LETTERS TO THE EDITOR

COMMENTS ON "FUZZY DISTORTION IN ANALOG AMPLIFIERS: A LIMIT TO INFORMATION TRANSMISSION?"*

The above paper on fuzzy distortion,[1] which was tendered by M. J. Hawksford as speculative, may itself suffer distortion and create new audio myths which will be carried to extremes by the audio community. At the heart of the issue, it seems to me, is the question of whether charge quantization creates anything other than well-known shot noise, and even given a staircase-like quantizer model, whether the concept implies any real nonlinearity which may result in lack of signal transparency, aside from noise. In this letter I will outline my belief that traditional theory has adequately dealt with charge quantization in bipolar devices.

First of all it is suggested that Eqs. (11) and (15) be interpreted as a granularity giving rise to amplitude quantization and $1/f$ noise. I find no compulsion to regard a small change in base charge as implying quantization. It simply results in a noise determinable by the application of statistics. To regard the base charge as defining collector current is not very useful at low frequencies ($2\pi f r_{b'e} c_{b'e} < 1$), for which the current in $r_{b'e}$ is more important than the current through $c_{b'e}$. The control charge has current carriers flowing into and out of it continuously, so it fluctuates on a very short time scale and generates shot noise of large bandwidth. Traditional theory would compute the noise from equivalent shot-noise generators across the transistor junctions, and would include the effect of partitioning the emitter current into base and collector currents.[2] The control charge itself does not generate noise as such. Its fluctuations represent the shot noise in the system. If a bipolar transistor is measured at very low levels, its distortion should be vanishing, with a resolution determined only by the random noise in the observation bandwidth. This indeed is the case experimentally, and any contrary hypothesis must be tested before it is given much credence.

Suppose for the sake of argument that we allow a sort of "cogging" to occur in the base charge, in units of e, a single electronic charge. We do not quite mean that the control charge is made up of an integral number of electronic charges, since that is true under traditional theory, and the fluctuations lead only to random noise as usual. Let us consider Hawksford's "fuzzy nonlinearity" model, Fig. 2(b), in which the incorporated quantizer does not lead to a deterministic nonlinearity, on account of the dither $n(t)$. If signal averaging cannot reveal the error waveform, then the dither must have an amplitude adequate to scarify totally the error from instant to instant, where such an instant may be the recombination time, 10^{-13} s, as indicated by Hawksford. Thus the shot noise acts as a dither signal. But for a totally random error the spectrum is white, and *truly represents noise*. Our work[3] on dither and that of others referenced in our paper show that for sufficient dither (1 LSB is enough) the digital system loses all its digital artifacts and represents a *truly* analog system of low distortion with *truly* white noise. Experiments verify this to be true. Thus a fuzzy nonlinearity as envisioned by Hawksford does not harm signal transparency, except that the noise may be renormalized. If vestiges of noise correlation or other subtleties remain, then a sufficiently intelligent signal averager could recover some error, and again we would have only a deterministic nonlinearity. Properly applied dither will not admit such a possibility, and the human hearing process, although sophisticated, would perceive the error as true noise.

Hawksford speculates that feedback is harmful to signal transparency since increased feedback "forces the signal to within a relatively few quanta." To test this hypothesis I constructed a low-noise input stage, using it as a straightforward amplifier and with about 20 dB of feedback using an additional amplifier, adjusted to the same net gain. Even on low-level signals near the noise limit there were no noticeable changes between the two configurations, even though there is a change in the input impedance.

In summary, for a number of reasons I believe that even if one admits a properly dithered fuzzy nonlinearity, it is indistinguishable from perfect linearity with benign wideband noise. However, the normal theory of noise in bipolar devices, in my view, has already included the effects of charge quantization, and it excludes any nonlinearity at low levels.

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AUTHOR'S REPLY[4]

First may I thank John Vanderkooy for his comments on my recent paper. My intention was (and is) to question some of the classical beliefs that are used to model

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* Manuscript received 1983 December 20.
[1] M. J. Hawksford, *J. Audio Eng. Soc.*, vol. 31, pp. 745–754 (1983 Oct.).
[2] F. N. H. Robinson, "Noise in Transistors," *Wireless World*, vol. 76, pp. 339–340 (1970 July).
[3] J. Vanderkooy and S. P. Lipshitz, "Resolution below the Least Significant Bit in Digital Systems with Dither," presented at 72nd Convention of the Audio Engineering Society, *J. Audio Eng. Soc.*, vol. 32, pp. 106–113 (1984 March).
[4] Manuscript received 1984 February 27.

transistor operation, especially as there is much polarization among circuit designers as to the merits (or otherwise) of particular topologies and devices.

In fact, if I have persuaded the reader to review some fundamental conceptual structures, then that is most gratifying, particularly as semiconductor materials exhibit complex physics at the atomic level compared with, say, the relatively simple and linear construct of a thermionic vacuum tube.

As an undergraduate student of the mid-sixties I had been influenced by the work of Beaufoy and Sparkes on charge-control theory, and later, as a research student, I explored the processes of delta and delta-sigma modulation. Unlike Vanderkooy I find, therefore, a strong compulsion to marry the two areas with reference to the inherent quantized nature of electrons and thus be concerned about the small changes in base charge, suggested by my primitive calculations, as representing quantization. The apparent insignificance of the model as implied by Vanderkooy is therefore an expression of more dominant error sources intrinsic within the transistor. However, a bound is still inherent, though obscured by internal dither.

One area of guilt to which I must, however, confess was an overwhelming desire to interpret each transistor within a notionally analog domain as a sequential digital machine. The wheel had turned full circle; an earlier paper[5] had described digital encoding in terms of analog modulation. It is important to observe that, within the suggested digital feedback model of Fig. 2(b), a substantial clock frequency is implied, which simulates Vanderkooy's observations of rapid control-charge fluctuation. This point is implicit where it resembles oversampling, and it is the vehicle by which signal resolution greater than that suggested by the apparent low changes in control charge is obtained in practice.

I disagree with the comment that since $2\pi f r_{b'e} c_{b'e} < 1$, then $r_{b'e}$ is more important in determining collector current at low frequency. Initially this appears reasonable as, from a circuit description, $c_{b'e}$ can be removed from the circuit where, apparently, a tractable model remains. However, this denies the existence of charge-control theory, which infers that the collector current is closely associated with the charge stored in $c_{b'e}$. Using a continuous model at low frequency, the base charge q_b is given as

$$q_b = c_{b'e} v_{b'e} \approx i_b \{c_{b'e} \quad r_{b'e}\}$$

that is, for $2\pi f r_{b'e} c_{b'e} < 1$, $q_b \propto i_b$ and not the integral of i_b, as is the case when $2\pi f r_{b'e} c_{b'e} > 1$. The former was implied in Eq. (5) and was fundamental to the observations on stored base charge. As a linear circuit problem, both observations are equivalent, though the charge-control model matches my discussion more closely.

I do concur with the general observations concerning the interaction of dither signals and quantization, where significant decorrelation between quantization distortion and primary signal results due to inherent fuzziness.[6] However, whether we should allow the process to translate quantization distortion into noise is open to some debate. It is undeniable that in the quantized/dithered channel the error signal is nonlinearly modified by the presence of the primary signal, a form of nonlinear modulation, where the dither signal is a carrier which codes the true quantization distortion. This modification does not occur in the additive noise channel. The ultimate question is whether this coding leaves vestiges of the primary signal, where I suggest that investigations should concentrate on the error signal devoid of the primary signal, thus eliminating masking processes. Experience suggests, for example, that very-low-frequency sine waves of ≈ 5 Hz leave detectable remnants within the error signal.

If we had two black boxes, one containing a quantizer and optimal dither source while the second contained an additive noise source where the quiescent outputs were normalized, could we construct an experiment to determine which box contained the quantizer? If not, then it would be acceptable to substitute either box within the forward path of a feedback amplifier prior to amplification, achieve identical results, and conclude that quantization distortion was transformed to noise.

I thank John Vanderkooy for his useful and constructive comments and observations.

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[5] M. J. Hawksford, "Unified Theory of Digital Modulation," *Proc. IEE*, vol. 121, pp. 109–115 (1974 Feb.).

[6] J. Vanderkooy and S. P. Lipshitz, Invited Letter (see Views), *Hi-Fi News Rec. Rev.*, vol. 39, p. 15 (1984 Mar.).

COMMENTS ON "DIRECT LOW-FREQUENCY DRIVER SYNTHESIS FROM SYSTEM SPECIFICATIONS"*

In the above paper[1] Keele presents a valuable unified view of the relation between driver parameters and system specifications and enclosure type. Unfortunately he uses the electromagnetic damping constant $R_{ME} = (Bl)^2/R_E$ as a measure of the driver cost. For drivers designed according to common practice R_{ME} may not be the best estimate of driver cost, however. The magnet volume, and thus cost, is determined by the magnetic energy in the voice-coil gap. This energy is proportional to the product of the field squared B^2 and the gap volume, which, neglecting insulation, equals the product of volume resistivity and wire length squared, divided by the resistance, $\sigma l^2/R_E$. Given 100% utilization of the magnetic energy, R_{ME} is thus a good measure of the

* Manuscript received 1984 March 19.

[1] D. B. Keele, Jr., *J. Audio Eng. Soc.*, vol. 30, pp. 800–814 (1982 Nov.).