What I Don't Know

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Almost 15 years ago I went from a bon-vivant chick-magnet to a precocious old coot. I got through that precocious phase. Now that I am in the fully-developed geezerhood stage of life, I thought I'd take this opportunity to write about things in acoustics that I don't know. That tome would cover many pages and take years off your life if you actually felt like reading it, so I'll limit it to the most pressing issues. (Disclaimer: when submitted to *S&V*, this article was full of profound truths and insights. If you miss them, it is only due to the decisions of the editor to remove any worthwhile material.) So here are some things in acoustics that bothered me . . .

Calibration Intervals of Instruments. For years I have been ranting about why most people insist on yearly instrument calibrations. Back when I was young (a long time ago), I attempted to get data from instrument manufacturers to justify the yearly calibration interval, which is a defacto standard. I had no luck and began questioning the arbitrariness of the interval.

Suppose you calibrate your instruments yearly; does that mean there is no need to recalibrate after you dropped one? The reality is, you only know an instrument is really in cal on the day you calibrate it. It turned out that I wasn't alone in my skepticism. There were several professional groups that questioned the norm, including the Organization Internationale Metrologie Legal and National Council of Standards Laboratories. After I began my questioning, both organizations put out documents that at least suggested the one-year interval was meaningless. They argued, as I did, that the interval should depend on use (if it sat on a shelf for a year, did it need to be calibrated?); on history of drift (suppose it was rock steady as most digital instruments are); on criticality of measurements (suppose you only care about approximate sound levels or have a limited frequency range); and on abuse (suppose you drop it).

The company for which I work has an independent calibration facility. It is not in our interest to state this, but finally ISO 17025, a standard for quality control of test and calibration laboratories, has wording that states a lab *may not* put calibration due dates on instruments. The standard reaffirms that only the user, based on past data and instrument use, can determine intervals. While I will deny I said this if pressed, in just about all modern instruments we have calibrated, very few have ever needed adjustment and the laboratory 'calibration' simply verified the manufacturer's published specifications. That means they are rock steady. My advice – develop a justification for your calibration intervals based on experience and past calibrations. Save money, time and tsuris (French for 'worry'). Don't follow the yearly requirements unless legally mandated. Use a quality calibrator and see if the results make sense. If they do, you are probably in good shape.

Use of Microphones – Another Baffling Problem. Most microphones are characterized as "free-field" or "random incidence." The former has a well defined frequency response for an acoustical field without any reflections and measures the pressure as though the microphone was absent. The latter is for a random incidence acoustical field (closely approximated by a pressure response) with the microphone as part of the field. But when we measure anything (community noise, HVAC noise, industrial noise), the field is *never* 'free' or 'random incidence.' This means the well defined frequency response for ideal fields is not met and the actual response to the measurement field is unknown. Thus the accuracy of the measurement is a function of the spectrum of the sound, which is not known except by measurement. Conclusion: the uncertainty of measurements is much more than we are led to think.

Windscreens. Just about all acoustical measurements outdoors include the use of a windscreen placed over the microphone grid. Few people give thought or care about the effects of the windscreen on the measurement. Ostensibly, the screen reduces the effects of wind on self generated noise at the diaphragm. But, at the same time, the windscreen adds a transfer function – an insertion loss – as a function of frequency that modifies the spectrum measured from the spectrum produced. Unless this transfer function is known with some uncertainty, the accuracy of the measurements is in dispute. Now, a few manufacturers provide some insertion loss characteristics of their windscreen but the test method is not defined and normally the uncertainty of the results is not given. However most windscreens are uncharacterized. Experience shows that a windscreen can easily change a Type 1 measurement to a Type 2 measurement, and worse, can effect frequency response by more than 5 dB at higher frequencies. Conclusions: if you want the measurement to be accurate you must know the insertion loss of your windscreen. At present, ANSI S1.17 is the only consensus standard to characterize windscreens.

Transmission Loss. This is a measure of the noise insulation of a partition. A flat panel is placed in an opening in a suite of two reverberation rooms. Noise is produced in one and the frequency response of the power on both sides is measured. The specimen area and the receiving room absorption are used to get a normalized value. But what is it that is being measured, a construction or a specimen? I say the latter since it is essentially a plate, or compound plate, with fixed or pinned (or an unknown combination of) boundary conditions. Leaving aside the internal construction variability, the frequency response is a function of the aspect ratio and the size. (Imagine that a very large specimen would have a very low low-frequency loss and a vice versa for a very small specimen.) What does the test say – the TL is a test of the specimen only and does not measure the property of the construction.

Field Transmission Loss (E-336 and Equivalent). If lab transmission loss is not well understood, the field version is even worse. Is there a realistic comparison between the field and lab values of TL when variability of diffusion, flanking, construction, measurement method, room volume and specimen area are often impossible to reproduce in the lab? I think not. My recommendation – abolish the concept of Field TL and stick with Noise Reduction (the only realistic measure of isolation anyway, in my opinion). Specify NR as a performance requirement and forget trying to compare a lab test with a field test.

Temperature and Wind Gradients. These occur all the time but are temporally and spatially varying. These phenomena can effect measurements of noise as well as long range propagation. Because these gradients are probably never measured, we know nothing of the accuracy of the sound measurement. The uncertainty of the measurement seems to be inversely proportional to measurement time. This suggests long-term averages are the only way to predict, specify and measure outdoor sound pressure level.

Value of Precision Measurements in Acoustics. I often wonder why we measure down to tenths of dB. What are we going to do with high precision test results? Do they matter? I suggest not, except for scientific veracity. For example, what do we really know about the randomness of community noise? Clearly short term measurements vary (cars, no cars, cars, etc.) and with changing environments, I suspect yearly descriptors (virtually useless) will never be the same from one year to another (changing demographics, etc.). What does one do with a very accurate measurement instrument? Does it give any better information? In industrial noise, except for few cases, repeatability of a test will never give the same results within 1-2 dB. In architectural acoustics, can you really tell the subjective difference of a ceiling tile with a random incidence sound absorption coefficient of 0.80 or 0.85 or 0.75? I suspect that one cannot.

The sound power determined from an air-handler in a reverb room can be measured within 0.1 dB (oh yes, there are uncertainties to be added). Since the quantity we really care about is sound pressure, and the air-handler is mounted differently in less-than-ideal-conditions when installed, does that 0.1 dB matter? My conclusions – the science and sophistication of noise measurement far exceed the subjective usefulness of the measurements. Type 2 meters could replace many measurements requiring Type 1 meters with no appreciable difference in real results. We could reduce costs and efforts with no reduction in effectiveness.

Sound Absorption Coefficients Greater than 1.0. This happens when diffraction effects, sample placement and size in test lab results in numbers that exceed reality. We report them because we want to report measurements correctly. How are the data used? Often the > 1.0 coefficients are rounded down to 1.0 because we know absorption coefficients greater than 1.0 are not theoretically possible. But what about all the other coefficients in the spectrum, are they rounded down the same amount, or left alone because they are okay? I know neither the answer nor what to do.

The OSHA 90 dBA Exposure Limit. This limit purports to protect workers' hearing. One thing no one ever accused me of was knowing much about the health effects of noise. If people ask, I spout the OSHA limits. But as I look back, I wonder how much "good science" was used to develop the criteria. Consider the available technology back then – no integrating meters, many noisy industries, no standard measuring methodology and heavy politics to encourage (unions), or kill (industry) the development of a regulation.

So I ask, what data were used to determine that when someone is exposed to levels of 90 dBA for 8 hours/day for his or her lifetime, hearing damage may ensue? I do not question the loud-noise relationship to hearing loss, I just wonder how the data were sufficient to be so precise in the wording of 29CFR1910.95. With no earphone mics, no sophisticated instrumentation and no methodology of measurement, there could not be much strong epidemiological evidence (I suspect) between a particular level and an expected hearing loss. Probably, there was some determination made that said the 90/8 criterion was protective of some percentage of the population. I would like to know how that was determined.

Uncertainty. Every measurement has an associated uncertainty. It comes from instrumentation imprecision, lack of known bias, operator variability and procedure variations. Every test method, every test report, every piece of test data should have an associated uncertainty so that the reader knows that he/she is reading non-exact data. And, this should go for every algorithm too. Since an algorithm is an approximation of a physical process, some uncertainty, determined when the algorithm was developed, should be part of the algorithm. So the user of the algorithms, consultant or customer, knows that the prediction is no better than the uncertainty. Of course this complicates reports and explanations and makes all our jobs harder. But, I believe it will reduce conflict when data or predicted levels are expected but are not achieved.

'Correcting' for Ambient Noise. Normally we say, to correct for ambient noise, we use an equation that is often in the form of a table. The equation is of the form $L_{p(\text{source})} = f\,[L_{p(\text{source+ambient})} - L_{p(\text{ambient})}]$. The equation and table only require that $L_{p(\text{source + ambient})}$ be greater that $L_{p(\text{ambient})}$.

From this table if the difference between the source with ambient and the ambient alone is 10 dB, the correction is about –0.5 dB; if the difference is 5, the correction is about –1.5 dB; if the difference is 3, the correction is –3 dB. If we continue with the equation and the difference is 1, the correction is –6.8 dB. The equation can hold to numbers very close to 0.0. But the conventional rule of thumb is this – if the source with the ambient is within 3 dB of the ambient alone, the measurement can't be made. Surely the equation allows it. So why the restraint? I really don't know, maybe you do. Keep in mind that community noise, especially ambient noise, is often not steady which complicates the implementation of the equation (little guidance is given to defining ambient noise, especially compared to, say, a maximum fast, a-weighted sound level.) I suspect it is because there is uncertainty in the specifications of the instrumentation, but this should not matter. Perhaps, with old analog meters, the accuracy of reading a fast moving needle or averaging it, gave an uncertainty of 3 dB. Then again, maybe not.

From the above (I do not want you to think I am a Luddite, even though I am), I am suggesting we presently put more effort and time into the science and instrumentation than is necessary for appropriate application of the results. SV

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