Signals

Dear Editors:

Reciprocity violated? One of the first accounts of acoustic reciprocity can be found in the 1878 book *The Theory of Sound* by Lord Rayleigh. The reciprocity principle relates two possible acoustic field states which share the same time-invariant domain. The most known consequence of this principle is the so-called physical reciprocity: The acoustic response remains the same when the source and receiver are interchanged. The reciprocity principle has been formulated by Betti for elastostatic fields. The general form is named the Betti-Rayleigh reciprocity theorem. In electromagnetic wave theory, reciprocity was introduced by Lorentz in 1896.

Through the years, reciprocity has proven to be a powerful principle. It has not only served as a tool for testing numerical modeling schemes, but it has found many applications in forward and inverse scattering problems; see for example the book *Seismic Applications of Acoustic Reciprocity* by Fokkema and van den Berg (Elsevier, 1993).

The reciprocity principle has to be applied with care. For example, in flowing media, reciprocity seems to be violated. However, this can be repaired by reversing the direction of flow between the two experiments. Another violation of reciprocity occurs in solutions of the one-way wave equations, which are normally used in seismic migration. Here the remedy consists of a proper normalization of the down- and upgoing wave fields: So-called flux-normalized one-way wave fields do obey reciprocity, unlike pressure-normalized one-way wave fields. Finally, note that solutions of nonlinear wave equations do not obey reciprocity. A general recipe for extending the reciprocity principle for nonlinear media cannot be given.

In the July 2001 *TLE*, Muerdter, Kelly, and Ratcliff discuss the nonreciprocity of amplitudes of subsalt responses. From ray-tracing experiments they observed that amplitudes from subsalt reflectors depend on the direction of shooting. To investigate this phenomenon, they defined a simple model with a salt body, in which they computed the product of the plane wave transmission coefficients along one and the same ray in two directions (Figures 5 and 6 in their paper). The transmission product for "downdip shooting was $T_{down} = 0.519$, whereas for "updip shooting" it amounted to $T_{up} = 0.620$. The same ratio of amplitudes (i.e., 0.519/0.620 = 0.837) was obtained by ray tracing in both directions. Since both ratios are the same but not equal to one, the authors concluded that reciprocity was violated. This was attributed to the dipping, high-impedance interfaces of the salt body.

Is reciprocity really breaking down for this situation? Ray tracing is based on the linear elastodynamic full wave equation in a nonmoving medium, so the precautions noted above do not apply. Consequently, reciprocity should be fulfilled (here at least in the high-frequency approximation for which ray tracing is valid). The crux is that the "independent" test with products of plane-wave transmission coefficients in two directions is deficient, since reciprocity applies to point sources and receivers (or line sources and point receivers in a 2-D inhomogeneous medium). The authors defend this test by stating that terms other than the transmission coefficients (such as spherical spreading) are the same in both directions and therefore cancel when the ratio of ray-tracing amplitudes is computed. Correct expressions for ray-tracing amplitudes, however, contain direction cosines and geometrical spreading factors which are *not* the same in both directions. The spreading factors can be found, for example, in Shah (GEOPHYSICS, 1973). The proper combination of transmission coefficients, direction cosines, and geometrical spreading factors leads to ray-tracing amplitudes which do obey reciprocity. Hence, the fact that the authors obtained the same ratio of ray-tracing amplitudes as of transmission products ($T_{down}/T_{up} = 0.837$) merely shows that their ray-tracing amplitudes are in error.

Let us now analyze the responses of the configuration with the salt body in more detail. Given the fact that reciprocity holds, we equate the proper expressions for the ray-tracing amplitudes for downdip and updip shooting and take out all terms that are the same at both sides of the equation. We thus obtain

$$\frac{T_{down}}{T_{up}} = \frac{\cos(\alpha_1) * \cos(\alpha_2) * \cos(\alpha_3) * \cos(\alpha_4)}{\cos(\beta_1) * \cos(\beta_2) * \cos(\beta_3) * \cos(\beta_4)}$$

with T_{down} and T_{up} defined above. The angles α_1, α_2, α_3, and α_4 are the incident angles, whereas β_1, β_2, β_3, and β_4 are the refraction angles, both measured along the ray for downdip shooting. To check this equation, we substitute the values given in Table 3 by the authors ($\alpha_1 = 21.78°$, $\beta_1 = 36.41°$, $\alpha_2 = 16.41°$, $\beta_2 = 31.59°$, $\alpha_3 = 51.59°$, $\beta_3 = 25.00°$, $\alpha_4 = 25.00°$, $\beta_4 = 15.32°$). For the right-hand side, we obtain 0.837, which is indeed equal to the value obtained for T_{down}/T_{up}. Hence, the fact that T_{down}/T_{up} is not equal to one is not a violation of reciprocity but is in perfect agreement with it. This simple computation confirms that reciprocity is obeyed, even for reflections below dipping, high-impedance interfaces of salt bodies.

Reciprocity has withstood its first attack of the 21st century.

—*KEES WAPENAAR and JACOB FOKKEMA*
Delft, The Netherlands

Dear Editors:

In the July 2001 *TLE*, Muerdter, Kelly, and Ratcliff assert that the reciprocity principle has been violated.

What is wrong is their imputation that it is the physics that causes the failure and this is clearly codswallop. I make the following points:

1) Reciprocity is intrinsic to the theory of linear elasticity and holds independent of heterogeneity, etc.
2) It is well known that the practical conditions under which field operations are conducted will often ensure that reciprocity will fail in a dynamic sense, even though it often holds very well kinematically.
3) Ray theory is itself an approximation to general elastic theory and it is being increasingly used to model situations where it is inappropriate because of the scale and complexity of the problem under discussion. However, that said, there is no excuse for a computational ray-tracing program not honoring reciprocity. It may screw up amplitude and phase, but:
4) Production computational ray tracing can and should honor reciprocity.

As a final word, I will add that I have no problems with *TLE* not enjoying a peer-review process, but it does mean that the authors should accept postpublication criticism and expect to get it. Also, the editor must take some responsibility for this unfortunate paper, particularly since the failure of reciprocity in the title should have raised a red flag.

—FRANCIS MUIR
Stanford University

Dear Editors:

The paper in the July 2001 *TLE* by Muerdter et al. got me thinking about the ambiguous status of our profession. Is "exploration geophysics" a craft, an engineering discipline, or a science? It's all of those, I think, which is perhaps its greatest attraction. We try to use as much rigor as we can, or as much as we can afford, but recognize that our efforts will often fall far short of the standards of hard science.

Unfortunately, in the urgency of day-to-day work, it is all too easy to delude ourselves that our "seat-of-the-pants numerical engineering" is really "rigorous math." For example, one often sees the assertion that two experiments are "reciprocal." Sometimes the term is used with precision, more often not.

Inconsistent use of the term naturally leads to confusion. Some invoke reciprocity whenever they wish to swap sources and receivers, whether it applies to their particular situation or not. Others assert that reciprocity is "only an approximation," and never should be satisfied exactly, even for a synthetic (the error Muerdter et al. unfortunately fell into).

In reality, reciprocity is an exact statement of mathematics, but one that may or may not be useful in any particular situation. As an exact mathematical symmetry of the elastic wave equation, it is particularly suitable for testing our synthetic modeling codes, which are also idealized mathematical constructions. If a modeling code ought to honor reciprocity (to numerical precision) but does not, then that's a bug, *not* "engineering."

Unfortunately, in these boom times there is tremendous pressure to blur the distinctions among the "craft," "engineering," and "hard science" aspects of our profession in the name of "getting the job done." Achieving rigor takes time, and time costs money. So we see ad-hoc algorithms hurriedly dressed up in the language of formal mathematics and presented as hard science, as if the mere use of the "sacred symbols" will magically imbue the results with preternatural accuracy.

Sorry, that's not the way the universe works, and we as a profession would do very well to remember that! Otherwise, we risk demonstrating the geophysical incarnation of Gresham's law ("if exchanged at parity, untrustworthy money will drive sound money out of circulation").

—JOE DELLINGER
BP Upstream Technology Group
Houston, Texas

Dear Editors,

D. Muerdter, M. Kelly, and D. Ratcliff claim that reciprocity does not hold in their Acquisition/Processing contribution entitled: "Understanding subsalt illumination through ray-trace modeling, Part 2: Dipping salt bodies, salt peaks, and nonreciprocity of subsalt amplitude response" (*TLE*, July 2001). I'm surprised the authors did not realize that reciprocity, if done correctly, must hold for the case they treated. One of the more useful aspects of the reciprocity principle is that it allows us to test whether our computations, algorithms, and thinking are correct by comparing responses under reciprocal conditions.

It has been demonstrated that reciprocity holds for heterogeneous, anisotropic, elastic media (by Betti in 1872 for static displacements and forces in elastic media, by Rayleigh in 1877 for time-harmonic fields, by Graffi in 1939 and 1946 for transient fields in isotropic media, by Knopoff and Gangi in 1959 for transient fields in heterogeneous and anisotropic elastic media). It has been shown that reciprocity holds in anelastic (viscoelastic), heterogeneous and anisotropic media by de Hoop in 1966 and by Gangi in 2000 (including the generalization of the inertial force to one that includes dependence on particle position and velocity). Dahlen and Tromp (1998) and Gangi (2000) show that reciprocity will hold in a rotating earth as well *if* the direction of rotation is reversed for the reciprocal case.

Therefore, the lack of equality for their reciprocal cases should have alerted the authors that something was wrong with either their calculations or their interpretation of their results.

I am disappointed that the editors of *TLE* did not require some additional checking of a claim that is clearly incorrect. *TLE* is supposed to be an educational (professional) journal; their allowing such patently incorrect results to be published does not educate the readership.

However, given that the authors get the results they have, how can we explain the discrepancy? Two things come immediately to mind. The first is the question of whether the sources and "receivers" are properly configured for reciprocity to hold. For example, the source should be a pressure source (i.e., a dilatational one) because it is in the water column *but* the receiver might be a vector receiver, like a geophone—especially if they are using the amplitude of the displacements in the water as the resulting amplitude. The receiver should be a pressure detector (i.e., a hydrophone) which measures or detects the divergence of the displacement. If they have an "air-gun" source and a "hydrophone" receiver, then the interchange would satisfy reciprocity.

However, I don't think this is the problem in their case; at least, I can't see how that would give the actual results they are getting.

The other problem is that they may not be taking into account the "focusing/defocusing" effect of the salt wedge and the other refractions at the interfaces. They say in the article that they took into account "spherical spreading, dispersion, etc." in the calculation (page 696).

Passing a wave through a wedge causes a change in amplitude of the wave because the area of the wave between a bundle of rays is different on the input and output sides. Note, this is true even for a plane wave where you don't have to worry about "spherical spreading." The amplitude of the wave is increased/decreased by the square-root of the area ratio because the energy is spread over the area and the energy varies as the square of the amplitudes.

The "focusing/defocusing" effect on the amplitudes for a plane wave would be given by

$$\frac{A_N}{A_O} = \sqrt{\frac{\cos(I_1) * \cos(I_2) * \ldots * \cos(I_N)}{\cos(T_1) * \cos(T_2) * \ldots * \cos(T_N)}}$$

where A_0 is the input amplitude, A_N is the output amplitude, the $\cos(I_n)$ and the $\cos(T_n)$ are the cosines of the incident and transmission angles, respectively, at the nth interface. For the reciprocal case, the incident and transmission angles are interchanged so that the resulting amplitude ratio of the waves received in the reciprocal cases (downdip to up-dip) would be given by the square of the above amplitude ratio. Therefore, using the values given by the authors in the paper ($I_1 = 21.78°$, $T_1 = 36.41°$, $I_2 = 16.41°$, $T_2 = 31.59°$, $I_3 = 51.59°$, $T_3 = 25.0°$, $I_4 = 25.0°$, $T_4 = 15.36°$) in their Table 3, we get $A_{down}/A_{up} = 1/0.837$ which is quite close to the reciprocal of 0.836 value given in their Table 4. The correction is a multiplicative one so that the product of the correction and the given amplitude ratio gives about 1.00. That is, the amplitudes are the same for both the downdip and updip rays when corrected for the "focusing/defocusing" effect. Apparently, this explains the discrepancy in their results.

Their ray-tracing program should have taken the effect into account. The "focusing/defocusing" effect should be part of the geometrical spreading of the wavefronts or of a bundle of rays. If the ray-tracing program had done the geometrical spreading correctly, there would not have been a discrepancy—and *no* "violation" of reciprocity would have been noted.

—Anthony F. Gangi
Texas A&M University
College Station, Texas, U.S.

Reply by Guillaume Cambois, chairman of The Leading Edge *Editorial Board*

As Francis Muir noted, *TLE* does not have a peer-review process, but it does have a review process. I edited the series of papers by Muerdter et al. (originally one long article which had to be cut in three parts). In my review, I did point out to the authors that they were violating the reciprocity principle and even suggested that it might be a bug in their software. The authors chose to publish nonetheless, and even elected to include "nonreciprocity" in their title.

A proper peer-review process would probably have rejected the paper. *TLE*'s "loose-filter" policy allowed it to be published. The three papers (Acquisition/Processing June through August) bring a wealth of geophysical insight and are quite exhaustive in their analysis of subsalt reflections. This is reason enough for publication. If the authors believe they have found a flaw in the reciprocity principle, that's an additional reason to publish. Criticism and rebuttal are then obviously welcome and the debate can move forward. The role of *TLE* editors is not to censor, but on the contrary to encourage new ideas (especially the ones that challenge "conventional wisdom") and stimulate debates.