

# “Our approach to subsurface characterization will continue a fundamental transformation... do not envision the end of geophysics era any time soon...”

- Interview with Ilya Tsvankin and Vladimir Grechka

*Ilya Tsvankin is a professor at the Colorado School of Mines and the director of the Centre for Wave Phenomena – a research consortium. Vladimir Grechka is a senior research geophysicist with Shell, based at Houston. Through their exceptional analytical skills and keen scientific insight, both have made fundamental contributions to the study of seismic anisotropy, as well as a wide range of practical problems in geophysics. For this pioneering work, they have been suitably rewarded by the SEG, Ilya receiving the Virgil Kaufman Gold Medal in 1996 and Vladimir receiving the J. Clarence Karcher Award in 1997.*

*Ilya and Vladimir were in Calgary in November 2005, during the CSEG Doodletrain week for conducting their course on anisotropy, and very sportingly agreed to our request for an interview. We found them modest, eager to share ideas, and conveying their points of view very clearly, as will become obvious from the following excerpts from the interview.*

*Bill Goodway, a well-known name in the seismic industry, also joined in and participated actively in the discussion.*

- (Photos courtesy: Penny Colton)



**S:** *Let us begin by asking you both about your educational background and work experience.*

**V:** I got my M.Sc. in Geophysical Prospecting from Novosibirsk State University, Russia in 1984 and Ph.D. in 1990 from the Institute of Geology and Geophysics, Novosibirsk, Russia. It seems it was a long time ago. I spent 10 years in the Russian Academy of Sciences, where I was involved in a number of projects in Western Siberia and Kazakhstan. Then I was at the University of Texas in Dallas for one year followed by about six years at the Colorado School of Mines, where I was working with Ilya. I joined Shell Exploration and Production in 2001 as a senior geophysicist.

**I:** I received my M.Sc. degrees in Exploration Geophysics from Moscow State University in 1978, and then my Ph.D. in Geophysics again from Moscow State in 1982. I worked at the Institute of Physics of the Earth in Moscow until 1989 before moving to the U.S. in 1990. After spending two-and-a-half years at the Amoco Research Center in Tulsa, I joined Colorado School of Mines (CSM) in 1992. I am now a Professor of Geophysics at CSM and director of the Center for Wave Phenomena—a research consortium supported by the industry and several government agencies.

**S:** *Both of you have done exceptional work on anisotropy amongst other things, and your names are well known with the people who work in this area and even outside. What made you get interested in this field?*

**V:** It is a good question. I was looking for a summer project at some point during my second year at the University. I was sort of mathematically inclined at the time, and anisotropy sciences appeared sufficiently mathematical and challenging. I began working on anisotropy-related issues in 1980, and I am still pretty much into it. Frankly, I like it. This year is my 25th anniversary.

**I:** First of all, I should mention that anisotropy studies have a very long history in Russia that go back to the 1950s and 60s. Unfortunately, many of these pioneering contributions have not been translated into English and are almost unknown in the West. I recall that when Felix Lyakhovitsky from Moscow State University gave a presentation at Amoco in 1990, Leon Thomsen recognized some of the NMO-velocity equations on his slides and pointed out that they are similar to equations in his famous 1986 paper in Geophysics. Felix replied that he had published these results back in 1971 in a major Russian journal.

For me, joining the anisotropic community in the mid-1980s was a natural move because I have always been interested in non-traditional seismological problems. You can notice that most of the titles of my papers have words beginning with “non” or “an”. The topic of my Ph.D. thesis was analysis of nongeometrical waves, which cannot be described in the conventional framework of ray theory. One of my major projects at the Institute of Physics of the Earth was devoted to nonlinear seismology. Although I had really enjoyed working on these problems, I found

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anisotropic wave propagation so fascinating that I have almost exclusively focused on it for the past 20 years. Isotropic seismology looks so bland after dealing with anisotropic models!

**B:** *Are we missing nonlinear seismic in addition to anisotropy at wide-angle propagation?*

**I:** I would not say that nonlinear phenomena are common for wide-angle data because seismic amplitudes at long offsets are relatively small. In most cases, you have to take nonlinearity into account only in the near field, close to the source. In the 1980s, however, we had some interesting experimental data acquired with several very powerful vibrators working simultaneously. Even when the vibrators were far apart, the interaction of their wavefields produced so-called “combination” frequencies that could not be explained by the conventional linear theory of elasticity. We realized that such nonlinear phenomena had the potential of providing information about the higher-order elastic coefficients which can be sensitive, for example, to the presence of hydrocarbons. In collaboration with Alex Litvin, my friend and classmate at Moscow State, I tried to build a framework for describing wavefields from point sources in stratified nonlinear-elastic media. It was an extremely challenging problem because most conventional tools of wave-propagation theory (Fourier transforms, the principle of superposition, etc.) break down.

**S:** *Both of you got awards from the SEG, the Virgil Kauffman Award for you Ilya and the Clarence Award for you Vladimir. What do you attribute these awards to – was it the tremendous amount of hard work you people did and still do, or does it have something to do with the training that you had before you came to the U.S., or the sharing of your ideas or what?*

**V:** I guess in my case it definitely was a good idea rather than hard work. It opened up many practical applications, where the measurement of azimuthally varying stacking velocities is critical. Fracture characterization is one of them. A small company, Axis Geophysical, has been formed in part to apply this technology and offer it as a service to oil companies.

In general, mathematical description of wave phenomena in anisotropic media is complicated. Therefore, it is important to cut through this complexity, identify a few (I would say the fewer, the better) quantities that control the signature or the measurement in question, and try to come up with its simplest possible description. I think the NMO ellipse is a good example. Only a circle is simpler but it is too simple even for isotropy.

**I:** I am sure that the solid training in math, physics, and geophysics that we got back in Russia was instrumental in making us successful. (Unfortunately, the level of higher education in Russia has been going down lately because of insufficient funding and the departure of many scientists to the West.)



Although I worked mostly on theoretical wave-propagation problems after receiving my Ph.D., the hands-on education in exploration geophysics at Moscow State helped me to quickly become productive when I joined Amoco in 1990. Also, I was fortunate to have been able to closely collaborate with such broadly knowledgeable and creative people as Leon Thomsen and Ken Larner and with a number of bright graduate students.

As to the Kauffman Award, it recognized the practical solution to the parameter-estimation problem for transversely isotropic (TI) media developed by my group at CSM. The approach that we have used with great success to solve that and many other anisotropic inverse problems is based on deriving simple weak-anisotropy approximations designed to separate the anisotropic terms and capture the key features of a certain operator or signature. Then we follow up with exact numerical analysis to verify our conclusions and generalize them for models with realistic levels of anisotropy.

**B:** *How do you assess the progress made by the industry in adopting anisotropic models?*

**V:** Doing new things is always challenging because you have to spend your time for an unknown outcome, and you do not want to fail too often. People know that and prefer doing more routine and less uncertain things. A new algorithm becomes conventional only after being tested many times on different data sets. It requires both time and expertise. So I would say that while there has been overall progress in implementing anisotropy over the last 10 or so years, this progress is slow.

**I:** We all know that there is quite a bit of inertia in the industry, and usually people are forced to try new things when they confront a problem that cannot be solved by conventional means. In the mid-1990s, when several companies began acquiring OBC surveys and processing high-quality multi-component data, it became obvious that PP and PS sections cannot be reconciled (i.e., tied in depth) without taking anisotropy into account. Also, the breakthroughs in parameter estimation for TI media showed seismic processors that there are practical ways to correct for anisotropy in imaging and, along the way, to estimate new parameters sensitive to lithology and fractures. I agree with Vladimir that the progress in accounting for anisotropy could have been faster but, for

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example, our colleagues from Exxon Mobil have recently told me that almost all 3D depth imaging in their company is done using VTI (TI with a vertical symmetry axis) migration codes.

**B:** *But in general imaging practice, like offshore Gulf of Mexico, there seems to be more emphasis on isotropic heterogeneity than anisotropy, I think?*

**I:** This emphasis is understandable given the level of structural complexity in subsalt exploration. Also, in some cases the influence of anisotropy on conventional-spread, narrow-azimuth P-wave data can be overlooked. For example, transverse isotropy in the Gulf often cancels the contribution of the vertical velocity gradient to the DMO operator, and constant-velocity isotropic DMO works just fine. Still, it has been shown that 3D migration in several areas of the Gulf can be substantially improved by accounting for anisotropy in sedimentary layers (especially in shales near the flanks of salt domes).

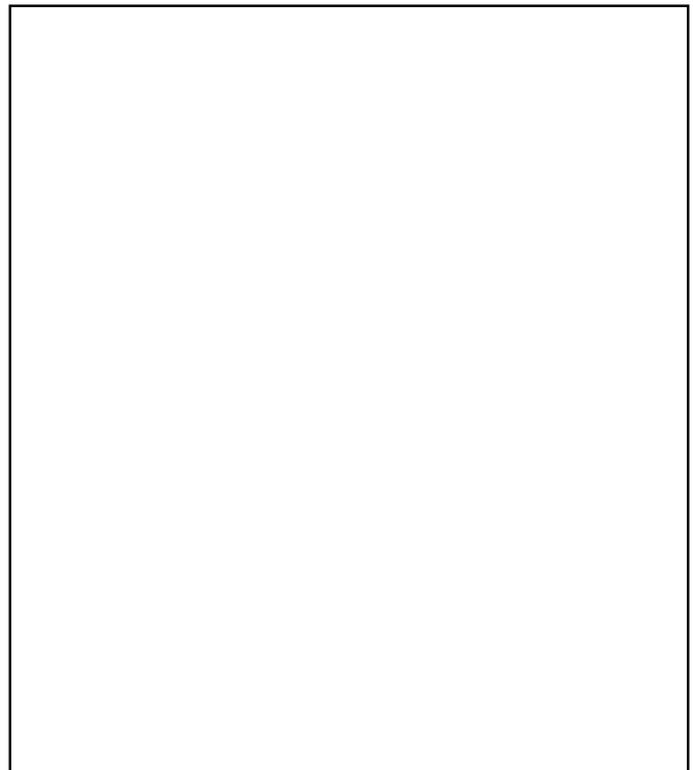
The situation is quite different in West Africa and in the North Sea, where many targets are overlaid by relatively thick anisotropic shale formations. In offshore Angola, for example, processors had to stretch offsets when applying conventional DMO algorithms, and still could not image fault planes at depth. Also, anisotropy-related misties in time-to-depth conversion are observed almost everywhere, and the most common way to correct for them is purely empirical. Finally, it is much more difficult to ignore anisotropy in prestack depth migration, where you cannot split your processing flow into several steps that can be tweaked separately.

**B:** *But initially we try the basic higher order NMO correction on flat lying VTI media and then maybe try to include some anisotropic term in ray bending migration to be more*

*rigorous. However as contractors equally suggest using either the more or less rigorous approach, means we may or may not obtain believable quantitative anisotropic information. Is believing the values for anisotropy sort of secondary, compared to what you are saying for correcting the image gathers' moveout for improved stacking?*

**I:** There are two different issues when we talk about nonhyperbolic moveout. One is flattening long-spread gathers, which can be accomplished by formally applying a nonhyperbolic moveout equation with the best-fit moveout parameters. Although these parameters are influenced by both anisotropy and heterogeneity (e.g., vertical-velocity gradients), we can just treat them as effective quantities needed for the moveout correction. Another issue is using the estimated moveout parameters in anisotropic velocity analysis. Then we have to apply Dix-type layer stripping to reflections from several interfaces in order to remove the contribution of vertical heterogeneity (in fact, it is often smaller than the contribution of anisotropy) to the nonhyperbolic moveout. For P-waves in VTI media, this procedure yields the interval anellipticity parameter  $\eta$  as a function of vertical time.

The anisotropic information obtained from nonhyperbolic moveout inversion or analysis of image gathers is physically realistic only if our model assumptions (e.g., about the medium symmetry) are correct. Image gathers for anisotropic models sometimes can be flattened using a laterally heterogeneous isotropic medium, but the required gradients usually do not make physical sense. Acquiring a wide range of source-receiver azimuths (and, in the best-case scenario, multicomponent data) can go a long way toward identifying the anisotropic symmetry and separating the influence of anisotropy and heterogeneity.



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**B:** *So if you can't compensate the moveout with just ray bending, you have to have an additional term, an anisotropic term, but what I meant to say was that for practical application you will attempt to flatten reflections to a certain extent, no matter what you assume, through inspection of the far offsets where there are lots of variations, like amplitude and frequency variations. So I tried the full anisotropic correction, to obtain the benefit of this flattened data through to a stack. However improvement in the result is not obvious as it's hard to justify stacking the furthest third of the full offset range, because the data changes so drastically, usually with abnormally strong amplitudes. This creates differences in the stack that are difficult to accept and especially so when one considers that the stack response is used by interpreters as a representation of a normal incidence P-wave, e.g. tie to normal incidence P-wave synthetics from logs or to zero-offset VSP's.*

**I:** Yes, making use of far offsets may be difficult. Still, whether or not we are able to stack or migrate the whole gather, velocity analysis of long-offset data is very important in anisotropic parameter estimation. Although the inversion of nonhyperbolic moveout is not always sufficiently stable, it provides us with an anisotropic model (actually, a family of models) that fits the kinematics of long-spread reflection events and, therefore, can be used in poststack time imaging. By applying AVO-sensitive semblance algorithms, we can mitigate the distortions in the moveout parameters caused by amplitude variations with offset and polarity reversals. Finally, I am wondering if these dramatic amplitude and frequency variations at far offsets can help us to better constrain medium parameters.

**B:** *Even if you get information on anisotropy or if you have better tools to flatten image gathers, the opportunity for more information by including very far offsets, even AVO, is not always realized. I think the benefit from anisotropy compensated moveout could be in obtaining meaningful normal-incidence P-wave velocity analyses for relative changes in interval velocity that might better predict over-pressure.*

**S:** *How did you both decide to come to the U.S.?*

**V:** It was easy for me because I came to the U.S. in 1994. It was a time of severe decline of the Russian economy. Essentially, people in research institutions and oil industry were not being paid for months. I had to make a choice between changing either my occupation or my country. I have chosen the latter. Now, more than 10 years down the road, I think it was the right choice.

**I:** I came to the U.S. in 1990, before the formal break-up of the Soviet Union. In the 1980s I had to work under the old Soviet system that was extremely inefficient but provided a measure of stability. We used to say that, “people pretend to work and the government pretends to pay,” which was basically the motto of the Soviet state before perestroika. (As Vladimir just mentioned, after perestroika the government sometimes stopped paying salaries altogether). Still, since scientists are primarily motivated by their curiosity, the Academy of Sciences could still produce some groundbreaking discoveries and maintain several high-profile scientific schools. One of the most serious problems was an outdated computing infrastructure; I was still using punch cards in the mid-1980s. So I could really sympathize with election officials in Florida in

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2000 when they struggled with hanging chads\*—that was an everyday routine for me. Another problem for many scientists including me was the inability to travel, even to socialist countries in Eastern Europe.

In the late 1980s things began opening up politically, but the economic situation was rapidly deteriorating. It became almost impossible to run serious scientific projects without help

from abroad. So in 1989 my wife and I decided to emigrate with our 3-year-old son, and at that time we could not do it without giving up our citizenship and even paying a hefty fee to the state. After leaving Russia, we spent more than six months in Austria and Italy as real refugees without valid travel documents waiting for visas to enter the U.S.

**B:** *That was all on your own, there was no sponsorship?*

**I:** We did have some support from charitable U.S. organizations, primarily HIAS (Hebrew Immigrant Aid Society). However, there were thousands of refugees from the Soviet Union living in Italy at that time, and the support structure was stretched thin. Also, neither my wife nor I had permits to work in Italy, which made our situation pretty difficult. After receiving U.S. visas, we arrived in Houston where we were hosted by the family of Azik Perelberg, a Soviet-born geophysicist who was then with Exxon Research. I was able to find a job with a small consulting company in Houston (primarily, translating Russian reports and papers), and then in the summer of 1990 I moved to the Amoco Research Center in Tulsa to work with Leon Thomsen. At that time, it was a good place to do applied research, and collaboration with Leon was one of the most satisfying experiences of my career.

**B:** *So how did you meet Leon Thomsen for the first time?*

**I:** I first met Leon in 1986 at the Second International Workshop on Seismic Anisotropy in Moscow. The Workshop was held about a month after the Chernobyl nuclear accident, and many participants from the West canceled their trips for fear of radiation. In fact, Moscow was clean, but western scientists had good reasons not to trust the Soviet government and its assurances. So it took courage for Leon, Stuart Crampin, Peter Leary, and several other anisotropists to come to the workshop, and I realized how special that group of people was. Two years later, Leon and I met again at the next anisotropy workshop, we had many good discussions and tried to stay in touch after that.

**S:** *What career accomplishments are you both proud of? What is one single accomplishment that stands out?*

**I:** First, I would mention my work on point-source radiation in nonlinear-elastic media that I've already talked about. Mathematically, it was probably the most difficult problem I have ever worked on (in one paper, we actually tried to treat

both nonlinearity and anisotropy), and obtaining a physically meaningful solution that was in agreement with experimental data was truly rewarding.

In terms of the impact on applied seismology, I would single out the parameter-estimation methodology for transversely isotropic media developed at CSM in the mid-1990s in cooperation with Tariq Alkhalifah, Ken Lerner, John Anderson, and others. Despite the general recognition that anisotropy often distorts processing results, building anisotropic velocity models had long been considered a hopeless exercise because even for the relatively simple VTI medium P-wave propagation depends on four stiffness coefficients that can vary in space. We were able to show that P-wave time processing in VTI media is controlled by just two parameters—the zero-dip NMO velocity and the anellipticity coefficient  $\eta$  ( $\eta$  is close to the difference between Thomsen's  $\epsilon$  and  $\delta$ ). Also, we developed several algorithms for estimating  $\eta$  directly from surface P-wave data.

Those results not only made VTI velocity analysis practical, they helped to turn around the overall attitude toward anisotropy among practicing geophysicists. Our group used these developments as a springboard to tackle parameter-estimation problems for much more complicated, azimuthally anisotropic models.

**V:** For me, it is what I got my Karcher award for: the NMO ellipses. This finding resulted in a number of remarkable conversations that I had with several different people. I would tell them, “You know, if you measure the stacking velocities in different azimuthal directions, and plot them as radius-vectors from a common origin, their ends should draw an ellipse in the horizontal plane.” They would reply, “No, it cannot be like that, it ought to be much more complicated.” Then I would begin explaining my reasons, and by the time I'd finished, they would say, “Oh, Vladimir, you are right. It's so obvious.”

**I:** When Vladimir presented this paper for the first time at a CWP consortium meeting, the audience was almost evenly divided: One half said that the result was wrong, and the other half said it was trivial!

**B:** *So where did you see this, in land data?*

**V:** No, there were no data at that time. The story was like this. I just joined the Center for Wave Phenomena at Colorado School of Mines, and, at the time, Ilya was working on these velocity issues for specific types of anisotropy. He had derived the NMO ellipse in a single, horizontal transversely isotropic layer with a horizontal symmetry axis and was doing the same for an orthorhombic layer. His derivations were lengthy and applicable only to those particular models. So I thought that the statement about elliptical azimuthal variation of the NMO velocity should be very general, and, therefore, one should be able to prove it without referring to any specific types of anisotropic symmetries or reflector geometries. This is what I did. Once you realize how you go about the whole issue, many other things become really simple and straightforward.

\* Chads are paper particles created when holes are made in a computer punched tape or punch card. Chads were made famous in the highly contentious 2000 presidential election in the United States, where a majority in the U.S. Electoral College was determined in Florida by the counting of punch card ballots. (<http://en.wikipedia.org/wiki/>)

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I: This elliptical variation has proved to be quite general for wide-azimuth data - it is valid not just for NMO velocities, but also for the small-offset geometrical spreading and, in many cases, the approximate AVO gradient.

**B:** *Then you saw this in field data?*

V: There are companies who make their business by extracting those NMO ellipses from the data. One of them is Axis Geophysical (now part of GX Technology), where Ed Jenner began doing this after he learned it from us when he was a student at Colorado School of Mines. Axis was so successful in estimating and interpreting the NMO ellipses that they got purchased by Input/Output.

Now, when you know the beginning of the story, let me tell you more. After joining Shell, I was involved in a fracture-characterization project. Axis happened to be the company that processed Shell's data, so we got their processing results. At some point, I was explaining to our geophysicists how the results were obtained, so I had to refer to some equations. They looked at my equations and were surprised. "Oh, you are using the same letter W for the NMO ellipses as Axis does. How do you know about their notation?" My response was "Well, this is my notation. I introduced it well before Axis even existed."

I: I recall that people from Delft did not like this notation when they saw it the first time because they used W's for matrices that have a completely different meaning. There are only so many letters in the alphabet, and in anisotropic notation we have nearly exhausted the Greek alphabet as well.



On a more serious note, the NMO ellipse is a good example of a development that not only provided a basis for anisotropic inversion, but also helped in data processing. It was difficult for service companies to overlook such a simple recipe for accurate stacking of wide-azimuth data.

V: It is always good when somebody presents a simple and straightforward solution to a seemingly complicated practical problem. Once people go through the logic that led to the solution, it becomes clear to everybody that the solution is correct and has a wide area of applicability.

I: Since complicated concepts like anisotropy take a lot of time and effort to absorb, it is not enough just to say, "We have an

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algorithm that is much more involved than the one you are currently using, but if you apply it properly, the result will be a lot better.” You need to show a marked improvement using tools that do not scare people away, and that is what I believe was achieved with both VTI processing and the NMO ellipse. After the success of our VTI algorithms, people have become a lot more receptive to the idea of applying anisotropic processing to lower-symmetry models

(like orthorhombic), which seemed to be completely out of reach a decade ago.

**B:** So you think that with contractors applying algorithms and interpreters using new and improved data attributes, we are getting closer to grasping what the equations mean?

**I:** Yesterday morning we came to the Geo-X building to teach our course and the first thing we saw after coming out of the elevator was a poster with our nonhyperbolic moveout equation for VTI media. Apparently, the company wants to market its ability to apply that equation and, in general, to handle transverse isotropy. Several velocity-analysis and migration algorithms for VTI models have become almost standard, especially  $\eta$  estimation from nonhyperbolic moveout. Also, misties in time-to-depth conversion are often interpreted using the parameter  $\delta$ . Azimuthal anisotropy is a different story, but there are many examples of successful application of P-wave azimuthal AVO analysis by Veritas and some other companies.

Another good sign is that I have been getting a lot of inquiries from industry people about various anisotropic algorithms described in my book that was published by Elsevier in 2001. To the surprise of the publisher, the book sold out last year, and they have just printed the second edition. Also, the two-day anisotropy course that Vladimir and I were teaching here in Calgary really took off this year. We taught it at five locations including Brazil and China, and most attendees were from service and oil companies.

**B:** Do you think people really understand what the parameter  $\delta$  is? I think most accept and understand an explanation as a parameter involved in the near vertical angle range, impacting P- and SV-waves. But do geophysicists really understand  $\delta$  from any sense of the physics of wave propagation or  $C_{ijkl}$ 's describing TI symmetry?

**I:** I believe most geophysicists involved in processing understand that  $\delta$  is responsible for the difference between the P-wave stacking and vertical velocities. Unfortunately,  $\delta$  was originally labeled a “strange” anisotropy parameter, which caused a bit of confusion. Now we realize that  $\delta$  is simply a scaled second derivative of the P-wave phase-velocity function in the vertical (symmetry) direction and, therefore, controls near-vertical P-wave velocity variations.

**B:** So does this mean the average geophysicist's physical understanding for  $\delta$  is that its biggest impact is at 45 degrees

due to propagation geometry from the separate incidence angle term as sine squared cosine squared? We all see this parameterization, but one wonders what it really means. By contrast,  $\epsilon$  is easy to understand physically, being measured along principal axes i.e. velocities and  $C_{ijkl}$ 's are horizontal and vertical. In a similar way,  $\gamma$  is easy because it's to do with shear wave vibration perpendicular or parallel to layering or fractures. These parameters are conceptually simple and most geophysicists can understand them, but gaining some physical insight about intermediate propagation angles is more difficult.

**I:** It is tempting to describe anisotropy in terms of the vertical and horizontal velocity but, unfortunately, this approach works only for elliptical models. To get an accurate description of P-wave velocity at intermediate angles, you need one more parameter, which can be  $\delta$  or  $\eta$ . Although in lab experiments  $\delta$  is indeed often estimated from the velocity measured at 45 degrees to the symmetry axis, this type of definition does not have much relevance for reflection data processing. It is more important in practice that  $\delta$  controls the VTI term in both the NMO velocity and AVO gradient of P-waves. In short,  $\delta$  is so useful because it determines the initial variation of the P-wave phase-velocity function near the symmetry axis.

**V:** It is interesting. I had a discussion with Leon about his notation a couple of years ago. He said that now he would do everything in terms of  $\delta$  and  $\eta$  rather than  $\delta$  and  $\epsilon$ .

**I:** That would definitely make sense for reflection seismic because  $\eta$  is responsible for P-wave time processing, while  $\delta$  controls time-to-depth conversion. However, 20 years ago it was impossible to foresee that P-wave time imaging is governed by the difference between  $\epsilon$  and  $\delta$ . Also, as Bill just said,  $\epsilon$  is the most natural measure of P-wave anisotropy; too bad that it does not directly influence reflection data!

**S:** What are some of the other interesting ideas that you both are working on now?

**V:** I have two projects that occupy most of my time. One is seismic characterization of fractures and the other is anisotropic parameter estimation from VSP data. Both projects are “anisotropic” by their nature. As to the fracture characterization, well, we simply have to rely on azimuthal anisotropy. So we are talking about wide-azimuth acquisition geometries that are needed to measure azimuthally varying seismic signatures of P- and possibly shear-waves. Fracture characterization is a potentially important area of exploration geophysics because, by some estimates, about one third of world's hydrocarbon reserves is trapped in fractured formations.

Anisotropy estimation from VSP data also has its history. I believe Ed White was the first who did it back in 1983, a long time ago. Multiazimuth, walkaway VSP is about the only technique that gives a fairly accurate estimate of local, in-situ anisotropy. If we have good polar and azimuthal coverage in both P- and shear-wave data, we can relax all assumptions about the symmetry and use the most general, triclinic anisotropy described by 21 stiffness coefficients. Then the symmetry can be derived from the obtained estimates as opposed to being assumed a priori. And this has actually been

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done. Next, we can think about relating the estimated parameters to fractures, stresses or other physical or structural features that cause anisotropy. It would be great if we could do it reliably. I believe we are slowly getting there.

**B:** *So, do you think that azimuthal P-wave variation in NMO or AVO is related primarily to stress or fractures?*

**V:** I think both are related to each other. Let us apply some stress to a piece of fractured rock. The fractures will be primarily affected because the stress mainly influences the most compliant part of the rock by opening or closing the cracks. In this sense, I would not separate these two things.

**B:** *So if you see areas of high azimuthal anisotropy, does that imply more or less compliance?*

**V:** It depends. There are highly compliant isotropic rocks.

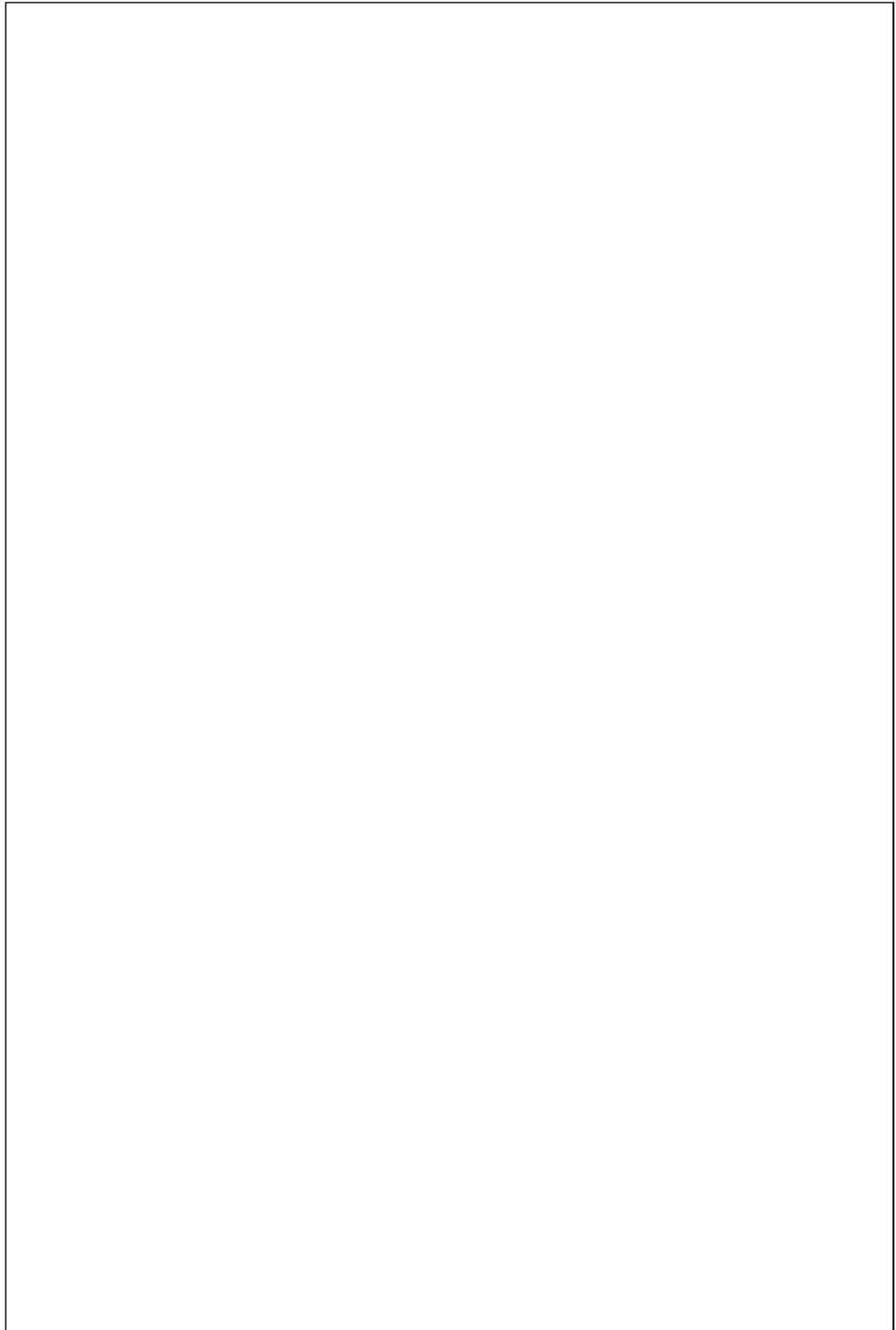
**B:** *Would an area be equally affected by high stress or lots of fractures? So if you saw significant anisotropy assumed to be due to increase in fractures, might this not have reduced the stresses, as open fractures result from a failure to resist stress?*

**V:** Well, if you have both a low velocity and stronger azimuthal anisotropy, you might make a conjecture about the fractures. In fact, it is not that straightforward. For instance, randomly oriented fractures, cause overall isotropy. We are lucky that this normally does not happen in the earth because the fractures are oriented nearly vertically.

**B:** *That does not imply open fractures necessarily; usually the single fracture set is the simplest model we can describe or impose with tractable equations.*

**V:** Yes, what you are saying is more or less applicable to a combined influence of stresses and cracks, in which case the fractures are aligned with the minimum stress direction. Again, one has to be careful because the minimum stress direction always exists and yet multiple fracture sets are typically observed. You need different types of data to unravel those.

**I:** Let me say a few words about the research done by my group, the A(nisotropy)-Team. For the past few years, our main focus has been on inversion and processing of multicomponent, multiazimuth reflection data and fracture characterization. Also, we are trying not to forget about more conventional, narrow-azimuth P-wave surveys because they still represent the bulk of exploration data. The main problem with P-wave depth imaging for VTI media is that reflection traveltimes alone do not constrain the vertical velocity and the parameter  $\delta$ . With my former student Debashish Sarkar (he is now with

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GX Technology), we found an interesting way of building reasonably accurate heterogeneous VTI velocity fields by dividing the model into factorized blocks or layers (factorized means that all anisotropic parameters in each block are constant) and applying migration velocity analysis (MVA). Since the velocity gradients in factorized media can be estimated from P-wave moveout, our MVA algorithm can reconstruct a

continuous vertical-velocity function, as well as  $\delta$  and  $\epsilon$ , using only the velocity at the top of the model.

Parameter estimation for azimuthally anisotropic media, however, typically requires multicomponent data. Recording both P- and S-waves with good coverage in azimuth and offset gives us a realistic chance to look “at the anisotropy” and resolve physically meaningful medium parameters. In addition to velocity (moveout) and AVO analysis, we have begun working on anisotropic attenuation coefficients. To make this problem treatable, we have recently introduced a set of attenuation-anisotropy parameters similar to  $\epsilon$ ,  $\delta$  and  $\gamma$ . It turns out that there are many analogies between anisotropic velocity and attenuation functions, so we can benefit from our experience with kinematics. We all know how difficult it is to make reliable attenuation measurements, separate intrinsic attenuation from scattering etc., but there is strong evidence that the attenuation coefficient may be the most sensitive indicator of anisotropy.

**B:** *Are you using converted-wave data or pure shear data or both?*

**I:** Unfortunately, shear-wave sources are seldom used these days, so we have little choice. PS-waves are generally regarded as a poor substitute for SS-waves in anisotropic velocity analysis because of the limited range of reflection shear-wave angles after the conversion. For example, the combination of PP and PS data cannot be inverted for the Thomsen parameters even in a single horizontal VTI layer. In contrast, these parameters can be determined if we combine PP-waves with wide-angle SS reflections.

But there is an interesting twist here. The moveout of PS-waves can become asymmetric (e.g., due to the tilt of the symmetry axis in TI media), so that PS traveltimes does not stay the same when the source and receiver are interchanged. Vladimir and I used to treat this asymmetry as a nuisance and developed a method (it is often called “PP+PS=SS”) to generate symmetric SS data from PP- and PS-waves. However, it turns out that the moveout asymmetry can provide critically important information about anisotropy that cannot be extracted from the pure modes. So in velocity analysis for tilted TI media we recommend combining PP data with mode-converted waves. Also, we have made good progress in the joint amplitude inversion of PP and PS reflection data.

**B:** *Has the difficulty in resolving P-wave AVO and combining it with converted-wave AVO resulted in both having gone out of favor?*

**I:** No, for many fractured reservoirs that are too thin to be resolved by moveout-inversion methods, azimuthal AVO analysis is the preferred fracture-detection tool. Extracting the reflection coefficient from surface data is not easy because recorded amplitudes are always distorted by the influence of the overburden, especially if there is anisotropy above the reflector (which is almost always the case!). One of my group’s current projects is aimed at computing geometrical spreading from reflection traveltimes, which allows us to implement the spreading correction for layered media without knowledge of the velocity model.

**B:** *Anisotropic geometrical spreading?*

**I:** Yes, if the medium above the reflector is azimuthally anisotropic, geometrical spreading can completely corrupt the azimuthal AVO signature. By fitting a nonhyperbolic moveout equation to long-offset, wide-azimuth data, we can accurately compute the azimuthally varying geometrical-spreading factor for pretty complicated models composed of orthorhombic layers.

**S:** *I wanted to ask you about Rüger’s approximation that you used, the output from that is usually given in terms of fracture density and the fracture orientation. Now the question is, how much more information does this give us? Suppose I run a coherence volume on the all-azimuth seismic data volume; so on the coherence displays I should be able to see the fracture orientation and the main faults. How much more information does the azimuthal AVO give us?*

**I:** This is a very good question. I believe sometimes people are trying to extract too much information from AVO analysis. For the HTI model, the difference between the P-wave AVO gradients in the two vertical symmetry planes depends on the shear-wave splitting coefficient (essentially, on the crack density) and a  $\delta$ -type parameter (for simplicity, let us just call it  $\delta$ ). These two parameters are generally independent, and they cannot be estimated from the AVO response without additional information. In particular, values of  $\delta$  are completely different for fluid-filled and dry cracks. So while the fracture azimuth is well-constrained by the azimuthal AVO response, in order to estimate the crack density we need to combine P-wave AVO with traveltimes information (if the reservoir is sufficiently thick) or shear (PS or SS) amplitudes. Obviously, AVO inversion does not get any easier for the more realistic orthorhombic model.

**S:** *The reason I asked this question is that, I have not seen significant information convincingly coming out of these outputs; so I am just curious to see at least one case study that shows such a comparison, which I expect.*

**V:** I guess you are talking about a Veritas type of approach. An implicit assumption there, as well as in Rüger’s equation is that you have just a single vertical fracture set. If your assumption of a single set is violated, the estimates of the crack density and orientation you got under this assumption will be incorrect.

**B:** *But do you think that crack density maps, I am not so sure about crack orientation, would still relate to, say, the*

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*absolute amount of multiple fracture sets? By this I do not imply a correct model for interpretation of azimuthal variation and so fracture density; when I say density I just mean one measurement like azimuthal NMO variation. In my experience, by having one measurement of the degree of ellipticity that you are trying to relate to, say isotropic NMO or spatial coherence discontinuities, the resulting maps appear to be actually unrelated. So what is the relationship between the elongation of the NMO ellipse and crack density?*

I: In general, the elongation of the NMO ellipse may be influenced not just by azimuthal anisotropy, but also by reflector dip and lateral heterogeneity. However, even if we assume that the azimuthal variation of NMO velocity is entirely due to a single set of fractures, the ratio of the symmetry-plane NMO velocities yields the  $\delta$  parameter that I just talked about. This parameter increases with crack density, but it also depends on crack saturation and on the P-to-S velocity ratio in the background. For the same crack density, the NMO ellipse is much more elongated when the cracks are dry. Therefore, spatial variations of the NMO ellipticity over a field may be due to a change in the fluid content of the fracture network rather than in the crack density. That's why it is so important to use multicomponent data and combine different signatures in fracture detection.

B: *In one case where I have seen a big change in the azimuthal P-wave AVO, in an area of open water filled fractures, the problem is that you see this azimuthal variation through the whole section at all reflectors and not just at the fractured horizon. When you see amplitude variations going through the whole section then you tend to look at the near surface as the culprit and wonder what impact that has on amplitudes. Conversely, in a spatial sense, through azimuthal AVO and azimuthal NMO analysis, I have also seen discontinuities in crack density or orientation from layer to layer and yet we might assume this to be false in a consistent stress field direction? How does this all come together physically for say a description of a model that engineers would believe and use in various types of simulations?*

I: Definitely, if the AVO response looks similar for several reflectors, the azimuthal amplitude variation is likely caused by the near surface. That's where the moveout-based geometrical-spreading correction that I talked about may prove very helpful. Discontinuities in the shear-wave splitting parameter and principal anisotropy directions across layer boundaries have indeed been observed in many areas. Sometimes there is a 90-degree change in the fast anisotropy direction inside a formation with two orthogonal fracture sets; it seems that each set dominates over a certain depth interval. We know how to handle such discontinuities in processing by applying layer stripping, but I am not sure if there is a convincing physical explanation for them in terms of the stress field.

As to your last question, we tend to assume very simple models in fracture characterization, and often try to get away with horizontal transverse isotropy. Unfortunately, HTI is too simplistic for many reservoirs since it describes a single system of penny-shaped cracks embedded in purely isotropic

host rock. Since we still have a long way to go in our understanding of the seismic response of realistic fracture systems, my preference is to process seismic data using a certain effective symmetry for the reservoir rather than a specific fracture model. In the ideal case, we can be completely general and operate with 21 stiffnesses (that's a triclinic medium), which is sometimes feasible in VSP inversion. More typically, we assume an orthorhombic or monoclinic medium and verify whether either model fits our data. For example, in several case studies (e.g., Weyburn field) shear-wave polarizations at vertical incidence were found to be misaligned with the axes of the P-wave NMO ellipse, which means that the symmetry should be more complex than HTI or orthorhombic. After estimating the Thomsen-style parameters or effective stiffnesses from seismic data, we can try to interpret the inversion results (maybe in cooperation with engineers!) using different fracture models.

B: *So for my understanding, if you have converted wave or pure shear data giving a measurement of the fracture orientation or density, let us say the anisotropic parameter  $\gamma$ , then is this considered a better or more correct value for  $\gamma$  than from P-P azimuthal AVO or NMO? How different would this  $\gamma$  be from the one estimated by Rüger's azimuthal AVO equation? I would tend to assume that it is primarily  $\gamma$  or a  $\gamma$ -type parameter that one might expect or want from P-P azimuthal AVO analysis. Has anybody done this direct comparison? For instance, the Reservoir Characterization Project (RCP) at CSM should have numerous datasets with P-P along with equivalent P-S and S-S in various azimuths and polarizations. This type of corroboration would certainly help us all understand the value, especially for P-P, of any seismic-based azimuthal information.*

I: The splitting parameter  $\gamma$  that we can get from SS- or PS-waves is exactly the same coefficient that we have in the P-wave AVO gradient for HTI media (the P-wave NMO ellipse does not depend on  $\gamma$ ). Therefore, combining P-wave azimuthal AVO with shear data would definitely help in quantitative fracture characterization. If  $\gamma$  is estimated from shear waves, the azimuthal variation of the P-wave AVO gradient can be used to find  $\delta$  and make some inferences about fracture infill. Ignoring  $\delta$  in the P-wave AVO gradient would not be a good idea because its contribution may be comparable to that of  $\gamma$ .

However, it is difficult to extract  $\gamma$  from P-wave data alone, so RCP has not yet made the direct comparison you are talking about. In principle,  $\gamma$  can be estimated by combining the P-wave AVO gradient and NMO ellipse. However, for realistic models with multiple layers and vertical velocity gradients, we have to be concerned about the difference in vertical resolution between traveltime and amplitude methods. For that reason, when we combine the P-wave AVO gradient with shear data, it is better to estimate the local value of  $\gamma$  for a certain boundary from the difference between the reflection coefficients of the split S-waves.

B: *So a measurement of the splitting  $\gamma$  in terms of S-wave reflection coefficient is related to the azimuthal P-P AVO gradient and there must be datasets that could show this relationship on the same reflection boundary. And as I*

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*mentioned this would help understand the azimuthal AVO gradient, whether or not the P and S-wave measurements do indeed measure the same anisotropy.*

I: Yes, we definitely need case studies of this type. As we just discussed, even for HTI media we cannot expect to get “the same anisotropy” from the P-wave AVO gradient (which includes  $\delta$ ) and shear data, but it would be interesting to see such a comparison. Another problem here is that the HTI model just does not seem to be adequate for many fractured formations. If the medium is orthorhombic, the azimuthal variation of the P-wave AVO gradient includes the difference between the two symmetry-plane  $\delta$  parameters. Although we can remove  $\gamma$  from the gradient using shear data, this difference is more difficult to interpret than the single  $\delta$  parameter for HTI media.

B: *I agree, but you might be able to find some relationship between the  $\gamma$  and  $\delta$  parameters contained in the equations for reflection coefficients.*

I: Such a relationship has to be based on a certain physical model for fractures because  $\delta$  is significantly different for dry and wet cracks. In the absence of reliable information about fractures, my preference is to treat these parameters as independent quantities.

B: *So you need another set of equations or relationships that are more directly related to  $\gamma$ .*

V: No, I think what is going to help is interjecting certain rock-physics understanding into all those processes. Of course, it is very important to make as many measurements as you can because they illuminate the same piece of the subsurface differently. In addition, however, if you make a justifiable assumption, for instance, that anisotropy is due to fractures, then you are already making a lot of progress because you are no longer dealing with, say, generic orthorhombic media. Now, you are concentrating on a special case of orthotropy that has fewer parameters to worry about. So, essentially the rock physics provides you with additional relationships that are helping you at the end.

B: *There is some simplification that might be known. For instance, engineers would tend to say, “Well, when we stimulate a fracture, it tends to be simple,” as in the fracture direction and height as measured by microseismic techniques. The fractures may not be consistent by varying from location to location, but there maybe some ground truth in this engineering reality, suggesting a single set of oriented fractures or a stress direction. Maybe that’s all the complexity there is and our measurements are too ambiguous to show this simplicity?*

V: In the mathematical sense, you want to add more equations that constrain the unknowns, so you use different types of seismic data. By doing this, you approach the problem from one end. You might also want to approach it from another end and remove some of the unknowns. This could be done by using rock physics, as it tells you that certain quantities are known or related to each other because you are dealing with a certain type of rock.

B: *That makes sense.*

V: Yes, the important things are the number of unknowns and the sensitivity of your measurements to them.

I: It is particularly promising to use VSP surveys in making in-situ anisotropic parameter estimation. As Vladimir already mentioned, multicomponent, multiazimuth VSP data provide a wealth of information that can be inverted for the local values of all 21 stiffness coefficients. Two years ago Vladimir and our student Pawan Dewangan published a paper where they applied this inversion to field data from the Vacuum Field in New Mexico and found (rather than assumed) the medium to be close to tilted orthorhombic. Unfortunately, velocity analysis of reflection data is a lot more ambiguous because we do not have local estimates of either slowness or polarization. Despite the high cost of borehole data, I would like to emphasize how important properly designed VSP’s are in constraining anisotropic reservoir models and improving our understanding of subsurface symmetries.

B: *That’s already a level beyond HTI and VTI. Are such orthorhombic models common in the algorithms provided by contractors?*

I: No, but we believe that orthorhombic models are common for subsurface rocks. I think we should be a few steps ahead of contractors and give them strong incentives to start looking beyond transverse isotropy. For example, we have been able to find practical ways of handling orthorhombic media by exploiting analogies between orthorhombic and TI symmetries. In fact, kinematic processing of narrow-azimuth P-wave data for orthorhombic models often can be handled by VTI algorithms. Also, it turns out that the jump from HTI to the more realistic orthorhombic medium is not that dramatic because both models have two vertical symmetry planes where velocities and polarizations are described by VTI equations. In particular, P-wave AVO approximations in HTI and orthorhombic media have almost the same form. The transition between TI and orthorhombic symmetries becomes really transparent if we use the Thomsen-style notation discussed in detail in my book. Of course, I do not mean that inversion for orthorhombic media is easy, but at least we know what parameter combinations to look for.

B: *For HTI we leave the fractures in an isotropic background matrix, which may not always be a good assumption.*

I: Yes, the background rock in fractured formations is often anisotropic, either due to some intrinsic structure on a fine scale (as in shales) or to thin layering. This background anisotropy immediately lowers the symmetry from HTI to at least orthorhombic. According to Schoenberg’s linear-slip theory, we can simply add the background and fracture compliances (even for multiple fracture systems) to find the compliance matrix of the effective medium. The simplicity of this theory is very attractive, but we have to be careful when applying it, for instance, to fractures in porous, saturated rocks.

V: I guess many people do not realize that this linear approach may not be accurate for fractures in anisotropic background. What Ilya is talking about is an approximation valid when the

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anisotropy of the background is weak in some sense. A general solution to this problem for a strongly anisotropic background is unknown.

**S:** *We have had enough of technical talk. I wanted to ask you about your other interests; do you get time to pursue them?*

**V:** I have another passion. I climb mountains but they are difficult to find in Houston.

**S:** *It must have been easy in Colorado?*

**I:** After Vladimir had climbed all challenging peaks in Colorado, he decided it was time to move on...

**V:** Yes, climbing was up and down in my life. I began climbing when I was in high school. It slowed down a few years ago. Now it seems to be picking up again. As there is nothing to climb around Houston, I am going to South America. I am still enjoying it.

**I:** My wife is a pianist and music teacher, and both of us like classical music. We often go to the symphony, although I wish we could listen to some orchestras we had enjoyed back in Moscow. Also, I am staying busy with my sons' activities, primarily ice hockey. On weekends, you can often find me at the local rink early in the morning (something most people in Canada are used to!) coaching youth hockey teams or sometimes playing myself.

**S:** *Are your kids interested in geophysics?*

**I:** My older son is now at the University of Colorado where he majors in biology and prepares to go to medical school. When I ask him about geophysics, he tells me that I seem to work hard but (judging by the titles of my papers) do not produce anything that “normal people” can relate to. Careers in geophysics and sciences in general are a tough sell in the U.S. where the pay scale is such that many bright kids elect to go to business, law or medicine. My younger son (he is 10), however, shows a lot of promise because he likes math (more than I did

in his age) and already asks questions about the earth's interior.

**S:** *So what would be your message for young people who are planning to pick up a career in geophysics?*

**I:** First of all, they have to be prepared to face some exciting but very complicated problems. In most cases, we do not have enough data to resolve the subsurface properties in a robust way, and many inverse problems in geophysics are ill-posed. So it is difficult to be a perfectionist when doing geophysics; your reward may come from partial solutions, and you should be able to enjoy the very process of searching for the answer. When working with students, I have realized how important it is to turn any unsuccessful project into a learning experience that can prove indispensable in the future.

Also, I would encourage young geophysicists not to feel constrained by available data or existing methodologies. I believe that our approach to subsurface characterization will continue a fundamental transformation as we move from acoustic to isotropic elastic to fully anisotropic models. I would love to see this new generation not just doing solid work within established paradigms, but also striving to be ahead of the curve by pushing forward this transformation process.

**V:** My advice is to try to do the most technically advanced work that you can possibly do. All existing developments obviously grew from simpler and less technical things. If current students can make highly technical work their main occupation, chances are they will be in business because the industry moves to more and more challenging and complicated areas. Clearly, students need a solid, I would say working knowledge of mathematics, physics, perhaps chemistry, and some engineering. I do not envision the end of geophysics era any time soon, so be as technically advanced as you can.

**S:** *Well, thank you Vladimir and Ilya for giving us this opportunity of sitting down together. We enjoyed talking to you. R*