Further experiments on phase audibility

A new method of estimating phase distortion in audio systems and some listening tests

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Like many developments in physics, this study arose from an *in*ability to do something. Although my attempts to fool people with live-versus-recorded comparisons have succeeded when the listeners were far from the sound source (more than twenty feet), my most diligent attempts to fool the same listeners have utterly failed when they were close to the loudspeakers (less than twenty feet away). This was true with both "one-eared" and "two-eared" experiments.

There are several possible explanations, and among them is "phase distortion" – by which I mean that the bass and the treble are delayed by different amounts of time during the record/playback process. According to this hypothesis, at a distance from the speaker, a large percentage of the sound is reflected, and therefore is phase-distorted, for *both* the live and the recorded cases. You can't tell the difference, and therefore you can be fooled. But close up, where the sound is mostly direct, the live sound would not be greatly phase-distorted, while the recorded sound would be distorted because of imperfections in the record/playback process. Presumably the ear could tell the difference and wouldn't be fooled.

New commercial loudspeakers with improved phase response have been appearing all over the place. Some are "linear phase,"[1] some are "minimum phase,"[2] and some are claimed to be essentially "phase constant."[3] I say "essentially" because it is not practical to be exactly coherent, since a motion of your head up or down from the centre axis of a two-way, non-coaxial loudspeaker can put you out of exact coherence when it comes to frequency pairs such as 800Hz and 8000Hz. If exact coherence is important, then the whole thing is hopeless from the standpoint of commercial loudspeaker design.

At the other end of the scale, extreme phase distortions, corresponding to differential delays of 10 milliseconds or so, have been shown by telephone researchers to be audible and bad. But that extreme sort of distortion is not what we are discussing here, either.

It has been hypothesized from time to time that a fair degree of phase coherence is necessary for realism because live musical sounds have steep initial wavefronts (almost like square waves) and the only way to preserve these fronts during recording is to keep the high frequencies and low frequencies travelling together. But this is probably wrong, because live musical sounds do *not* have steep initial wavefronts, and, quite the contrary, they take at least a few tenths of a millisecond to build up to full volume. That has been shown for music and handclaps by Duncan *et al.*,[4] and you can see it yourself if a storage oscilloscope is available. A few tenths of a millisecond is several complete cycles at 8kHz, so initial wavefronts do not have to be steep – at least from that line of reasoning they don't.

However, it is well known that we don't fully understand these things, and "lines of reasoning" do not always correlate with audio realities. If phase distortion is audible, maybe it does affect realism, even if we can't say why. Anything that is a "distortion" and is audible should probably be eliminated. So is it audible? Many previous experimenters have said, "no" for monophonic sound. But V. Hansen and E. R. Madsen of B&O in Denmark have claimed[5] that small monophonic phase changes can be audible under some circumstances. A few acousticians

Definitions and examples

In a sound reproduction process, which inevitably involves some sort of a time delay, the term "phase distortion" refers to a change in the shape of a complex waveform, such that the low frequency parts of the wave are delayed by times which are different from the delay times of the high frequency parts. (These are absolute times, measurable in seconds, not the relative times measurable as multiples of a variable such as a peak-to-peak wave period.)

To clarify the concept, let us consider a short musical note consisting of a 1kHz wave plus some harmonic content at 2kHz and 3kHz. Suppose it is reproduced through a microphone, amplifier, and loudspeaker with perfect overall phase coherence, that is, zero phase distortion. Suppose the three frequencies are uniformly delayed by the same absolute time, namely one millisecond. The 1kHz part of the waveform will be delayed one whole cycle, but the 2kHz part will be delayed longer *relative* to its own time period, that is, two whole cycles, and the 3kHz part will be delayed 3 cycles. So the relative delay, sometimes expressed in the form of angles, depends linearly on the frequency. There can be linear phase *relationships* to the original signal, even though there might be zero phase *distortion* (all harmonic components remaining perfectly in-phase with each other).

Suppose, however, that the loudspeaker system imposed some absolute-time phase distortion, causing the output to have a two-cycle delay at 1kHz (2 milliseconds absolute delay), a three-cycle delay at 2kHz (1.5 millisecond), and a four-cycle delay at 3kHz (1.33 millisecond). The number of cycles of phase delay could still be plotted against frequency and show a linear relationship to the original signal, but the absolute time delays in milliseconds would each be different from each other, with accompanying distortion of the waveform. There are also many other ways to plot phase angle versus frequency which are linear (especially on the usual semi-logarithmic paper!) but which actually involve phase distortion of the waveform.

Fig. 1. Wave used in new test method for estimating phase distortion.
Fig. 2. The test wave in Fig. 1 after being subjected to phase distortion.

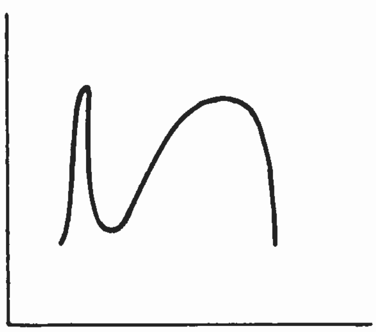

Fig 1

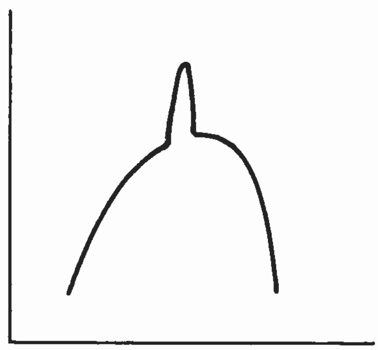

Fig 2

believe that Hansen and Madsen are correct, and a few others don't. (Of course, everyone agrees that *inter*aural phase changes are audible, and this contributes to stereo localization.)

I think that Hansen and Madsen overlooked a serious potential problem in their technique, which I happened to uncover while trying to duplicate part of it. They used Koss ESP-9 electrostatic headphones, which I also used, and they assumed that the acoustic output of these headphones has a wave shape that is a very accurate reproduction of the electrical input. However, my impulse tests show that the ESP-9 in an essentially anechoic environment rings a little bit, and it therefore acts in some ways like a slightly reverberant room. Hansen and Madsen admit that a reverberant situation will give a falsely enhanced audibility to phase shifts, due to destructive interference effects. This causes loudness changes at certain frequencies, which the ear can hear very well indeed. So maybe we just don't have enough transducers to do the experiment unequivocally.

I don't think we really need to know whether a *pure* signal of some kind is audible, although it is an academically interesting subject. What we do need is an experiment that directly compares a phase-coherent loudspeaker with an incoherent one, keeping everything else identical, and playing music. We need to determine which one is more realistic.

B&W Loudspeakers Ltd have published a report that seems at first sight to be concerned with just that very experiment.[6] They arranged listening tests of music with two nearly identical loudspeakers, one phase-coherent and the other non-coherent. The jury was polled on its preferences, which turned out to be strongly in favour of coherence. Frankly, I think their results are inconclusive. First of all, their experiment was evidently not done blind, and we really might expect a jury to choose a sophisticated-looking speaker (their model DM6) as opposed to a plain one (which the incoherent one certainly was). Secondly, the "better" sound was chosen without immediate access to the live performance, so "better" might not be "more realistic."

Estimating phase distortion

Before we can compare different degrees of phase distortion, we have to be able to measure it. I would like to offer here a new method for estimating the amount of phase distortion present in any one link of the record/playback chain, or in the whole chain. The advantage of this method is that it is easy to use, as compared with phase meter approaches (which are not as simple as they might appear to be), and compared with the "raised cosine"[7] or "sine-squared"[8] and fast Fourier transform[9] approaches. (By the way, the Fourier method is subject to considerable error, unless the frequency response and other critical attributes are measurable to a high degree of accuracy.)

The new method involves running a 60Hz square wave through an octave-type graphic equalizer. If the slide that controls the 8-kHz frequency band is set at +6dB, and the other slides are all set at -12dB, each square wave will become a skinny spike, as viewed on an oscilloscope. Now, if the 60-Hz slide is also raised to +6dB, the waveform becomes the thing shown in Fig. 1. I call this an "S-wave," because it looks like what an American cattle rancher with a branding iron would call a "lazy S."

If an S-wave is now run through a tape recorder, in most cases it will become "phase-distorted" and look something like the wave shown in Fig. 2, because there now is a difference in the delays applied to the treble and the bass frequencies.

A disadvantage of this testing method is that one cannot easily obtain a continuous reading of phase shift versus frequency during a sweep through the audible spectrum. However, we usually don't need a continuous reading, and looking at only four or five points on the frequency scale will tell us a lot. For improved accuracy at the treble end, it is best to break the frequency span into smaller steps such as 8kHz/2kHz, then 2kHz/500Hz, then 500Hz/120 Hz, etc. This way, small time delays in the higher of the two frequencies being studied will show up better.

Using a sequence of S-wave tests, I have found that, while the extreme bass and treble of the Tandberg 3300X cross-field tape recorder are badly phase-distorted (unequally delayed), the range from 120Hz to 8kHz is essentially constant phase. (Note that this is not "linear phase"[1] or "minimum phase"[2] but is essentially zero phase distortion.[3])

S-waves can also be sent through a complete record/playback system, and my experiments with that can be summarized as follows. The recording chain consisted of an S-wave going through a Bose 901 equalizer, a Dynaco 400 power amplifier, a single Bose 901 loudspeaker facing forward (not reflecting), an air link, a Thermo Electron 814 microphone, and a Tandberg 3300X tape recorder with Maxell UD tape at 7½ in/s. (Only four coplanar cones of the Bose 901 were used. The other five cones were covered with lead-loaded vinyl sound absorbing sheets.) Playing the Tandberg back through the equalized Dyna 400, the Bose 901 speaker (facing forward again), and the 814 microphone to an oscilloscope showed no phase distortion visible with the S-wave test, from 120Hz to 8kHz. Therefore, the whole system was essentially phase-coherent.

The Bose 901 loudspeaker was used because it has no crossovers, and the Tandberg machine was chosen because its cross-field system is reputed to minimize phase shifts. The 814 microphone is an electret type which is very similar to the more commonly known AKG Model C-451E. Having an unusually flat response curve in the entire audible frequency range, it also has minimal phase effects over the important range of 120Hz to 8kHz.

However, a variety of other devices such as dynamic microphones, other tape recorders, and electrostatic headphones each showed gross phase distortion (as in Fig. 2) when they were individually substituted into the chain.

In addition to measuring phase distortion, S-waves can be used to test speakers and microphones for ringing. A very-low-frequency square wave is fed into the graphic equalizer, which causes a gap in the time between successive S-waves. Overswing across the zero-amplitude line on the oscilloscope display indicates ringing (a form of poor "transient response"). The pulses are too short to allow full ringing build-up, so the method is less than ideal. But it is convenient, and it does quite graphically show up any tendency toward undirectional overswing. The room reverberations can usually be separated out, since they come much later.

By adjusting the equalizer pass bands to find those worst-case frequencies that maximize the ringing, it was found that electrostatic transducers (ESP-9 phones and B&W model 70 speakers) and also Magnapan speakers are not Simon-pure after all, and do ring slightly. This was also true with pure treble as well as pure bass. Good-quality cone-type speakers turn out to be just as effectively damped. (I suppose we should have expected this. The Mylar diaphragms might have low mass, but they also have very little mechanical damping action – not much more than in a bass drum!) For confirmation of this, see advertisements for the B&W model DM6 speaker[8].

Audibility of phase distortion

Using the 814 microphone, Tandberg, and Bose chain, I monophonically recorded repetitions of a 698-Hz xylophone note (with its overtones, of course). The loudness was kept at moderate levels so that overload was not a problem. It was played back using separate graphic equalizers as a crossover, splitting the signal into the below-1-kHz part, which went into one 901 speaker, and the above-1-kHz part, which went into another 901 speaker. (The frequencies above 8kHz, were filtered out altogether, since they would have been phase-distorted.) Putting the two speakers approximately side by side (or one above the other) gave no difference in realism from putting them several inches in front of and behind each other.

The graphic equalizer itself has some effect on the phase, so the essentially zero phase distortion (coherent) signal was not obtained with exact side-by-side placement. Also, the treble tends to come from the apex of the speaker cone, while the bass comes from areas farther forward.[10] A displacement of 1¼ inches did not produce essentially coherent sounds. Both these and the incoherent (5-inch displacement) sounds were compared with live performances of the xylophone notes. (This was also repeated with a variety of other musical notes.)

There were plenty of "differences" in the sounds. In fact, I have never found two loudspeakers that sound exactly alike under ordinary circumstances,[11] so the sound does depend on which Bose 901 handles the treble, etc. Watch out for this when you read other people's reports on similar experiments! No relative speaker position was clearly the most realistic when quickly or slowly A-B'ed against the live performance. (This is an example of what I called the LAB test in a previous article.[12])

The conclusion is that a fairly high level of phase distortion does not affect realism.

This business of speakers each sounding different dredges up another one of those deep philosophical problems. Unless we attack it with very clear thinking, it's liable to become a virtual Loch Ness monster. Suppose all loudspeakers sound different (and S. K. Pramanik of B&O states very definitely in reference 11 that they do). Then how can we ever expect one to sound like the live performer, if it can't even sound exactly like a duplicate loudspeaker? It seems to be impossible to remove all such differences, or at least it seems impractical.

Here is my way of pushing the philosophical monster back down. I am willing to accept a *difference* between the live and recorded sounds, just as I will accept a difference between duplicate live instruments, each being equally "realistic." What I am trying to do is prevent a blindfolded listener from *identifying* which sound is recorded. This is what does happen at a distance, where listeners can actually be fooled. Then, even if a listener practices for a while and does learn to identify the sources, I am trying to get the honest listener to say that neither one is *best.* It should sound like two different "live" instruments. That is what I mean by "realistic."

The main criticism I can see for the whole study is that the Bose 901 loudspeaker facing forward is possibly a too-imperfect device to prove anything, primarily because of the diffraction peaks in its frequency response curve caused by its multiple drivers. (It is not meant to be used facing forward for close listening.) Also the side-by-side arrangement of the two speakers causes additional interference peaks, because the crossover that feeds them is not optimized to prevent this. But actually, the whole loudspeaker-room system was carefully equalized using a "pseudo-performer" method,[12] and it did sound quite good in spite of the diffraction.

It would be useful if other people tried similar experiments with a variety of loudspeakers. Just please be observant of all the snares mentioned above. This is a tar pit surrounded by quicksand.

What is the true explanation of my failure to fool listeners up close, if it is not phase distortion? I don't have a strong opinion at this point. My best results in listener-fooling have been obtained with a Magnapan MG-II loudspeaker*; played through a large, thin curtain which is strongly lit up from the front and dark behind. This speaker has a fair amount of phase distortion, probably because of its crossover design. Maybe the Magnapan's relatively good performance is due to its size, to its bipolar radiation pattern, or to its unusual frequency response curve. It doesn't have a monopoly on realism, though, because a giant pile of conventional speakers arranged to be bipolar and big sounded just about as good.

I have a feeling that the ear is sensitive to subtleties in the back-reflections off the walls of the listening room, and that is how we can tell the live from the recorded sounds. This might be an interference effect that gets translated into a frequency response effect, and it might be affected by the size and shape of the loudspeaker. Maybe small speakers have high-Q (finely tuned) environmental interferences and resonances, causing strong colorations, while large speakers such as Magnapans or electrostatics have diffuse and weaker colorations of this type. Or, maybe it's the shape of the wavefront, with large speakers providing a more nearly planar-shaped wave.

For a xylophone-to-microphone distance of about a yard or more, the wavefront that hits the microphone is nearly planar. If the loudspeaker is put where the microphone was, maybe the speaker should produce a similarly planar wavefront. (However, I suppose that a closer microphone distance might work better with a non-planar speaker, and I feel this ought to be explored further.)

I repeated these tests in the open air, up on ladders, but I am still unable to fool listeners who are closer than 15 feet from the xylophone. Philosophically, this type of negative result is not very meaningful. Large loudspeakers still gave the best results, but I cannot separate such hypothetical factors as diffraction at the loudspeaker cabinet edges from a myriad of other possible factors. Are the inevitable *small* amounts of phase distortion the important thing? Or the residual traces of reflection from the grassy ground? If I had to guess, I would gamble on imperfections in the amplitude-versus-frequency curve being the culprit. But this guess is only being made because a small turn of the tone control knob can have such a great effect on the listener's impressions, and not because of any well-understood weighting of the many factors to be considered.

* For UK readers it should be noted that the Magnapan is a large, diaphragm type loudspeaker, similar to an electrostatic, but operated electromagnetically by a grid of fine wires on the surface of the diaphragm. The electromagnetic field from this grid interacts with an array of small, fixed permanent magnets on the framework of the speaker. The unit has separate woofer and tweeter areas.

References

1. "Linear phase" means that the phase distortion is not zero but is linear with increasing frequency. An unusually clear example of this is shown in graphs for a loudspeaker reported by S. Ishii *et al.* of Panasonic, in a paper delivered at 52nd AES Convention, preprint 1059-3L (available from the Audio Engineering Society, New York, N.Y., U.S.A.). Some Technics and Infinity speakers are claimed to be linear phase.
2. "Minimum phase" does not mean zero phase distortion either, but in this case it refers to the *least* amount of such distortion that is theoretically possible when using conventional LC crossover designs. Many speaker systems are designed this way. See R. C. Heyser, *Audio*, Dec. 1974, p. 71.
3. This is really zero distortion, or nearly zero. It is being claimed for the Sony 880-2 tape deck and for some of the newer Ohm, Dahlquist, B&O and B&W loudspeaker designs. (Some types of "linear phase" can be equivalent to "essentially zero phase distortion," if a constant time delay is subtracted from the wave equation. Unfortunately, not all types of linear phase delay have this relationship. Equipment manufacturers have not always been specific in stating which type of linear phase they are claiming for their products.
4. M. G. Duncan, *et al., J. Audio Engineering Society*, Oct, 1975, p. 610 (Fig 5C).
5. V. Hansen and E. R. Madsen, *J. Audio Engineering Society*, Dec. 1974, p. 783.
6. J. Bowers and S. Roe, *Hi-Fi News and Record Review*, April 1976, p. 56.
7. I. Nomoto, *et al., J. Audio Engineering Society*, Jan, 1976, p. 9.
8. Advertisement for B&W Loudspeakers Ltd., *Wireless World*, Feb. 1976, p. 70.
9. T. Muraoka, *et al.*, 52nd AES Convention, preprint 1088-6G (available from the AES, New York).
10. J. Moir, *Wireless World*, April 1976, p.74.
11. S. K. Pramanik, *Wireless World*, Nov. 1975, p. 531.
12. D. Shanefield, *The BAS Speaker*, March 1975. (This article describes a tape recording playback, arranged so that an A-B comparison between two audio components can be juxtaposed against a live performance (called "L"). If the tape recording was originally made in the same room as the playback, environmental resonance effects will become magnified. However, the article describes a method for minimizing these, by using a special technique for equalization (frequency response adjustment). A standard frequency sweep is played through an extra loudspeaker which has been carefully equalized (called a "pseudo-performer"). Then the tape record/playback system is also equalized, while recording and playing back the psuedo-performer's sounds. The equalized tape system itself is now found to be "flat" and is suitable for the L-A-B test).
13. D. Shanefield, *The BAS Speaker*. Nov. 1974.

EXPERIMENTS ON PHASE AUDIBILITY

SEVERAL readers have asked for further clarification of two points in my article on phase audibility (October 1977 issue, pp. 79-81). I would therefore like to add a few comments to the record, as follows.

Question: Were the Bose 901 loudspeaker tests done "up close", and were they "blind"?

Answer: The single Bose speaker, and also the crossed-over pair of Bose speakers, were compared with the live performance "up close", that is, at a distance of ten feet from the listening jury. The tests were run blind, through a lit-up gauze curtain. Listeners could not be fooled at this distance, but a rank ordering of quality (best, equal, worst) was attempted. The essentially phase-coherent playback was not any more like the live performance than was the phase-distorted playback.

When the tests were run indoors, in a typical household environment, the Bose speakers were able to fool listeners at a distance of 35 feet (through a large, open doorway), but not any closer. The Magnepan speakers fooled the listeners at 25 feet indoors and 15 feet outdoors, but not at ten feet.

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Editor's note: The following corrections should be made to Dr Shanefield's article. On page 79, middle column, the final six lines of the column should have been printed before

Letters to the Editor

THE LANGUAGE OF HI-FI

Your balanced and sensible leader in the August issue came as balm to my inflamed spleen after also reading in one of your considerably less distinguished contemporaries that a highly respected preamplifier "sounded boring" and "made the music sound as if played by amateurs". Surely the nadir of lunacy in the use of subjective language! One gets the impression that these terminological outrages are being perpetrated on gullible readers by a new breed of journalistic *wunderkind*, who would probably be hard pressed to define a decibel. The reasons for this development are beyond me – probably it is either an effort to conceal technical incompetence or because it makes saleable copy; or a mixture of both.

Of course, I am not against the use of subjective language. What I am against is the increasing tendency to use language of imprecise meaning. To misquote Gertrude Stein "a volt is a volt is a volt" and I hope no one is going to question that or challenge that a volt measured in hi-fi equipment is any different from any other. But when someone says vis-à-vis the performance that the "information retrieval efficiency was low" (yes, really — I didn't make it up) then like the late and quite unlamented Hermann Goering, I reach for my axe. If I as an experienced professional engineer cannot understand it, then heaven help the poor layman.

We commentators in engineering journalism have a heavy responsibility and should never resort to language that is capable of alternative interpretation or is open to doubt: and if there is a slight doubt, then it should be clearly defined or explained. At the risk of being accused of pedantry, I will go further and say that every observed phenomenon in reproduced sound is measurable and may be expressed in quantitative terms. Some subtle effects perhaps may be harder to measure than others; but I am with Galileo and Lord Kelvin. Inventing new words is not the way out.

May I finish with another observation, and a warning against another tendency not confined to the popular hi-fi press? This is the lack of a sense of proportion and a failure to appreciate the realities of the technical side of audio. I have just been reading with interest an article in a well-known technical publication. The writer discusses with great insight, the technical desiderata for a pickup input stage; then spoils it all by proudly declaiming in the final paragraphs that the improvements result in a reduction of the t.h.d. to 0.0004%. Marvellous. Then if someone is able to make a gramophone record and cartridge capable of the same order of inherent *Dt* we might just be able to notice the difference.

Reg Williamson
Norwich

AURAL SENSITIVITY TO PHASE

I fear that Mr Moir (Letters, July 1977 issue) has misunderstood the point which I was trying to make in my letter on the audibility of polarity reversals (Letters, May 1977). Far from the distortion of one stage in the amplifier chain being cancelled by a complementary distortion in a subsequent stage, as suggested by Mr Moir as an explanation for the effects I discussed, I was at pains in my letter to make clear that this was *not the case*. All subsequent stages in the chain, including the transducer, were shown not to be responsible for the effect in question. (In the case of the loudspeaker, this was done by listening from both front and back of the dipolar electrostatic panels, thus introducing a polarity reversal in the acoustic waveform, which was found to reverse the effect.) The change in quality of the signal was due entirely to its *own* asymmetry, not to subsequent distortion. This confirms the earlier work cited in my letter.

An even more vivid demonstration of this effect can be obtained by linearly combining two sinusoidal oscillator signals, one a "fundamental" frequency of around 400Hz and the other an adjustable-level "second harmonic" of around 800Hz. If the second harmonic is allowed to drift *slowly* in phase relative to the fundamental a very pronounced cyclic change in the sound quality of the signal will be heard, and it is instructive to listen to it while observing the asymmetric waveform on an oscilloscope. No such effect appears to occur if the 800Hz signal is shifted to the third harmonic, i.e. 1200Hz; the waveform is now always symmetric with respect to polarity reversals. With a fourth harmonic, however, the effect is again subtly audible if the level is suitably chosen.

Towards the end of his letter, Mr Moir in fact seems to support my argument, by agreeing that *on good signals* a polarity reversal *is* indeed subtly audible. This strikes me as being an important conclusion! Even more than just standardizing the absolute polarity of the whole audio chain, as I suggested, it would seem that the non-linear-phase errors inherent in the use of pressure and/or velocity microphones in recordings, which are reproduced indiscriminately via either pressure or velocity transducers, also requires serious investigation.

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Ontario, Canada.

Mr Driscoll, responding in the July issue to my letter of last February, asserts of himself "My grasp of basic principles is not so uncertain that I could believe Coleman's claim that "tone bursts which differ in the framing of phase" (I wrote "OR 'phase' ") of the sine wave with respect to the burst envelope have spectra of different shapes." My claim can easily be checked, and is correct. Where does that leave his "grasp of basic principles"?

If the members of a regular sequence of tone bursts are well separated, so that they are heard as separate bursts, it is enough to calculate the Fourier transform or spectrum of any one of them. If a particular burst consists of the sinusoid $\sin(2\pi f_0 t+\epsilon)$ gated on for $2n$ periods centred about the time $t=0$ then its transform is $K\sqrt{(f-f_0)^{-2}+(f+f_0)^{-2}+(f^{-2}-f_0^{-2})\cos 2\epsilon}\,\sin(2\pi nf/f_0)e^{j\phi(f)}$ where $\phi(f)=\epsilon-\tan^{-1}((f-f_0)\sin 2\epsilon/(f+f_0+(f-f_0)\cos 2\epsilon))+\pi/2$ and K is is independent of both f and ϵ. If the burst is not a whole number of periods long the expression becomes more complicated.

This spectrum peaks at $f=f_0$, and the width of the peak, taken between neighbouring zeros, is f_0/n, inversely proportional to the burst length, and compatible with the requirements of the acoustic uncertainty relationship. Its shape, i.e. the variation of its modulus with f, clearly does change when the value of ϵ changes, and in addition the reference phase $\phi(f)$ of the component of frequency f depends in a non-linear fashion on both f and ϵ. If the centre of the burst occurs, not at time $t=0$, but at $t=T$, then $\phi(f)$ contains a further additive term $-2\pi fT$. If $\epsilon=\pi/2$ the spectrum of the burst decays at frequencies far from f_0 as f^{-1}, whereas if $\epsilon=0$ it decays as f^{-2}. This is understandable since in the latter case the burst has discontinuities of slope at its ends, but in the former has amplitude discontinuities, which will splash the spectrum out much further, a point about which I warned Mr Driscoll in my February letter. He doesn't have to take my word for these statements – presumably one of his brighter students could check the calculations, or he could ask one of the enterprising loudspeaker manufacturers who have set themselves up with minicomputers, f.f.t. programmes, and graphics terminals to let him see for himself what a sinewave toneburst spectrum really looks like, in phase as well as in amplitude.

It is all too easy for those acquainted in principle with Fourier transforms to mention the use of transfer functions and Fourier transforms for calculating network responses to signals of finite duration, leaving the impression that this is essentially a trivial extension of normal a.c. calculations. It is not, and exposure to the specific Fourier transforms of a few simple signals, such as tone bursts, can go a long way towards driving the point home.

C. F. Coleman,
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Oxon.

CONFUSION ABOUT DISTORTION?

In a letter in your August issue Mr Greenbank quotes an earlier correspondent who states: ". . . 'loss of information' occurs during amplifier 'latch-up' – when, as we all know, 100% intermodulation distortion occurs." This statement is symptomatic of a general confusion which has resulted from harmonic distortion, intermodulation distortion, "latch-up", "clipping", "slew-rate limiting", and transient intermodulation distortion all being regarded as "distortion".

The use of distortion as a generic term is probably responsible for it being generally unnoticed that the above list may be the results produced by two fundamentally differing mechanisms.

Consider the case of an amplifier which, though it has a non-linear transfer function, has no clipping point or slew-rate limit. Such an amplifier may be modelled by a "one-to-one" mapping function, and because of this an inverse mapping function may be discovered which precisely restores any mapped set of points back to their initial positions. With any distortion which may be described this way, therefore, we always (in principle, at least) perform another process which gives us the information in its "undistorted" form.

Such is not the case with "latch-up", "clipping" and "slew-rate limiting". Each of these may *not* be regarded as a "one-to-one," mapping – rather, they are characterised by a "many-to-one" mapping function. In these cases no inverse mapping function exists which may be employed to restore any arbitary initial point to its original position. We have created a singularity, and a set of points are "doomed to fall down it".

For this reason it will unfortunately tend to cloud the issue to regard "many-to-one" imperfections in a transfer function as "distortion". Hence it is misleading to regard clipping or latch-up as "100% intermodulation distortion". Similarly, it is unhelpful to call the effects of slew-rate limiting "transient intermodulation distortion."

I would not wish to argue that "many-to-one" imperfections are not "distortion" as the word is currently defined— only that we are here clouding the problem by our choice of terms.

As for the "loss of information" concept which prompts Mr Greenbank's letter, all I can do is point out that this may be defined in terms of "many-to-one" rather that "one-to-one" functions. It remains to be seen, however, if either form of imperfection proves inherently "audibly more objectionable".

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EXPERIMENTS ON PHASE AUDIBILITY

SEVERAL readers have asked for further clarification of two points in my article on phase audibility (October 1977 issue, pp. 79-81). I would therefore like to add a few comments to the record, as follows.

Question: Were the Bose 901 loudspeaker tests done "up close", and were they "blind"?
Answer: The single Bose speaker, and also the crossed-over pair of Bose speakers, were compared with the live performance "up close", that is, at a distance of ten feet from the listening jury. The tests were run blind, through a lit-up gauze curtain. Listeners could not be fooled at this distance, but a rank ordering of quality (best, equal, worst) was attempted. The essentially phase-coherent playback was not any more like the live performance than was the phase-distorted playback.

When the tests were run indoors, in a typical household environment, the Bose speakers were able to fool listeners at a distance of 35 feet (through a large, open doorway), but not any closer. The Magnepan speakers fooled the listeners at 25 feet indoors and 15 feet outdoors, but not at ten feet.

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Editor's note: The following corrections should be made to Dr Shanefield's article. On page 79, middle column, the final six lines of the column should have been printed before