishes a gas bright spot from a lithologic bright spot: "gas-charging a sediment does not affect its shear modulus". By contrast, Poisson's ratio increases both with gas-charging and with decreasing quartz content, so the interpretation of $\Delta\nu$ is not clean.

In summary, it is clear that the apparent simplicity of Hilterman's approximation (involving ν) to the linearized reflectivity is due to a further approximation ($\nu = 1/3$). Wherever that approximation fails, reliance upon it can lead to costly error, so the approximation itself should be avoided. It had originally been proposed in order to simplify an equation which had previously been needlessly complicated by the introduction of Poisson's ratio. Other formulations, without ν, are sufficiently simple so that no further approximations are required. LE

ROUND TABLE

Poisson was not a rock physicist, either!

LEON THOMSEN
Amoco Exploration and Production Company
Houston, Texas

In "Poisson was not a geophysicist" *(TLE,* December 1990), I argued that Poisson's ratio is not a useful quantity for analysis in geophysical problems. The primary focus of that article (and in the subsequent discussion in *TLE,* April 199 I) was on seismic wave propagation, with AVO analysis as the prime example. I showed that consideration of Poisson's ratio invariably complicates the analysis, which is better conducted in terms of other relative measures of rigidity-versus-incompressibility (such as V_p/V_s) which arise more naturally in the equations of wave propagation.

Towards the end of that discussion, I casually broadened the scope of topics where Poisson's ratio could lead to error or confusion, such as the calculation of horizontal stresses. In the interest of brevity, I de-emphasized that part of the argument; perhaps this was unwise.

In fact, recently in these pages, Gretener (October 1994) and Domenico (September 1995) have initiated a discussion on this very topic. Gretener argued that Poisson's ratio is independent of fluid content in the pore space, and then used the angle of repose of loose sand (with or without an aquarium) to prove his point. He went on to describe the implications of this result for the computation of horizontal stress. Domenico responded that compressibility was indeed a function of fluid content, and reiterated previous ultrasonic data to prove *his* point. If the distinguished Professor Poisson had indeed been a rock physicist, he would probably have wished the discussion to include the following considerations.

A good starting place is Gretener's *definifion* of Poisson's ratio. If longitudinal stress is applied to the ends of a long stiff cylinder with free sides, then the (negative) ratio of the radial and longitudinal strains is *defined to* be Poisson's ratio:

$$\nu \equiv -\left(\frac{\varepsilon_h}{\varepsilon_z}\right) \qquad \text{...Poisson's experiment} \quad (1)$$

So far, so good. If, in addition, the material is linearly elastic and isotropic, then it obeys Hooke's law, which says that strain depends linearly upon stress in the isotropic way. Then it is elementary to show that the ratio above, expressed in terms of the rigidity G and incompressibility K is

$$-\left(\frac{\varepsilon_h}{\varepsilon_z}\right) = \frac{(3K-2G)}{(6K+2G)} = \nu \qquad \text{...elastic isotropic} \quad (2)$$

thereby expressing Poisson's ratio in these more general quantities.

Separately, the use of Hooke's law in the wave equation implies that seismic waves travel with velocities given by

$$V_p = \sqrt{(K+4G/3)/\rho} \qquad (3)$$

$$V_s = \sqrt{G/\rho} \qquad (4)$$

where p is density. Then, we find from equations 2 to 4 (i.e., subject to these assumptions) that

$$\nu = \left[\left(V_p/V_s\right)^2 - 2\right] \Big/ \left[\left(2V_p/V_s\right)^2 - 2\right] \qquad \text{....isotropic elastic, wave equation} \quad (5)$$

which appears to express Poisson's ratio in terms of seismic velocities.

Finally, a separate application of Hooke's law to the case of uniaxial *strain* (described by Gretener) yields the result he highlighted and questioned in his final summary:

$$\frac{\sigma_h}{\sigma_v} = \frac{\nu}{1-\nu} = \left(1 - 2\left(V_s/V_p\right)^2\right) \qquad \text{...uniaxial strain} \quad (6)$$

The question at issue may now be restated: How can equation 6 be true if the left side is independent of the fluid content in the pores (Gratener) while the right side does depend on fluid content (Domenico)?

It can happen that we deceive ourselves in our use of these simple equations: the symbols hide the assumptions. The problem is that Poisson himself only applied them to materials (like iron) which closely approximate his assumptions (homogeneous isotropic, linearly elastic). By contrast, we often apply them to *rocks* (which *are anisotropic, inhomogeneous,* and *quasi-poro-elastic* instead) to which Hooke's law is not directly applicable. Unless we are careful, this can get us into trouble. Let us consider these complications, one at a time.

In the interest of brevity (probably another mistake!), I will defer discussion of anisotropy, since this is not what divides Gretener and Domenico (but see my 1986 article in GEOPHYSICS for a relevant discussion).

Rocks are imperfectly *(quasi-)* elastic on two counts:

I) For large stresses (such as the static compression experiment described by Domenico), the strains are not reversible (i.e., release of stress does not release all the strain), and they are not linear; hence they are not properly described by Hooke. In the interest of brevity (!), I de-emphasize this point, until the end of this note.

2) Even for small stresses (such as the dynamic experiments described by Domenico), the elastic "constants" actually depend on the *time-scale* of the application of stress, e.g., on the frequency. (This was mentioned by Gretener with respect to salt, in a context of large stresses.) This frequency-dependence was discussed at length by Domenico, but I believe that it is *not* the crux of the dispute. However, I return to this issue below.

The rocks of the earth are *inhomogeneous* on all scales, but the inhomogeneity which drives this discussion is on the scale of the grains and pores. This brings us to the crux of the issue: *poro-elasticity.* Homogeneous elastic materials are characterized by two field quantities: i.e., the stress and the strain. Correspondingly, two-component materials (e.g., poro-elastic rocks, containing both grains and fluid) are characterized by four field quantities, i.e. the stress and strain in *both* components (or equivalently, the average (total) stress, the average strain, and the stress (the pressure) and strain in the fluid.) As we will see shortly, the issue which divides Gretener and Domenico involves the pressure in the fluid, a concept which is outside Hooke's law and never considered by Poisson.

However, these elements of poro-elasticity were illuminated by another great francophone, Maurice Biot, in a classic soil mechanics paper in 1941. This was subsequently applied to geophysical problems by Gassmann in 195 1 and eventually by Domenico in 1976. Among other points, they showed that the stress-strain relations for a poro-elastic medium also lead, in the wave equation, to the seismic velocities in equations 3 and 4, so long as the symbols K and G are properly defined. If the rock is compressed by a finite subsurface stress, then leaving aside certain complications, these equations *still* apply to a seismic wave with small *incremental* stress, strain, and pore pressure. For a clear discussion of Biot's central idea, with a few equations, see the review contained in my 1985 article in GEOPHYSICS.

However, we don't need the equations to see the essentials here. As Biot pointed out, if the pore fluid is free to drain out of the rock during compression, then the fluid cannot help support the load and, in fact, the incremental fluid pressure dPp vanishes. Hence, in such a drained experiment, it doesn't matter whether the fluid is liquid or gas, and the *effective* elastic "constants" $K_{drained}$, $G_{drained}$, $\nu_{drained}$, etc. are independent of fluid content. This explains Gretener's thought-experiment, i.e., why the angle of repose of loose sand in air, or in water, is the same. In both cases, one uses the *drained* elastic constants (which depend on the granular framework *only)* to calculate the effective Poisson's ratio, hence the angle of repose.

By contrast, if the fluid is *confined* during rock compression, and cannot escape, then in such an *undrained* experiment, the fluid does help support the load (to a degree which depends, among other things, upon its fluid incompressibility). During the passage of a seismic wave in the earth (or an ultrasonic wave in the laboratory), the fluid pressure does not have *time* to flow away (in response to the pressure transient $dPp(t)$); hence such an acoustic experiment measures the *undrained* elastic "constants." $K_{undrained}$ is larger, and $G_{undrained}$ is the same as their drained counterparts (at seismic frequencies); this means (obviously) that $Vp/Vs_{undrained}$ is larger and (after *some* thought) that $\nu_{undrained}$ is also larger.

Finally, it is obvious that if the fluid has negligible incompressibility, then the drainage conditions do not matter; in neither case does such a fluid support the load. Hence, a dynamic, undrained experiment on a gas-bearing rock should yield the same result for $K,G,$ and ν as a static, drained experiment. This explains the first-order equivalence between curves a and b in Domenico's Figure 3. (The *differences* between these curves are then explicable in terms of the concepts de-emphasized here for brevity).

(When the conditions of fluid pressure drainage are specified, this reduces the number of field variables from four to three - stress, strain, and fluid pressure. There are then *three* corresponding wave types: the two familiar ones discussed here, plus the "Biot slow-wave" which need not concern us here.)

Although it may not be clear from Domenico's discussion, the static compressibility test (his curve b) was done on a brine-saturated rock which was free to drain or "express" the brine (personal communication from A. Frisillo, ex-Amoco). Also, it should be stated explicitly that his computation of Poisson's ratio from these incompressibilities utilized Biot's result (implicit in Domenico's equation 8) that, at low frequencies, the rigidity G is independent of fluid content.

In the interest of future brevity, it should be clarified that Domenico's references to frequency-dependent "coupling" of fluid-to-frame encompass three different time-dependent effects on wave propagation :

1) *Macroscopic* flow of the fluid (draining, for distances comparable to an acoustic wavelength) leads (via Biot's equations) to a negligible degree of dispersion in most contexts:

2) Out-of-phase microscopic flow of the fluid (for distances comparable to a grain size) leads to the Biot slow wave, dismissed earlier.

3) In-phase microscopic flow ("squirt") of the fluid (for distances comparable to grain size) leads to significant attenuation in the ultrasonic band. The modest velocity dispersion accompanying this "fluid squirt" attenuation is most directly seen when a very wide band of frequencies (e.g., seismic-to-sonic) is available for comparison of velocities. This frequency dependence falls into the same class of imperfectly elastic effects mentioned above; it is *not* what drives the present dispute.

It is generally agreed that the low-frequency form of Biot's equations (with perfect "fluid-frame coupling", i.e., omitting *all* these effects) is sufficient to understand seismic-band wave propagation, particularly as it involves fluids. By focusing on the incremental fluid pressure within a rock at seismic frequencies, we see that brine-saturated seismic velocities are governed by the undrained equations of Biot, whereas gas-bearing seismic velocities are (approximately) governed by the drained equations. (A gas at high pressure in the subsurface will in general have a small but non-negligible incompressibility, whose effect can be taken into account using the equations of Biot, Gassmann, and Domenico. Likewise, these equations show that a small degree of gas saturation has almost the same effect as full gas saturation.) These seismic velocities, so understood, may then be used to construct a "seismic Poisson's ratio" using equation 5, although (following my 1990/91 argument) this *invariably* leads to more complicated seismic equations (for example in AVO studies), and sometimes to mistaken conclusions.

Then where does that leave the *rock* physicists, who wish to have a measure of rigidity-versus-incompressibility to use outside of this narrow seismological context, for example to calculate horizontal stresses using equation 6? Let us remind ourselves again that this equation assumes the material to be elastic. During burial over geologic time, a rock is subject to diagenetic consolidation, which is decidedly irreversible and nonelastic. So, although the burial deformation may be uniaxial, it does *not* follow Hooke's law of elasticity.

Nevertheless, we can imagine various thought experiments involving quasi-static unloading and loading of the *consolidated* rock which might lead to direct determination of the horizontal stress, and through equation 6 to an effective poro-elastic Poisson's ratio. Leaving aside, for brevity (!), the considerations mentioned above (anisotropy, finite stress, time-dependence), let us concentrate on the fluid effects during such quasi-static experiments, as a guide to how to *avoid* such experiments via a *seismic* experiment which would determine an effective poro-elastic Poisson's ratio for use in equation 6 to estimate the horizontal stress indirectly.

Perhaps in the interest of brevity, Gretener did not mention explicitly that when applied to rocks in the subsurface, the stresses in equation 6 are *effective* stresses:

$$\sigma_h = \left(\sigma_{hTotal} - \alpha P_p\right) \tag{7}$$

$$\sigma_v = \left(\sigma_{vTotal} - \alpha P_p\right) \tag{8}$$

where the factor a , introduced by Biot, was defined by Geertsma in 1957, as noted by Domenico. σ_{vTotal} is the overburden stress, obtained, for example, by integrating the density. Then equation 6 may be written out as:

$$\frac{\left(\sigma_{hTotal} - \alpha P_p\right)}{\left(\sigma_{vTotal} - \alpha P_p\right)} = \frac{\nu}{1-\nu} \tag{9}$$

The effective stresses, calculated in this way, are the stresses felt by the granular framework (i.e., the influence of the fluid pressure is removed). In other words, these *effective* stresses are independent of in-situ *Pp,* hence are representative of *drained* conditions. Therefore, in our quasi-static thought experiments, we should allow the in-situ pore pressure *Pp* to drain away, and use stresses which are correspondingly smaller *(cf* equations 7 and 8) than the total subsurface stresses.

This analysis has obvious consequences for seismic estimation of an effective Poisson's ratio for use in equation 9. In particular, we should use a *drained* poro-elastic Poisson's ratio on the right side, just as *we* use *drained ("efective")* stresses on the left side. Since the seismic velocities are *undrained* quantities, these must be corrected to *drained* quantities before being used in equation 9. This correction, using the equations of Biot, Gassmann, and Domenico, is a minor correction if the rocks are gas-bearing, but a significant one if the rocks are brine-saturated.

In the particular case (quartzite) mentioned by Gretener, one would correct the seismically-derived brine-saturated *undrained* Poisson's ratio ($\nu_{undrained} = 0.4$) to the *drained* Poisson's ratio ($\nu_{drained} = 0.1$). Note that this has a drastic effect on the calculated ratio of effective stresses (the undrained ratio is 0.67 and the drained calculation is 0.11). (However, note that for unconsolidated rocks, more analogous to Gretener's sandpack than is quartzite, Poisson's ratio and velocity ratio, both undrained and drained, are considerably higher. Gretener's calculated Poisson's ratio of .28 is fully consistent with seismic values, corrected to drained conditions, on unconsolidated rocks).

I am not in a position at this time to comment on whether or not this drainage correction commonly leads to an improvement in predicted total stresses. If so, this careful analysis will have been worthwhile.

If not, this analysis will *still* have been worthwhile, by the following argument. If we model the stresses using the correct conditions of fluid-flow, but overly simplistic rheology (isotropic, linearly elastic) as above, and get the wrong answer, then this erroneous conclusion focuses our attention (quite properly) on that simplistic rheology. It motivates us to better understand the physics underlying the observation; in this way we will eventually make progress. Even if using the wrong fluid-flow conditions in the calculation should yield a better estimate for stresses in some particular area, this is "getting the right answer for the wrong reason," and such a success cannot be easily generalized to other places or contexts.

A 1994 article by Yale and Jamieson in *Rock Mechanics* presents data and discussion on this point. They note that for their samples, and with their experimental procedures, the quasi-static measured *drained* values for Poisson's ratios coincidentally correlated less well with the ultrasonic measured *drained* values (calculated assuming isotropic linear elasticity) than with the *undrained* values calculated from these using the equations of Biot. That is, the dynamic → static decrease in $\nu_{drained}$ due to the large stresses and the long times in the static test, could be nearly cancelled by an *increase,* calculated by pretending that the conditions were *undrained.* Does this imply that seismically (or sonically) derived Poisson's ratios should *not* be corrected down to drained conditions, as recommended here, for use in estimating stress?

Probably not. In analyzing their results, we should first note that their static tests measured Poisson's ratio itself (the strains in equation 1), under conditions of triaxial controlled stress (unfortunately, they did not report measurements of horizontal stress under uniaxial *strain* conditions). Since, as they note, these various physical effects are independent of each other, in *other* particular cases the different effects may cancel to different degrees, so that the observed "coincidence" (their term) is not easy to generalize.

Further, let us consider the nonlinear effects of large stresses. In rocks, stress-strain curves well below the failure point are concave, with greater strain requiring disproportionately greater stress. In other words, the slope (hence the elastic modulus) increases with increasing stress (the effect on Poisson's ratio is less clear, because of experimental difficulties). Yale and Jamieson averaged Poisson's ratio over cycles with σ_v/σ_h extending above 4, whereas we expect to find this effective stress ratio should be much closer to unity (i.e., < 2) in the earth's subsurface. To be most applicable to the determination of subsurface stress, Poisson's ratio should be measured in the range of stresses expected there.

More fundamentally, this nonlinear behavior raises the issue of how to generalize equation 9 (which assumes linearity) to such materials. Can we use some value ($\nu/(1-\nu)$) which is averaged over some cycle of σ_v/σ_h? Or should we use only the loading part of the cycle? The unloading part? Or should we use an aymptotic value near the ambient condition of σ_v/σ_h (which, of course, is not known, a priori)? All of these interesting questions are distorted, or even excluded from consideration, if we impose incorrect drainage conditions.

So, I summarize the discussion as follows. Seismic velocities (undrained conditions) are indeed dependent upon fluid content and, in fact, this is why AVO is interesting to the petroleum industry. (However, this fluid dependence of seismic behavior really *is* best understood in terms of V_p/V_s, rather than v!)

The *independence* of Poisson's ratio from fluid content (cited by Gretener in his angle-of-repose argument) applies to *drained* conditions, whereas the *dependence* (shown by Domenico in acoustic experiments) applies to *undrained* conditions. The equations of poro-elasticity (and the intuitive argument given above) imply that the effective stress ratio depends upon the *drained* elastic "constants." Hence, when properly corrected to *drained* conditions (using the equations of Biot, Geertsma, and Domenico), seismic velocities can indeed be used to estimate effective stresses in a useful way using equation 9.

However, such estimates of effective stresses from elastic measurements involve strong assumptions (e.g., perfect linear isotropic poro-elasticity) which are not necessarily realized in nature. Extending the final summary of Gretener's discussion, I write:

$$\left(\frac{\sigma_h}{\sigma_v}\right)_{effective} \; ?= \left(\frac{\nu}{1-\nu}\right)_{drained} < \left(\frac{\nu}{1-\nu}\right)_{undrained} = ?\left(1-2\left(V_s/V_p\right)^2\right) \qquad (10)$$

The question marks in the above expressions remind us that the equalities expressed are subject to the critical assumptions of linear isotropic elasticity. *Many* properties of real rocks are not included in this simple model; all of these should be estimated thoughtfully before conclusions are accepted. The effects of *poro-elasticity* in isotropic rocks at seismic frequencies are adequately represented by the central *inequality,* without the question marks.

Finally, the casual use of "Poisson's ratio" can hide this complexity of assumptions behind apparently simple notation, and can easily lead one astray. Rock physicists need to be precise when extending such classical concepts to more complex situations, particularly when engaging in multidisciplinary dialog such as this. 6

Acknowledgements: I thank Peter Gretener for initiating this discussion and Norman Domenico for bringing the article by Yale and Jamieson to my attention, and both for encouraging my contribution. I thank Mike Mueller and Gerry Beaudoin (Amoco for useful discussions, and Amoco Exploration and Production for permission to publish these comments.

AVO and Poisson's ratio

It remains disturbing that AVO analysts still cling to the term Poisson's ratio (see e.g., Dutta, *TLE* February 2002).

In 1990 (*TLE*) Leon Thomsen pointed out that it is both unnecessary and confusing to approach the V_P/V_S ratio (my PS ratio) by way of Poisson's ratio. Gretener (*TLE*, 1994) showed that it is also erroneous resulting in values that from a mechanical perspective are unacceptable, if not ludicrous. Domenico's (*TLE*, 1995) and Thomsen's (*TLE*, 1996) replies remain unconvincing and have never been challenged.

The dispute centers on the so-called dynamic determination of Poisson's ratio (v_q)

$$\nu_d = \frac{\left[(V_p/V_s)^2 - 2\right]}{\left[2(V_p/V_s)^2 - 2\right]}$$

or (1)

$$V_p/V_s = \left\{(1-\nu_d)/(1/2-\nu_d)\right\}^{1/2}$$

The original static definition of Poisson's ratio as given by Poisson (v_s) is:

$$\nu_s = \varepsilon_h / \varepsilon_z \qquad (2)$$

where: ε_h lateral strain, ε_z axial strain in a specimen subjected to uniaxial stress. For linearly elastic materials, such as metals, we have:

$$\nu_s = \nu_d \qquad (3)$$

Unfortunately rocks are elastic but *not* linearly elastic and thus we have:

$$\nu_s \neq \nu_d \qquad (4)$$

Domenico's value of 0.1 for a dry sandpack (GEOPHYSICS, 1976, 1977 and *TLE*, 1995) and Gregory's values (GEOPHYSICS, 1976) reaching into the negative realm (*sic*) make no sense. The reason: A fundamental assumption of equation (1), linear elasticity, is not fulfilled. In Poisson's time, attention was focused on metals which *are* linearly elastic.

Poisson's ratio has been in the AVO literature ever since the Muskat and Meres paper (GEOPHYSICS, 1940). To abandon the term is *not* devastating to AVO analysis as stated by Domenico (*TLE*, 1995). AVO analysts merely use it to define the PS ratio. The name is irrelevant because it only represents a mathematical substitution. To retain this name severely impairs the credibility of AVO analysts as physicists and confuses the dialogue with rock mechanicists.

Bottom line: Use what you really measure (and assume) and what really matters for AVO—the PS ratio.

—PETER GRETENER
University of Calgary
Calgary, Alberta, Canada

Response from Leon Thomsen:

I, too, am disappointed at the continuing references to Poisson's ratio in AVO analysis (although usage does seem to be shifting slowly toward V_P/V_S). However, the shortcomings in the use of Poisson's ratio are not due to any failure of linear elasticity (as Gretener states), but rather to the nonlinear connection between Poisson's ratio and V_P/V_S (what Gretener dismisses as a "mathematical substitution"), and the consequent mathematical complications that are introduced into AVO analysis.

The difficulties that Gretener sees in the computation of a static Poisson's ratio from dynamic velocities arise from a time-dependent aspect of poroelasticity (the difference between drained and undrained behavior), not from nonlinear elasticity. This was fully explained in the pages of *TLE* (Thomsen, 1996), which as Gretener notes, has never been challenged. If Gretener will explain just *why* he finds this explanation is "unconvincing," perhaps the explanation can be enhanced.

—LEON THOMSEN
BP
Houston, Texas, U.S.

Response from Peter Gretener:

Thomsen objects to my dismissal of his 1996 paper (*TLE*) as "unconvincing"—a decision based on the fact that his final equation (10) still contains two question marks.

Point No. 1: Thomsen himself alludes to the fact that nonlinear elasticity is the basic problem when he writes: "The problem is that Poisson himself only applied them to materials (like iron) which closely approximate his assumptions (homogeneous, isotropic, linearly elastic). By contrast, we often apply them to rocks (which are anisotropic, inhomogeneous, and quasi-poroelastic instead) to which Hooke's law is *not* directly applicable. Unless we are careful, this can get us into trouble."

Point No. 2: It goes without saying that the Gassmann equation refers to effective stresses. Thomsen contends that for the rapid pressure pulses of a seismic wave the liquid filled porous rock acts as a closed system (undrained). This accounts for the slower rise in velocity with increasing stress as shown by Domenico (*TLE*, 1995). The strong difference in V_P for the dry and wet condition, however, results from the replacement of a low velocity pore filler (gas) by a high velocity pore filler (liquid) as described by the Time-Average equation. It is caused by the heterogeneous nature of rocks, a fact incompatible with linear elasticity. Therefore my equation (4) above stands without any question marks.

Bottom line: We both agree that it is not good practice to define the PS ratio via the Poisson's ratio.

—PETER GRETENER

P.S. I thank Larry Lines for his patience while acting as sounding board.

Response from Leon Thomsen:

"In response to Point No. 1: In the second part of the (1996) quote above, the emphasis was *not* that real rocks differ from Poisson's assumption of *linear elasticity*, but rather that they differ from his assumptions in other ways. In 1996, I argued extensively that, although rocks do show nonlinear behavior in some contexts, the essential *seismic* issue is their *poroelasticity*, i.e. the phenomena that arise because of the presence of both grains and pores.

(Continued on p. 72)

Dear Editors,

I enjoyed Linda Sternbach's recent October *TLE* "Unsolved mysteries ..." article and agree with most of its conclusions regarding the seeming lack of widespread innovation adoption over the last 20 years. Having been on the software services side of the business for the last seven years I have certainly seen the slow pace of technology adoption in the market. Upon attending this year's SEG Annual Meeting in Salt Lake City, I was surprised to see how slow the industry appears to be progressing in technology adoption, especially relative to the amount of technology available.

I do want to share some comments regarding two questions in this article. Sternbach mentioned that the "integrated geoscientist" emerges only via postcollege cross training. While this is probably true for the majority of graduates, there are some exceptions. For example, students graduating from the Masters in Exploration Geophysics program at Stanford University have taken courses in seismic interpretation, petroleum geology, sedimentary basin analysis, practical and theoretical well logging, rock physics, migration, deconvolution, "hands-on" seismic processing, and supportive courses in petroleum and electrical engineering. As part of the program, all students participate in industry internships in which they engage in "real life" geophysical exploration or exploitation activities. When I started my professional G&G career with a major oil company after graduating from Stanford, I hit the ground running with far more practical seismic interpretation experience than the majority of my peers. While we need more, there are some university programs from which integrated geoscientists emerge ready to take on seismic interpretation challenges.

Regarding the question "Why did automated computer interpretation stall out?", I wholeheartedly agree with Sternbach's zeroing in on unassigned fault naming as one of the biggest time wasters in the interpretation process. She suggests that certain technologies, for example voxel picking, can help eliminate those types of (what should be) archaic processes. Short of full volume interpretation, however, other software solutions help address this issue. For example, GeoGraphix's SeisVision application allows the interpreter to quickly pick faults in any number of arbitrary views, then view the individual fault segments in a simple 3D visualization environment. In that environment the interpreter easily sees which fault segments appear to be part of the same fault plane and simply assigns them to a given fault surface. The application tests the topology between the segments to verify whether the surface is geometrically valid.

This approach allows an interpreter to quickly explore "what-if" scenarios with various fault geometries. This process itself helps him/her begin to understand the structural architecture of the geologic setting rather than struggle with, as you point out in your article, "the cumbersome interpretation process of drawing unassigned faults and assigning them names." (In the interests of full disclosure, I did work at GeoGraphix from 1995 to 1999).

Again, I thank Linda Sternbach for the stimulating article. Looking at the industry demographics published in the same issue, it is easier to understand why "entrenched workflows" are so predominant, and why technology innovation can be so slowly adopted. Articles like this one drive discussions that eventually help set us in the right direction.

—RICHARD E. HERRMANN
Denver, Colorado, U.S.

(Round Table, from p. 70)

Linearly elastic media are those for which the stress is linearly proportional to the strain. Of course, for any material, at sufficiently large strains, the linear assumption does fail, but it is poroelasticity that causes the observed seismic dependence of V_P/V_S on fluid content. This point is reinforced just below.

In response to Point No. 2: So, here is the sought-for enhancement to the previous explanation. We assume linear behavior (i.e., small strains, as in seismic waves, far from the source) in an inhomogeneous, poroelastic rock. The *undrained* V_P is greater than the *drained* V_P because, in the *undrained* case, the stiffness of the fluid contributes to the overall stiffness of the rock, whereas in the *drained* case, this does not happen, because the fluid simply drains away as the overall pressure increases. (*Undrained* behavior is seen in seismic experiments, or in suitably constructed static experiments; *drained* behavior is seen in very slow dynamic experiments, or in suitably constructed static experiments.) The amount of stiffening in the *undrained* case obviously depends upon the stiffness of the fluid (there is a formula for this in any mathematical discussion of poroelasticity); a highly incompressible fluid (like brine) stiffens the *undrained* response more than does one with low incompressibility (like gas). In fact, in the limit, a very compressible gas, even if *undrained*, provides no stiffening at all; in other words, the *undrained* formula, for V_P in a rock with gas in the pores, reduces exactly to that of the *drained* case. Hence the *undrained/drained* argument does, in fact, explain the fluid-substitution (brine/gas) seismic experiments, and supports the use of *seismic* AVO to detect the type of fluid in the pore space.

Linearity and inhomogeneity are *not* necessarily incompatible. In fact, the argument above lies entirely in the realm of linear mechanics, *even though* the medium is assumed to be inhomogeneous. This success of the linear argument is reassuring, because the strains in seismic waves (far from the source) are so small that any nonlinear effects attributed to them would require an enormous theoretical stretch (to make up for the miniscule physical stretch!).

On the other hand, as Gretener points out, real rocks do show a nonlinear response whenever they are subjected to large strains (in measurements of velocities, for strains larger than about 10^{-6}). When we perform static experiments, usually the strains are larger than this, so that nonlinear behavior is usually observed, leading to Gretener's equation (4) above, which of course is correct, as written.

Excluding cases of large strain, hence remaining in the realm of linear poroelasticity, we can summarize this extended discussion as

$$(V_P/V_S)_{drained} = (V_P/V_S)_{gas} < (V_P/V_S)_{brine} = (V_P/V_S)_{undrained}$$

which indicates (in the middle) that seismic V_P/V_S does depend upon fluid content (because of the poroelastic argument). Of course, AVO analysis is complicated by seismic anisotropy, but that is a separate issue.

Bottom line: Use what you really measure (and assume) and what really matters for (isotropic) AVO—the PS ratio. We both agree that it is not good practice to define the PS ratio via the Poisson's ratio.

—LEON THOMSEN