Re: Measuring Acoustic Transmission Loss Using the 3-Point Method by S. Bilawchuck, and K.R. Fyfe, Vol. 30, No 4, 2002

The authors present a method that is claimed to provide a means of in-situ testing of dissipative splitter-type silencers. The manuscript contains a number of errors, which render the analysis and experiment invalid.

It is well known that transmission loss is defined as a ratio of acoustic powers. The authors chose to use intensity: that is the flux of energy, a vector with dimensions of W/m^2. Under very restrictive conditions the numerical value of magnitude of the acoustic intensity may equal the acoustic power. In any event the division of vectors is not a valid mathematical operation. A proper definition would have been appropriate. After all, it is not the function of papers published in a scientific journal to mislead non-expert readers.

The inappropriate use of vectors is also found in the discussion of spectra, particularly the cross-spectra. These are normally complex valued functions. Although there are certain superficial similarities between complex numbers and vectors, the spectra referred to in the paper are not vectors, and should not be defined as such.

It is claimed that $P=P_i e^{-ikx} + P_r e^{ikx}$ is the general solution of the one-dimensional wave equation. Now it is easy to show that $f(t-x/c)+g(t+x/c)$ is the general solution of the wave-equation. This was already well known to Euler who noted that the functions f and g could be arbitrary and did not have to be continuous, a dilemma only resolved much later with the introduction of generalized functions. Even though some readers may be able to 'read between the lines' and add the phrase "for pure tones" there is no reason to the so sloppy. There are also typographical errors in 2a,b.

The authors then present an equation, which purports to extract the incident sound power from measured data. The formalism assumes the incident acoustic power to be invariant along the duct. It follows that any equation describing it cannot be a function of a single distance from the face of the silencer. Terms containing only X1 or X2 are quite suspect. On physical grounds forms containing (X1-X2) have at least the potential of being valid. The appearance of $(e^{-i}+e^{i}) =2\cos(1) \sim 1.0806$ is also puzzling. This writer has certain suspicions about the genesis of the term. It is known to appear, albeit infrequently, when students unfamiliar with the algebra of exponents try to factor complex numbers.

The illustration of the apparatus shows the duct height to be 2ft. If the width is no larger than 2ft, the assumption of 1D waves in the duct fails for frequencies greater than about 280 Hz, rendering all measurements above that frequency invalid. I find it hard to believe that the authors are not aware of this fundamental feature sound in ducts!

Downstream of the silencer only a single measurement is performed. Again, as was the case upstream, there are higher order duct modes. Even below the cut-on frequency the sound field is not uniform: the sound waves emerge from the air-passage, and not the splitters. Also, fiberglass terminations of the kind shown do not perform well at low frequencies ($f < 300$ Hz).

The technique relies on phase and magnitude differences between the two field points, one would expect that matched microphones (phase and amplitude) are required. No reference is made of this rather important point. The expression for the incident sound power is singular. The incident sound power is completely arbitrary and does not depend on the choice of the field point separation. The result is an artifact of the technique, similar to the limits imposed the measurement of acoustic intensity with two closely spaced (phase matched) microphones. While important issues such as singularities are ignored, the authors, for some un-explicable reason, stress that cross-spectra must be measured simultaneously.

The statement that one must seek out special locations for 'best results' is also quite troublesome. Who decides what are 'good results'? One may speculate that the algorithm returns negative incident powers for 'badly chosen locations'.

The authors propose their method as an 'in situ' technique. Silencers of the type tested are normally installed in HVAC ducts, and the air is in motion. The acoustics of a moving medium differs significantly from that of a quiescent medium. This is especially true for the energy flux. No attempt has been made to warn potential users of this, and other measurement problems that arise in a moving fluid.

Even if the analysis is revised to make the equations valid, there is no magic wand that can impart any degree of validity to the measured data presented in the paper.

Werner Richarz, Ph.D, FASA
Aercoustics Engineering Ltd.
Toronto, ON

Steven Bilawchuk Responds:

First, we trust that the reader understands that this was a STUDENT paper presented in a conference proceeding and only allowed 2 pages to summarize an involved topic. The following are direct responses to the specific items noted in the letter.

Indeed definition of TL should have been stated as the ratio of acoustic powers.

We used the term "vectors" to denote that the values of pressure are not just at a single frequent, but rather over a range of frequencies. Using matrix notation, we typically denote a single column matrix as a vector. This is not to be confused with a value which has magnitude and direction. We are sorry if this caused any confusion.

It was not our intent to mislead the readers regarding solution to the wave equation. When dealing with 1-D waves, the equation as we have stated is correct. There is an error in the equation when defining the wave number, k. The "p" in the equation should be the symbol for Pi and is such in the version we sent in for publishing. This must be an error in format when publishing.

It was not stated (although it should have been) at the beginning of the paper that the method is only valid while plane wave propagation exists. This means that the sound pressure level is indeed constant throughout the cross section. It is not sure what the issue is regarding X1-X2. This is a simple subtraction of two distances. Also, the comments on the exponential terms are not relevant and the information presented in the paper is correct.

The upper frequency limit of 280 Hz is correct. We are currently looking into methods to go beyond the plane wave propagation limitation, thus enabling higher frequency measurements.

Again, the limitations of plane wave propagation enable the single measurement point. We agree that it would be better to have two points downstream since the equipment and analysis techniques are already in place.

Indeed phase matched microphones were used for the test. Alternatively, non phase matched mics could be used if a "normal" and "reversed" test is completed to cancel out the phase differences (as outlined in ASTM E 1050-98). This was not stated in the paper for lack of space. We felt it important to point out the simultaneous requirements for the cross-spectrum.

There were many tests performed for mic location. The results were not discussed in detail due to the lack of space. A "snapshot" of the results was presented. As with any acoustical measurement, there is going to be a degree of error. We were simply trying to find that range of error for various measurement locations. Differences for all locations were still small (less than 1-2 dB, which falls within any reasonably expected instrumentation/operation error). This amount of difference was not stated as it perhaps should have been.

Indeed the work presented was for motionless air. Also it assumed homogeneous fluid conditions throughout. We understand that real world systems are not this ideal, but the work has to start somewhere. Once basic methods are in place, they can be built on for more accurate results. Again, due to the limited space in which to convey the "gist" of the topic, we did not mention this.

We feel that the equations are valid within the range of plane wave propagation and that they need no revision. Indeed, beyond the maximum valid frequency, the results are not to be generally accepted. However, we are puzzled by the fact that the two methods still match each other so closely throughout a very large frequency range.

We apologize to any readers who may have been mislead or confused by our paper. The information presented is important in that it conveys a basic premise for measuring in-situ TL. Work still is required to extend the basic relationships to a more general method. At this time, we do not recommend use of the method other than to get a general idea of the TL performance, and not necessarily an exact result. In addition, we feel that the learned author of the letter could have used much more tact in wording his concerns. They are indeed valid concerns which address some oversights on our part. This could have been pointed out in a more constructive way which better serves to contribute to an overall increase in knowledge among professional colleagues.

Thank you for allowing us the opportunity to reply.

Steven Bilawchuk, M.Sc., E.I.T.
ACI Acoustical Consultants Inc.