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Do core-sample measurements record group or phase velocity?

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keywords: *velocity, phase, anisotropy*

ABSTRACT

Laboratory core-sample measurements do not record elastic constants directly; they record traveltimes from which the elastic parameters must be deduced. But do the measured traveltimes represent group velocities, phase velocities, or something else? For propagation down symmetry directions group and phase velocity are the same so there is no ambiguity. However, propagation in nonsymmetry directions is key to determining a complete set of elastic constants. In nonsymmetry directions there is no guarantee the energy radiated from the source will travel straight up the core to the receiver; the leading planar portion of the wavefront may crab sideways and partially or completely miss its desired target. Our model results show that unless the miss is complete picking first breaks will still give a good phase-velocity arrival time, the amount of additional error due to the miss being less than the normal random measurement errors of one or two percent. Unfortunately it is still true that even a small error of only one or two percent in a crucial nonsymmetry-direction phase-velocity measurement can result in a significant error in the derived value of C_{13}, especially for qP waves. If good qP *and* qSV nonsymmetry-direction measurements are both available, however, the error bounds on C_{13} can be considerably improved.

INTRODUCTION

Elastic parameters of rock samples are typically measured in the laboratory by cutting cylinders out of samples of the rock, affixing a transducer to either end, and measuring the travel time of ultrasonic waves across the sample. Layered rocks such as shale (likely to be transversely isotropic) are usually cut at angles to the layering of 0°, 90°, and 45° (as shown diagrammatically in Figure **??**). Elastic constants are then determined from the set of recorded travel times.

Before we can determine accurate elastic constants from the recorded traveltimes, however, we need to know what velocities the recorded traveltimes depend on. The-

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oretically we know that if we could somehow do the experiment using ideal point sources and receivers, we would really be measuring group velocities along the direction from point source to point receiver. Similarly, if we could somehow do the experiment using infinite parallel planar sources and receivers, we would really be measuring phase velocities along the direction normal to the source and receiver planes. While neither of these idealized experiments is possible in practice, we usually expect to be close enough to one extreme or the other to know what we are measuring, and for the error to be reasonably small. But is this common assumption correct? In particular, do the traveltimes measured in a laboratory experiment like the one in Figure **??** give us accurate vertical phase velocities, accurate vertical group velocities, or something in between?

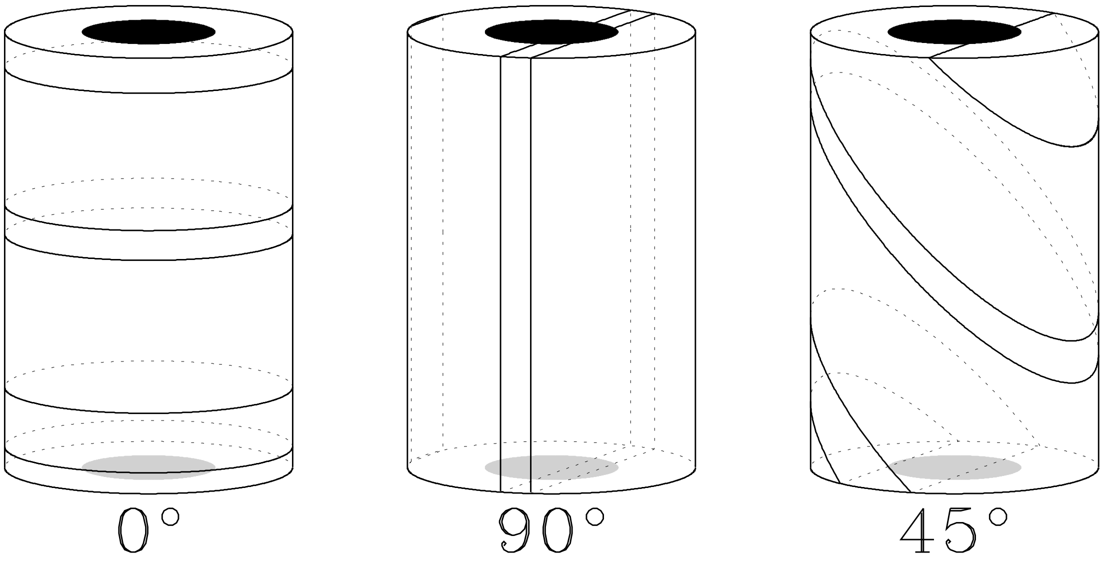

Figure 1: Shale cores cut at 0°, 90°, and 45°. The disks at the top and bottom of each core show the relative size of the P-wave transducers. The S-wave transducers were almost twice as wide (nearly as wide as the cores themselves). joe1-Cores [ER]

To find out, we examine the results of a computer finite-difference model patterned after a laboratory experiment done by Vernik and Nur (in press). Their experiment was chosen because it must be close to the "worst-case" likely to be encountered for any geological sample. The P-wave transducers were 12 millimeters wide but 40 millimeters apart, so the distance the waves traveled was fully three times farther than the size of the source and receiver. Furthermore, the anisotropy of their Bakken Shale sample was quite severe, about the worst we might expect to encounter in a geological sample. Despite these vicissitudes, Vernik and Nur proceeded on the assumption (based on geometrical arguments) that their measured traveltimes still represented phase velocities.

Our numerical modeling shows that Vernik and Nur were correct to do so. The only phase-velocity error due to the severe anisotropy that might have affected their results is a .5% delay in the 45° P-wave measurement. Given that they found typical random errors on their measurements were about 1% to 2%, an additional .5% of error is almost insignificant. The numerical model furthermore shows that for the experiment to have measured P *group* velocities to a similar level of accuracy, the transducers should have been at most *2mm* wide! We conclude that laboratory experiments of this kind should almost always measure accurate phase velocities; at the very worst they may measure something hard to interpret in the never-never land between group and phase.

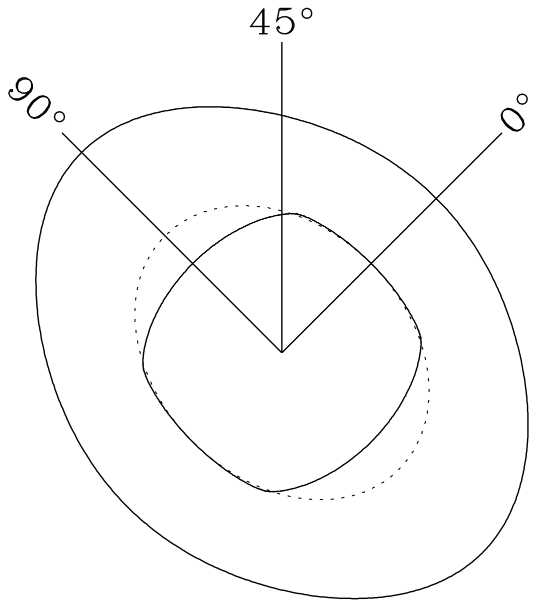

Figure 2: Impulse-response curves showing the shapes of wavefronts propagating in the medium used in our numerical models: qP (outer curve), qSV (inner solid curve), and SH (dotted). The 90°, 45°, and 0° labels show the direction of vertical for the corresponding shale-core orientations. joe1-Waves [NR]

LABORATORY AND NUMERICAL MODELS

Figure **??** shows the laboratory experimental configuration. The cylinders are cut from a sample of Bakken shale (Vernik and Nur, in press); Figure **??** shows the shape of qP, qSV, and SH wavefronts emanating from a point source in this shale. The layers shown in Figure **??** are merely to indicate orientation (the real layers would hopefully be on a much finer scale relative to the size of the core). The aspect ratios of the cores in Figure **??** are correct (40mm tall and 26mm wide); the disks at the top and bottom of each core show the true relative widths of the P-wave transducers (12mm). The SV and SH transducers are almost twice as wide, 20mm, nearly as wide as the core itself.

In this paper we are interested in showing how anisotropy can effect the direct wave from the source to the receiver transducer. A very simplified model is more than adequate for this purpose. We will not clutter the model by attempting to include the rather complex boundary conditions entailed by tilted-axis anisotropy interacting with a truncated cylindrical surface; we will also keep the model two-dimensional. The elastic constants we used for the numerical simulation are $C_{11} = 20.16$, $C_{33} = 11.97$, $C_{55} = 4.00$, $C_{66} = 6.86$, and $C_{13} = 5.51$. The density has been normalized out so these are all in units of mm/μs.

Vertical phase versus vertical group

Before examining our simplified numerical simulation of Vernik and Nur's experiment, we will first show two complementary "ideal" simulated experiments, one designed for measuring the vertical group velocity and the other one for measuring the vertical phase velocity.

The lower plot in Figure **??** shows how vertical group velocity could be properly measured by using extremely small source and receiver transducers. The anisotropic wavefront radiates out from the point source at the bottom; the point receiver at the top detects the part of the wavefront with vertically traveling energy as it passes by. The distance between the two transducers divided by the measured traveltime gives the vertical group velocity.

The upper plot in Figure **??** shows how true phase velocity could be properly measured. The source must be wide enough to launch a reasonable facsimile of a plane wave. Since the source is not infinite, the "plane wave" is truncated; the receiver on the top must be positioned where it can sample the flat central part of the wavefront, away from the diffracting truncated edge. Note that while the source transducer runs from -20 to $+20$ mm, the flat part of the wavefront in the figure runs from -30 to $+10$. While the wavefront has traveled vertically 40mm from the bottom of the model to the top, it has also slipped sideways 10mm. The receiver in the upper plot in Figure **??**, while OK, is perilously close to the edge; a position 10 or 15 millimeters further to the left would have been better. Note that if the source transducer had been infinite, there would be no such complications: the position of the receiver would be irrelevant.

The upper plot in Figure **??** was constructed by summing multiple copies of the lower plot in Figure **??** shifted from -20 to $+20$ mm. The flat part of the wavefront in the upper plot is the sum of all the shifted copies of the highest point on the wavefront in the lower plot. From this basic relationship, we can see that we could have measured the vertical phase velocity directly from the lower plot by simply shifting the receiver over to where the wavefront first encountered the upper surface (around the -11mm position) and dividing by the *vertical* distance from source to receiver (40mm in the figure). Note this distance is less than the true distance between the source and the offset receiver (which would be $\sqrt{40^2 + 11^2}$mm). We are now ready to examine similar figures showing the situation in Vernik and Nur's experiment.

MODEL RESULTS

P waves

Figure **??** shows the situation for P waves for two different core layering orientations in Vernik and Nur's experiment.

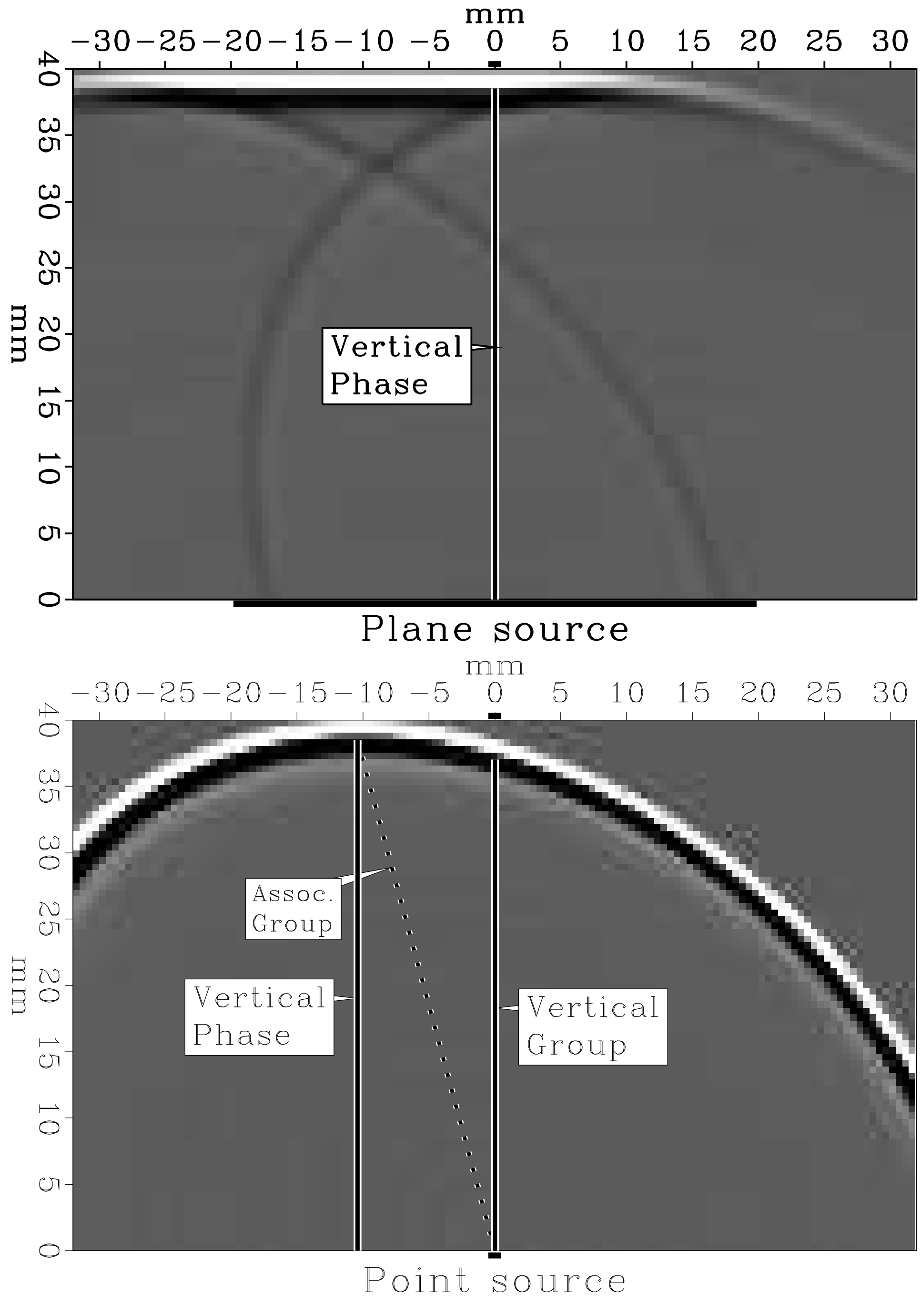

Figure 3: Snapshots demonstrating the two "ideal" core-sample experiments. The positions and sizes of the source and receiver transducers are indicated respectively by thick horizontal lines at the bottom and top of the model. Top: an infinite-source experiment for measuring vertical phase velocity. Bottom: a point-source to point-receiver experiment for measuring vertical group velocity. (As you can see the two experiments are really approximations, the "infinite" transducer is 40mm wide and the "point" transducers are 2mm wide.) The labeled vertical and near-vertical distances mark the progress of key points on the radiated wavefront during its travels from the source up to the top of the model. Three distinct velocities are indicated: the vertical group (energy) velocity, the vertical phase (plane-wave) velocity, and finally the (nonvertical) group velocity *associated with* the vertical phase velocity. (If the medium were isotropic, all three of these would be equivalent.) joe1-phasegroup [NR]

offset (mm)	break	% delay	peak	% delay	% amplitude
-12 (peak)	10.29	0	10.79	0	100
-6	10.30	.1	10.82	.3	85
0 (actual)	10.34	.5	10.94	1.5	52
6	10.48	1.8	11.17	3.7	29
12	10.78	4.8	11.52	7.1	16

Table 0.1: Arrival times from the left part of Figure **??**, showing how the picked arrival time would vary with source-receiver transducer offset. Two different sorts of arrival times are shown, the first-break time and the first-peak time. The first-break time is somewhat less sensitive to side-slipping. The rightmost column shows how the peak amplitude of the recorded trace drops off with offset.

In the upper plot the layers run vertically; there is no tendency for the wave energy to "slip sideways" and the flat part of the wavefront impacts the receiver transducer well centered. We can see from the figure that things are working correctly in this case. Mathematically this is because the group and phase velocities happen to be the same for this propagation direction. The same good behavior also occurs when the layers in the core run horizontally, because this also sends the waves along a symmetry direction where the group and phase velocities are identical (for all wavetypes).

The situation is not so happy in the lower plot. Here the layers are at a 45° angle, running from the lower right to the upper left. The P wave travels faster along the layers than across them; as a result the "flat" part of the wavefront (containing the main focus of energy) tends to follow the layers and slips sideways to the left. In our particular example the accumulated sideways slip from bottom to top happens to be about the same as the transducer widths, so the "flat" part of the wavefront only just grazes the edge of the receiver. What traveltime will be measured in this case?

The answer is contained in Figure **??**. The left part of this figure shows the result of a tiny "seismic survey" over the shale-core model. The source transducer is held fixed. The horizontal axis shows how the trace recorded at the receiver would vary with offset if the receiver were moved around (instead of glued in place). The first break on the earliest arrival occurs at 10.29μs for an offset of -12mm. (The "correct" phase-velocity arrival time as defined by running the model with infinite-length transducers is very slightly later, 10.30μs. We will remark further on this in the discussion.) The first break at zero offset (and thus what corresponds to the signal recorded in the actual experiment) occurs at 10.34μs. If this time were used to calculate the vertical phase velocity it would cause an error of only .5%, which is smaller than the typical errors Vernik and Nur encountered in picking first breaks in their experiment, about 1% for P waves and 2% for S waves. (The only significant effect of the borderline miss in our simulated experiment is a 48% drop in trace amplitude.)

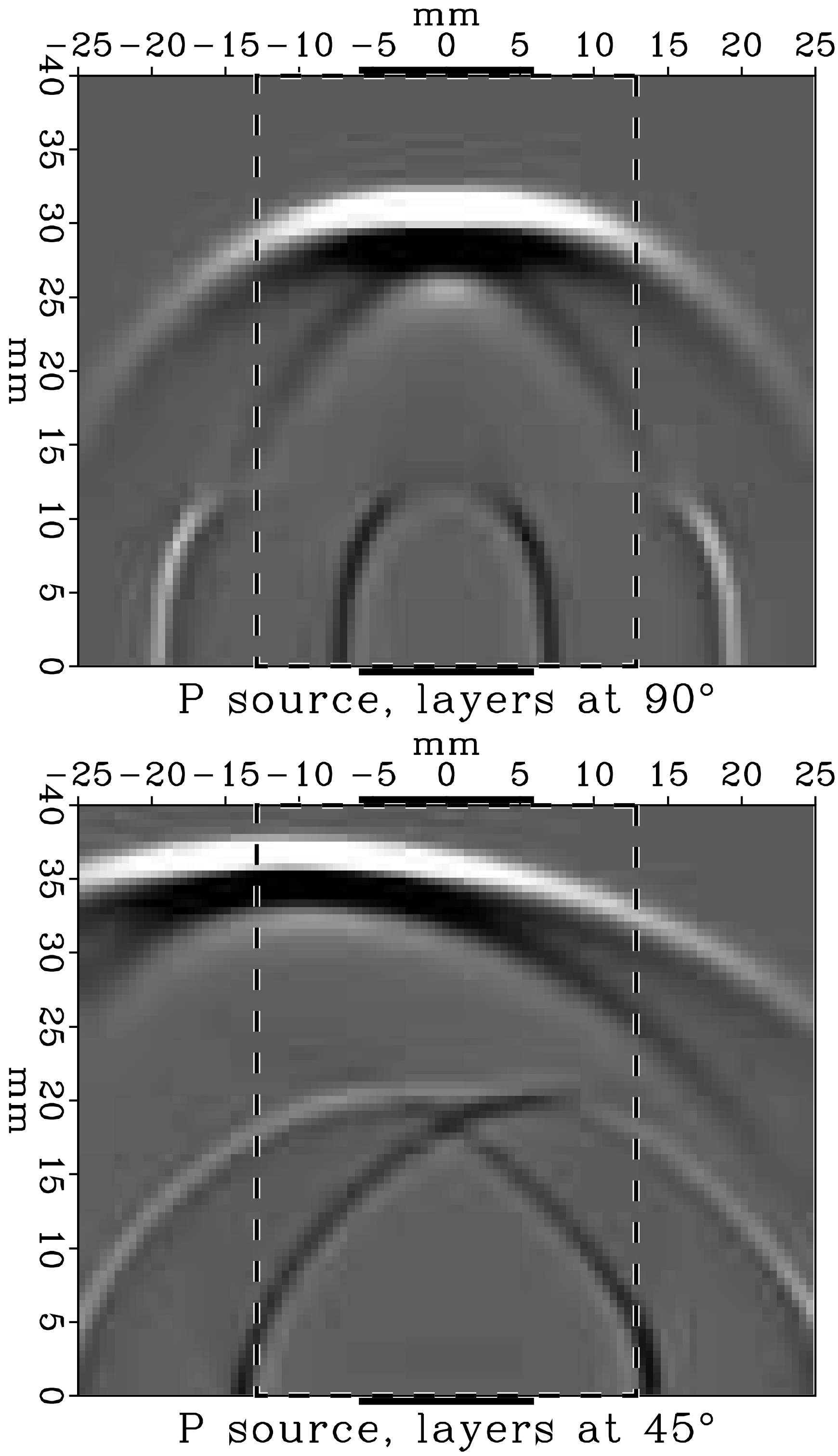

Figure 4: Snapshots showing the behavior of qP waves in our 90° (top) and 45° (bottom) core-sample simulations. (The top snapshot shows the situation at 7.5μs, the bottom at 10.μs.) The dashed lines show the width of Vernik and Nur's cores, while the thick solid lines at the bottom and top show the size and positions of the P-wave source and receiver transducers. Note in the 45° case how the leading part of the wavefront is aiming to miss its intended target, hitting the top of the core somewhat to the left of the receiver instead. joe1-Pexam [NR]

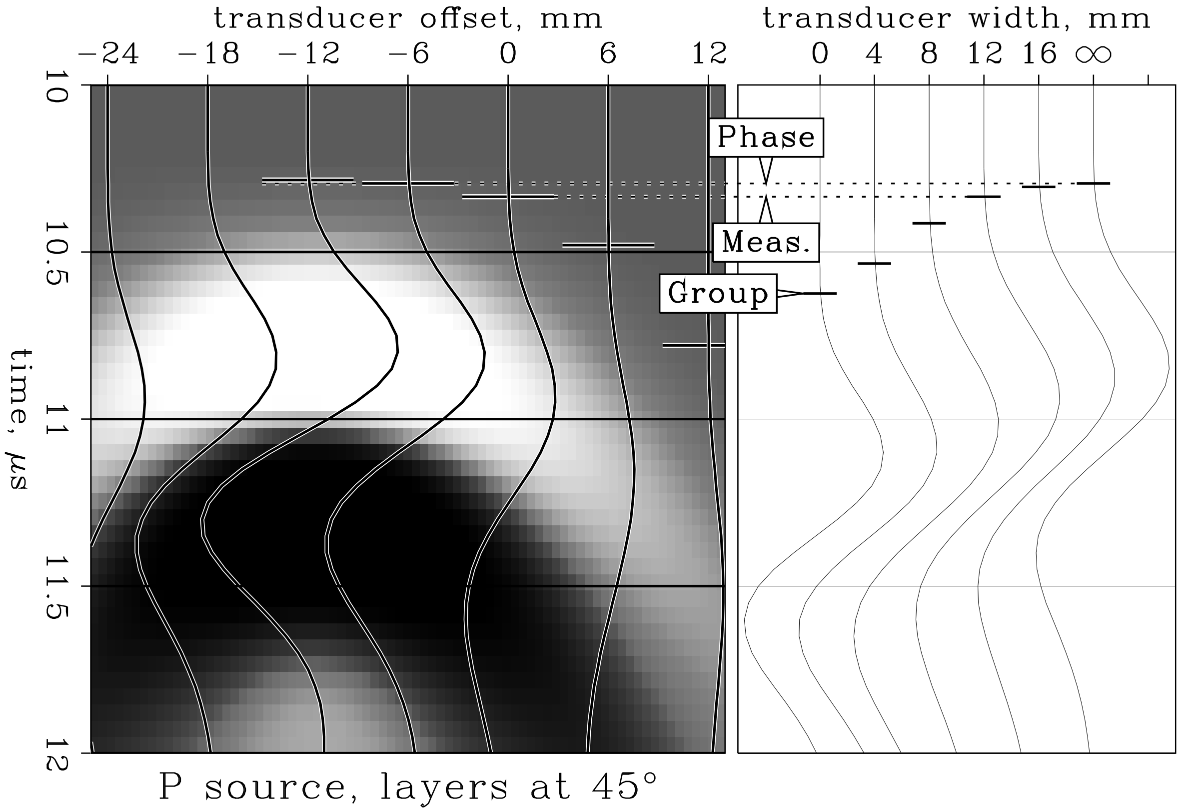

Figure 5: A double plot showing how the signal recorded in the 45° P-wave case (lower plot in Figure **??**) would change as a function of two normally fixed parameters, source-receiver transducer offset (left subplot) and source and receiver transducer width (right subplot). In the "real life" case the transducer offset was fixed at 0mm and the transducer width was fixed at 12mm. In the left subplot the transducer width is held at 12mm, but the transducer offset is allowed to vary from −24mm to +12mm (with individual wiggle traces shown every 6mm). In the right subplot the transducer offset is held at 0mm, but the transducer width is allowed to vary by 4mm steps from 0mm to 16mm (and then is abruptly jumped to ∞). First-break times on various traces are marked by short horizontal lines. Three significant first-break times are labeled. The 0mm-width 0mm-offset trace defines the group-velocity arrival time. The 12mm-width 0mm-offset trace occurs in both subplots and shows the measured arrival time for the "real life" case. The ∞-width trace (offset is irrelevant) defines the phase-velocity arrival time. Except for a tiny mismatch (caused by a wavelet phase shift) the ∞-width first-break arrival time is the same as the −12mm-offset first-break arrival time. joe1-Palt [NR]

transducer size (mm)	break	% delay	peak	% delay
∞ (phase)	10.30	0	10.84	0
24	10.30	.0	10.84	.0
20	10.30	.0	10.85	.1
16	10.31	.1	10.88	.4
12 (actual)	10.34	.3	10.94	.9
8	10.42	1.2	11.02	1.7
4	10.54	2.3	11.08	2.2
0 (group)	10.63	3.2	11.10	2.4

Table 0.2: Arrival times from the right part of Figure **??**, showing how the picked arrival times depart from the ideal ∞-transducer time for various finite transducer sizes. Two different sorts of arrival times are shown, the first-break time and the first-peak time.

Table 0.1 shows how the first-break time varied with offset for the traces shown in the left part of Figure **??**. From the table we can discern that if the miss had been smaller (say if the anisotropy had been less, the transducers had been wider, or the core had been shorter), there would have been an insignificant delay in the first break and only a very small drop in amplitude. If the miss had been by more, on the other hand (say if the anisotropy had been slightly greater, the transducers had been narrower, or the core had been taller), the first break would have been significantly delayed. Essentially, if any of the leading "flat part" of the wavefront manages to hit the receiver it is enough to initiate a first break at the true phase-velocity arrival time; if the flat part misses completely the error can be substantial.

The right part of Figure **??** demonstrates this another way, by showing the results of re-running the numerical experiment for a range of transducer sizes. Zero-offset traces corresponding to what would be measured by a laboratory experiment are shown. With a point source and point receiver, the first break measures the group-velocity arrival time, 10.63μs. As the transducer size is increased towards 12mm the first break moves rapidly earlier to within .5% of the phase-velocity arrival time. As the transducer size is increased yet further the first-break time closes in on the phase-velocity arrival time more slowly, finally reaching it when the transducer width is 20mm. All wider transducers measure the phase-velocity arrival time. (Table 0.2 gives the numbers for this case.)

S waves

What about S waves? For transversely isotropic (TI) media, there are two kinds of S waves, SH and qSV.

The SH wavefronts in TI media should be exactly elliptical, although the amount of elliptical anisotropy can be large. Other than this slight simplification (P-wave wavefronts aren't normally elliptical in TI media, but they probably aren't too weird in ordinary rocks either) measuring SH traveltimes should theoretically be very similar to the case for P waves already discussed. In practice, S-wave transducers are usually twice as large as P-wave transducers in any given experiment, so it is even more likely that a good first break corresponding to an accurate phase velocity will be measured. Figure **??** shows how this works in our numerical simulation of Vernik and Nur's experiment; although the "sideways drift" of the wavefront in the 45° SH case is nearly the same as for the 45° qP case in Figure **??**, the receiver transducer solidly encounters the all-important "flat" part of the wavefront instead of merely being grazed by it.

The case for qSV waves is trickier. In TI media these waves can be highly anisotropic and non-elliptical (for example see Figure **??**). On the minus side this means that "esoteric" anisotropic effects such as wavefront cusps could complicate picking the "first break". Cusps are always associated with precursive energy leading the main qSV arrival (theoretically anchoring it all the way back to the tail of the already-arrived qP wavefront). They are point-source, not plane-source phenomena, so in the worst case they might occur in experiments such as ours at the edges of the "flat part" of the wavefront. If qSV cusps prove to be problem in an experiment, it should be possible to lessen the confusion by making the transducers wider. On the plus side qSV wavefronts almost always have a near-symmetry about 45°, so the amount of "side slip" for 45° cores should be small. Figure **??** demonstrates that the qSV waves in our numerical simulation are indeed the best-behaved of the lot. The sideways drift is only 3mm to the right, insignificant compared to the size of the S transducers. (Note the other two wavetypes drifted to the left.) Furthermore, the qSV wave was not preceded by a significant amount of precursive energy, so there should have been no special trouble picking the first break off the recorded signal.

It is somewhat ironic that qSV waves (normally thought of as the most complex of TI wavetypes) appear to be the most well-behaved here, while "mild-mannered" qP waves are the likeliest to be mismeasured.

DISCUSSION

Group(θ) vs phase(θ) vs group($\phi(\theta)$)

For another irony, consider the relation between group and phase velocity, usually written

$$V_{\mathrm{group}}^2(\phi_{\mathrm{group}}(\theta_{\mathrm{phase}})) = V_{\mathrm{phase}}^2(\theta_{\mathrm{phase}}) + \left(\frac{dV_{\mathrm{phase}}(\theta_{\mathrm{phase}})}{d\theta_{\mathrm{phase}}}\right)^2. \tag{1}$$

This equation is just the scalar part of the statement "the group velocity vector is the sum of the phase velocity vector (perpendicular to the wavefront) and the sideslip

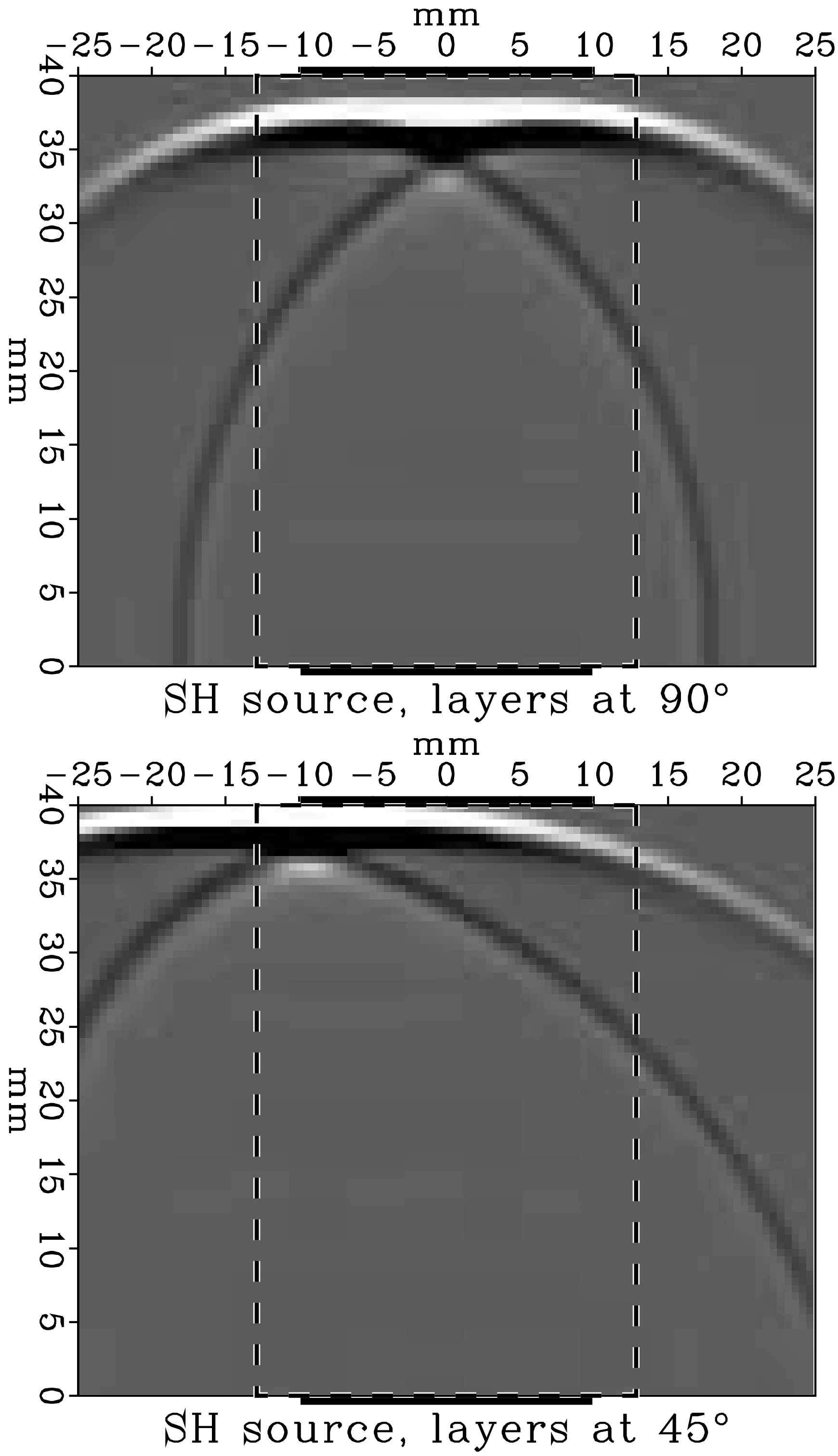

Figure 6: Snapshots showing the behavior of SH waves in our 90° (top) and 45° (bottom) core-sample simulations. (The top plot shows the situation at 15.μs, the bottom at 17.5μs.) The SH wave in the 45° case slips to the left about the same amount that the qP wave did in Figure **??**, but the transducers are twice as wide so the wavefront hits the receiver anyway. joe1-SHexam [NR]

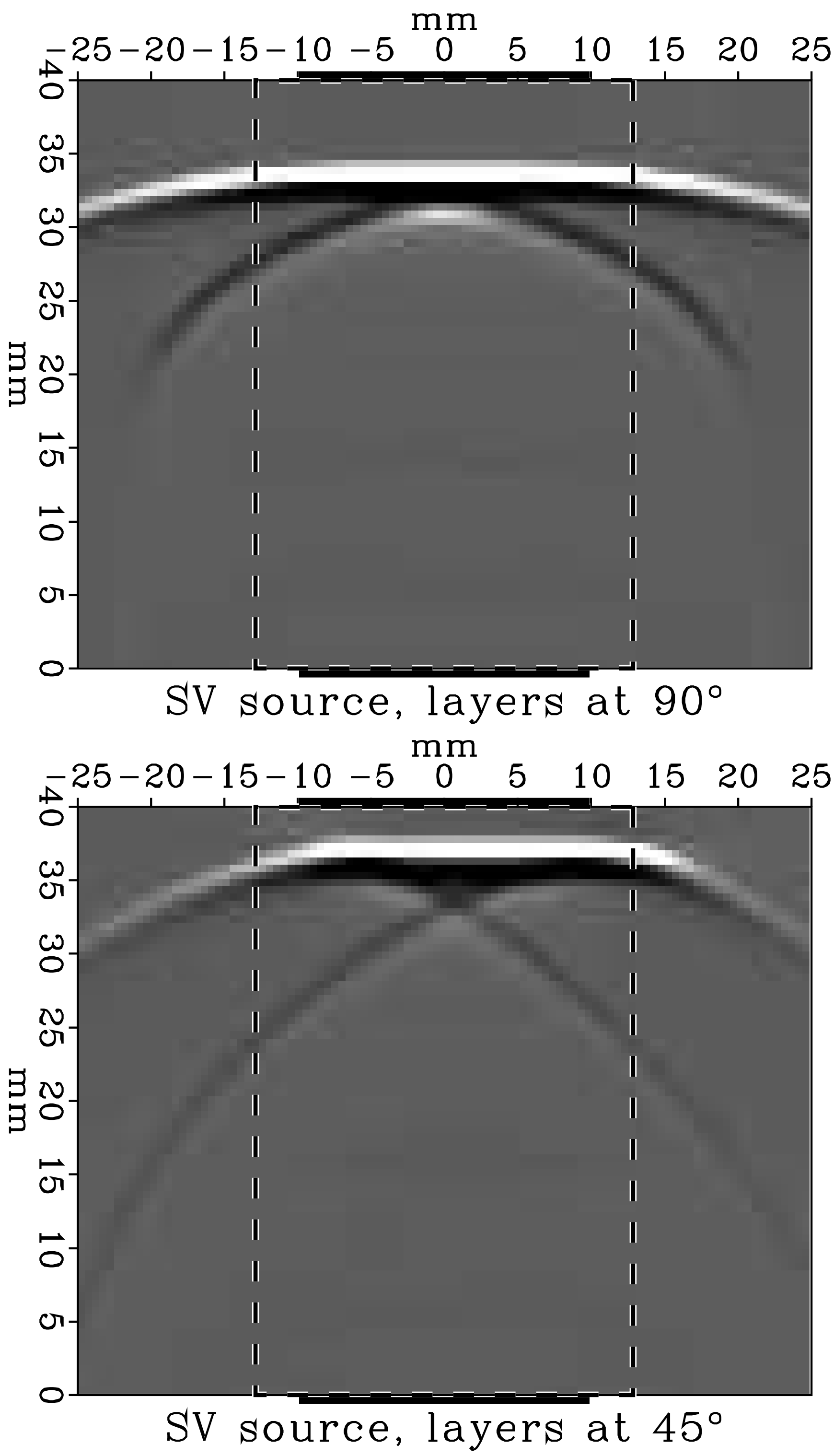

Figure 7: Snapshots showing the behavior of qSV waves in our 90° (top) and 45° (bottom) core-sample simulations. (Both snapshots show the situation at 17.5μs.) It so happens that for this wavetype the 45° phase direction and its associated group direction are nearly the same, so the wavefront in the 45° case (lower plot) does not significantly crab sideways. $\boxed{\text{joe1-SVexam}}$ [NR]

Measurement	velocity
$V_P(0°)$	3.46
$V_P(45°)$	3.86
$V_P(90°)$	4.49
$V_{SV}(0°)$	1.99
$V_{SV}(45°)$	2.13
$V_{SV}(90°)$	2.02
$V_{SH}(0°)$	2.00
$V_{SH}(45°)$	2.33
$V_{SH}(90°)$	2.62

Table 0.3: Velocity measurements in mm/μs for a sample of Bakken Shale, from Vernik and Nur (in press).

velocity vector (parallel to the wavefront)". This equation immediately implies the inequality

$$V_{\rm group}(\phi_{\rm group}) \geq V_{\rm phase}(\theta_{\rm phase}). \tag{2}$$

or "group velocities are greater than (or equal to) associated phase velocities". Earlier in this paper, however, we saw that "vertical phase velocities are greater than (or equal to) vertical group velocities", or mathematically

$$V_{\rm group}(\psi) \leq V_{\rm phase}(\psi). \tag{3}$$

These inequalities seem contradictory at first glance, but they are not because they refer to different things. Equation (2) compares the phase and group velocities associated with the same point on a wavefront. Equation (3) compares the phase and group velocities for the same propagation direction (ψ). The solid vertical lines in Figure **??** show how the phase and group velocities for the same (vertical) propagation direction are related. The tilted dotted line in the same figure indicates how the vertical phase velocity also has an associated (non-vertical) group velocity. The non-vertical group velocity associated with the vertical phase is faster than the vertical phase, which is in turn faster than the vertical group!

Thomsen's anisotropy parameters and errors

These antagonistic inequalities do suggest that phase and group directions are important and should not be confused. This was shown more rigorously by Thomsen (1986), who found that for weak anisotropy associated phase and group velocities could be taken as equal to first order, but associated phase and group directions could not. Unfortunately his unnumbered equation on page 1962 giving δ in terms of 0°, 45°, and 90° P-wave rock-core velocity measurements makes just this error, using group directions which must be phase directions for the equation to be valid. Happily, from the results in this paper we know that rock-core measurements measure phase velocities, not group, so Thomsen's equation can be corrected by simply using phase velocities and directions throughout:

$$\delta = 4\Big[V_P(45^\circ)/V_P(0^\circ) - 1\Big] - \Big[V_P(90^\circ)/V_P(0^\circ) - 1\Big], \tag{4}$$

where $V_P(\theta)$ is the P-wave *phase* velocity for the phase-direction θ.

Using this equation the δ anisotropy parameter can be calculated from the three experimentally measured P-wave phase velocities. Table 0.3 lists the velocities found by Vernik and Nur for their Bakken Shale sample; plugging those numbers into equation (4) we find $\delta = .165$. Unfortunately, each of the three P-wave velocity measurements has a standard deviation of about 1%; the implied absolute standard deviation in the calculated δ is about .05, of nearly the same order as δ itself.

This is not the whole story; Equation (4) is a weak-anisotropy approximation to the equation

$$\delta = \frac{(C_{13} + C_{55})^2 - (C_{33} - C_{55})^2}{2C_{33}(C_{33} - C_{55})} \tag{5}$$

given in Thomsen's paper as equation (17*) (Thomsen, 1986). (Thomsen's δ is itself a weak-anisotropy approximation to a more fundamental small-offset normal-moveout parameter. See for example Lyakhovitsky and Nevsky (1971).) Our anisotropy is not weak, so it is not safe to use equation (4) as an approximation for equation (5).

The "new, improved" equation does have some drawbacks. Equation (4) requires only $V_P(0^\circ)$, $V_P(45^\circ)$, and $V_P(90^\circ)$; equation (5) contains the term C_{55}, and so requires in addition $V_{SV}(0^\circ)$ (and/or $V_{SV}(90^\circ)$). Working with equation (5), assuming a standard deviation of 2% for SV velocity measurements, we find $\delta = .075 \pm .077$. This is quite a different answer from the first one and suggests we should be wary of equation (4) if the anisotropy is not weak.

Of course, this is *still* not the whole story; if we make use of the 0° and 90° SV measurements, why not the 45° measurement as well? There may even be compelling reasons to include it; if we assume the 45° measurement has a standard deviation of 2% in Vernik and Nur's example (like we did for the 0° and 90° SV ones) we find that the 45° SV measurement actually constrains C_{13} (and thus δ) almost twice as well as the 45° P measurement. How is this possible, given that the P velocity measurements were assumed to be twice as exact? It happens because a small change in C_{13} has more effect on 45° SV phase velocities than it does on 45° P phase velocities. (This is not some fluke of our particular example; this should almost always be the case.)

It is unlikely the values of C_{13} calculated using the two different 45° measurements will coincide. For our example we find from the P measurement $C_{13} = 4.79 \pm .78$ and from the SV measurement $C_{13} = 6.20 \pm .46$. If the two values wildly disagree it probably indicates something is wrong with one of the measurements; here from the modeling we suspect the 45° P measurement could be .5% too slow. If we adjust the

$V_P(45°)$ value to correct for that we get $C_{13} = 5.13 \pm .78$ for our P-only value, which narrows the mismatch to a more acceptable level.

Ideally all available information should be used to find the most likely value for C_{13}; knowledge of each measurement's error bars along with its contribution to the answer can then be used to put error bars on C_{13}. Doing this for Vernik and Nur's model (using the adjusted 45° P measurement) we find $C_{13} = 5.94 \pm .37$, and $\delta = .189 \pm .045$.

Clearly the value of δ we find is rather *disturbingly* sensitive to which equation and subset of available measurements we use. (For this example we could have found δ to be .075, .108, .165, .180, .189, or .217!) Of course not all values are equally justifiable; great care must be taken. Vernik and Nur decided to discard their 45° SV measurement because it implied such an (improbably?) large value for δ.

The situation is not so grim for Thomsen's ϵ anisotropy parameter; the exact equation is

$$\epsilon = V_P(90°)/V_P(0°) - 1. \tag{6}$$

For Vernik and Nur's example we find $\epsilon = .298 \pm .019$, a value indicating a rather extreme level of anisotropy.

First break versus first peak

In the numerical samples in this paper we defined the first-break time precisely (if somewhat arbitrarily) as the point where the wavelet attained an amplitude of 1% of the maximum amplitude in the arrival. Measuring a first-break time in experimental conditions is somewhat more subjective; for this reason some experimenters prefer to pick the arrival time at the first peak instead of the first break.

Table 0.2 shows how the first-break and first-peak times vary with transducer size for the 45° P-wave case shown in the right part of Figure **??**. The time picked in the ideal infinite-transducer case is used to define the "correct" arrival time. First of all, note by comparing the "-12 (peak)" case in Table 0.1 with the "∞ (phase)" case in Table 0.2 that both picking methods are sensitive to the phase shifts in the recorded wavelet due to the effective dimensionality of the source. The 12mm transducer width is evidently not quite wide enough to fully mimic the wavelet launched by an infinite planar source, even recorded at the center of the leading flat spot. The first-break method is five times less sensitive to the difference in the wavelet phase than the first-peak method, however.

Similarly in Table 0.2 we find the first-peak method is more sensitive to variations in the recorded wavelet due to the flat spot partially missing the receiver. This should not be surprising. The first-peak method must wait until a substantial part of the wavefront's leading energy surge encounters the receiver before it can detect the arrival; if the leading focus of energy mostly misses the receiver there will be a delay while the detection method waits for more energy to arrive from the trailing portion of the wavefront to the side of the leading flat spot. The first-break method, on the other hand, ideally can detect an arrival when any of the leading "flat part" of the wavelet hits the receiver; anything short of a complete miss should result in the phase-velocity answer. (Of course in practice we have to pick the first break at some finite amplitude level, so there will always be some slight delay.) While it is true that first-break times should be more robust against anisotropic effects, Table 0.2 shows that using the first-peak time for the 12mm-wide transducers in our 45° P-wave example would still have resulted in a phase-velocity measurement error of only 1%, not that bad.

How can you tell if you're in trouble?

We have shown that unless the anisotropy is too strong, the transducers are too narrow, or the core sample is too tall, core-sample first-break measurements should measure the phase velocity. But what precisely do we mean by "too" strong, narrow, or tall? It depends on the elastic properties of the rock being measured. Of course if we knew the elastic constants we wouldn't have to do the experiment in the first place! Is there some warning sign to be found among the measurements in hand?

If the amplitude for the 45° case is anomalously low compared to the untroublesome 0° and 90° cases, this could indicate the energy is missing the receiver and thus the 45° traveltime measurement should be regarded with suspicion. Unfortunately amplitude variations in such experiments can be caused in many ways, so this is not a reliable indicator.

Another cross check is possible because some of the elastic constants are overdetermined. In particular, (as we already saw) the key elastic constant C_{13} can be found from either the 45° qP or qSV traveltime measurements; assuming the velocity measurements at 0° and 90° can be trusted, we can compare the values of C_{13} calculated from each of the two 45° measurements independently as a check. There should not be a problem with both values of C_{13} erring in the same direction due to side-slip effects. Since the phase-velocity arrival time is the earliest possible, a "partial miss" must always result in a delay, resulting in a velocity measurement that is too low. The effect of such a mismeasurement on the calculated value of C_{13} depends on the wavetype; a too-slow 45° qP phase-velocity results in finding a C_{13} (and δ) that is too low, while a too-slow 45° qSV phase-velocity measurement results in a C_{13} (and δ) that is too high.

Probably the best way to estimate whether there might be significant side-slip problems, though, is to use the (possibly inaccurate) measured elastic constants to model the problem. Analytically, the side-slip velocity is simply $-dV_{\text{phase}}(\theta)/d\theta$. In the case of qP and qSV waves in transverse isotropy the total side-slip at 45° can be expressed directly in terms of the core height H and the measured phase velocities:

$$-H\frac{(V_P(90^\circ)^2 - V_P(0^\circ)^2)}{2V_X(45^\circ)^2}\ \frac{(V_{SV}(90^\circ)^2 + V_{SV}(0^\circ)^2) - 2V_X(45^\circ)^2}{(V_P(90^\circ)^2 + V_P(0^\circ)^2) + (V_{SV}(90^\circ)^2 + V_{SV}(0^\circ)^2) - 4V_X(45^\circ)^2}, \tag{7}$$

where V_X is either V_P or V_{SV}, depending on which wavetype the sideslip is being calculated for. The corresponding equation for SH waves is quite simple:

$$-H\frac{(V_{SH}(90^\circ)^2 - V_{SH}(0^\circ)^2)}{2V_{SH}(45^\circ)^2}. \tag{8}$$

Conveniently, these equations appear to be rather insensitive to the precise value used for the 45° phase velocity; for our example $V_P(45^\circ)$ could be varied by 10% with only a 1mm resulting change in the calculated qP sideslip. (The denominator does start to blow up if $V_P(45^\circ) \approx V_{SV}(45^\circ)$, but hopefully this fact alone would already have suggested there just *might* be a problem!) If equations (7) or (8) indicate the total sideslip is greater than about half the transducer width for a given wavetype, extra care should be taken when interpreting the corresponding 45° measurement as a phase velocity.

CONCLUSIONS

Our modeling results indicate that Vernik and Nur were correct when they interpreted their core-sample traveltime measurements for Bakken Shale as representing vertical phase velocities. Although their 45° qP measurement may have suffered some error because of the strong anisotropy, our modeling predicts the magnitude of that error should be several times smaller than the normal measurement errors.

We expect that the Bakken Shale example investigated here represents about the most anisotropic geological specimen likely to be encountered in the laboratory. If this is true then the traveltimes measured in similar laboratory core-sample experiments should also represent vertical phase velocities, *assuming* the critical ratio of core-sample height to transducer width is no greater than it was in their experiment (i.e., ≤ 3). If the core-sample height to transducer width is larger it becomes more likely that some indeterminate quantity representing neither vertical phase nor vertical group velocity is being sampled; in case of doubt equations (7) and (8) can be used to estimate whether there might be a problem for the 45° measurements in a particular experimental configuration.

Our modeling indicates that as an additional safeguard first breaks should be picked instead of first peaks whenever possible. This can be important in cases like the 45° qP measurement in our example that are close to being detectably delayed due to side-slipping of the wavefront. In any case, note that it is highly unlikely a core-sample experiment could accidentally measure vertical group velocities; our numerical modeling suggests the core-sample height to transducer width ratio needs to be on the order of 20 (preferably even larger) to be reasonably sure of sampling vertical group velocities.

While it is comforting to know that core-sample traveltime measurements sample phase velocity, this still does not mean we are assured of finding accurate values for all the various anisotropy parameters we might wish to know. At least some sorts of errors are avoidable, and *should* be avoided. If we want to measure Thomsen's anisotropy parameter δ we should clearly state whether we are using equation (4) or (5); the choice of equation makes a difference (unless the anisotropy is very weak). We find equation (5) to be the more useful. Unless we have reason to discount some of our measurements we should try to reconcile and use all available information; the more independent measurements we can use the more confidence we can have in the results. In particular, the 45° SV measurement is often the single most important piece of data for constraining C_{13}.

Some errors may be unavoidable, unfortunately. For this reason it is important to have at least a rough idea of the reliability of the measured traveltimes and to use those input errors to determine what the corresponding output errors in the calculated anisotropy parameters may be. If the standard deviations in the measured traveltimes are too great the values of C_{13} and δ derived from them may be effectively meaningless. For this reason when publishing elastic constants we should state what measurements we used to calculate the tabulated constants and what magnitude of error we expect in them. Similarly, when interpreting elastic constants published by someone else we must be wary if this information is not provided (Seriff and Sriram, 1991).

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