J Am Acad Audiol 5: 355–358 (1994)

Letters to the Editor

RESPIRATORY DISTRESS SYNDROME

To the Editor:

I have some concerns regarding an article recently published in *JAAA*, entitled "Delayed-Onset Hearing Loss in Respiratory Distress Syndrome" (Konkle and Knightly, 1993). My concerns begin with the conclusions drawn from the two case studies presented and end with the purpose of case study discussion.

Specifically, I question the relationship between respiratory distress syndrome (RDS) and hearing loss versus chronic lung disease and hearing loss. The neonates discussed had probable bronchopulmonary dysplasia (BPD) and had 12 and 17 days of conventional or jet ventilation. There is literature to support a link between immediate or progressive sensorineural hearing loss (SNHL) and BPD. In addition, ASHA's Joint Committee's 1990 position statement specifically suggests the screening and follow-up management of infants with chronic lung disease to rule out hearing loss and to identify potential progressive hearing loss. I also have questions regarding a possible link between the type of ventilation (specifically hyperventilation) and hearing loss. The article in question did not specify whether the babies were hyperventilated. Further research is necessary in this area.

Finally, if the author's assumptions are correct and there is a direct correlation between delayed-onset SNHL and RDS, I am hesitant to alter my audiologic practice based on two case studies. I was fortunate to have been involved in a neonatal screening program where the attending physicians were very supportive of my program. They, however, do not agree with mass screening or unnecessary auditory screening. For that reason, if I were to make such an extreme change in protocol (such as testing the 50% or more of our Neonatal Intensive Care Unit babies diagnosed with RDS), I would need to provide sufficient literature to support my cause. One article based on two case studies would hurt my credibility as an audiologist. Even if mass screening is implemented, we, as a profession, need to be cognizant of the etiology of hearing loss. Knowledge of why we practice as we do can only help us further to obtain the respect we deserve.

Case studies are a necessary and effective tool we can use as a profession to communicate with each other. However, they must be used as the building blocks to form research studies, rather than the sole means to dictate specific change.

Bridgette Fowler
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Joint Committee on Infant Hearing. (1991). 1990 Position statement. *ASHA* 33(Suppl 5):3–6.

Konkle D, Knightly CA. (1993). Delayed-onset hearing loss in respiratory distress syndrome. *J Am Acad Audiol* 4: 351-354.

Response to Fowler

We appreciate the concerns raised by Fowler (1994) in response to our report of two infants diagnosed with respiratory distress syndrome (RDS) who subsequently suffered either progressive or delayed-onset sensorineural hearing loss (SNHL). We feel, however, that Fowler missed the primary intent of our report. That is, we wished to emphasize the importance of audiologic monitoring, in addition to an initial hearing screening, for the management of patients with severe respiratory disease.

Fowler asserts that the hearing impairments suffered by the infants summarized in our report resulted from bronchopulmonary dysplasia (BPD) rather than RDS. We again emphasize that both infants summarized in our report were diagnosed with RDS and not BPD. Moreover, unlike Fowler, we are not aware of literature that supports a link between BPD and progressive or delayed-onset SNHL.

Our intent was not to exclude infants with BPD from audiologic monitoring. Indeed, we state in our report that "it is probable that the infants suffered bronchopulmonary dysplasia, since they were both discharged from the hospital on oxygen" (p. 353). We stress, however, that this observation was solely ours and was offered without medical confirmation. Without

a documented diagnosis of BPD, we continue to feel our patients are best described as RDS, especially since there is no literature to support a causal link between BPD and delayed-onset or progressive hearing loss.

We agree with Fowler that the type of ventilation, in addition to the duration of ventilatory support, is an issue worthy of further investigation. As noted in our summary, both infants were placed on high-frequency jet ventilation; they were not hyperventilated.

Based on the 1990 position statement of the Joint Committee on Infant Hearing (note that this is not an ASHA committee, as Fowler's letter suggests, but a committee of which ASHA is only one of several organizational members), we feel that our suggestions for continued audiologic assessments were consistent with the Joint Committee's recommendations for infants at risk for progressive hearing loss. We recognize that the two infants described in our report were not managed in a manner consistent with the 1990 statement, but, as we stated in our report, both infants were hospitalized during a time when our program operated under the 1982 high-risk register. Thus, at the time these infants were initially seen in our program, we considered the screening process complete when each infant passed an auditory brainstem response screen.

Like Fowler, we too are concerned over developing cost-effective and efficient screening programs that focus on identification of hearing loss in the neonate and infant populations. It was not the intent of our report, however, to address these issues. Frankly, we fail to understand how Fowler generalizes from our discussion any "sole means to dictate specific change" concerning mass screening. We simply did not discuss mass screening in our report.

Last, we join Fowler in observing that case studies represent an important tool that can stimulate discussion and motivate communication among professionals.

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Carol A. Knightly
Philadelphia, Pennsylvania

Fowler B. (1994). Respiratory distress syndrome. *J Am Acad Audiol* 5:355 (letter).

Joint Committee on Infant Hearing. (1991). 1990 Position statement. *Audiology Today* 3:14–17.

INAPPROPRIATE USE OF TERM "SATURATION"

To the Editor:

In an article entitled "Comparison of Sound Quality and Clarity with Asymmetrical Peak Clipping and Output Limiting Compression," published in *JAAA*, Hawkins and Naidoo (1993) used the term "in saturation" repeatedly to describe the output limiting functions of both a Class A linear circuit and a compressor circuit. Shortly thereafter, in another journal, Fabry (1993) stated: "Output-compression instruments do not produce substantial amounts of distortion when in saturation" (p. 26). The context suggested that Fabry was using the term "in saturation" similarly to Hawkins and Naidoo (i.e., ambiguously and, perhaps, synonymously with either "in compression" or with "limiting").

Either of these two instances, occurring in isolation, might have merited only passing notice, but coming as they had in close succession from Hawkins and from Fabry, two of our foremost young investigators, my concern was raised that others might follow their lead in what I consider to be the overly broad and potentially misleading use of a technical term that has a highly specific and time-honored meaning in electronic engineering, particularly as it is applied to electronic amplifying devices. Within that context, the published statements alluded to above are clearly incorrect. A compressor circuit is not "in saturation" when limiting unless it is overdriven (and, strictly speaking, perhaps not even then), and output-compression instruments are, indeed, likely to produce substantial amounts of distortion when they are in saturation.

My argument for using the term "saturation" in a restricted, precise, and unambiguous manner is based upon clear precedent dating almost as far back as the year 1907, when De Forrest invented the first vacuum-tube amplifier, the "Audion." Shortly thereafter, methods of specifying the operating characteristics and transfer functions of vacuum tubes and, later, transistors, were developed and codified, as well as a distinct nomenclature that included the term "saturation." Since it is against this background that the term is best understood and upon which my argument hinges, a brief review of some basic concepts may be instructive.

Simply put, all electronic amplifiers are based upon the principle of a small input signal precisely controlling the flow of an external source of finite energy (the power supply) so that variations in signal amplitude at the input of the device are faithfully reproduced in proportionately greater amplitude at the output, at least up to a point, or, more precisely, two points. One of these points occurs when the peak value of an input signal causes maximal current to flow through the device. The amplifier is then said to be "in saturation," and input signals of greater amplitude cannot produce greater output signal amplitude because maximal current is already flowing. The second point occurs when the peak amplitude of a signal of opposite polarity causes minimal current to flow through the device. The amplifier is then said to be operating "at cutoff," and input signals of greater amplitude cannot produce a further change in output amplitude because minimal current is already flowing.

Thus, limiting may occur at saturation, at cutoff, or at both, a distinction seemingly lost on most authors, who invariably use only the term "saturation." The effect of such limiting is instantaneous truncation of the amplitude of one, the other, or both peaks of the output signal. The result, whether mapped on a graphical plot such as the dynamic transfer characteristic or displayed visually on an oscilloscope, we recognize as "clipping." The more the signal exceeds the limits imposed by saturation or by cutoff, the more its waveform is changed, and the greater the magnitude of the distortion products generated.

Asymmetrical peak clipping occurs when a signal peak of either polarity is distorted as a result of limiting by saturation or by cutoff. Symmetrical peak clipping occurs when peaks of both polarities are distorted. Asymmetrical clipping, as used by Hawkins and Naidoo (1993) in their experiment, gives rise to distortion products at both even and odd multiples of the fundamental frequency, not simply at odd multiples, as these authors state (p. 227). The generation of distortion products only at odd multiples of the fundamental is characteristic of symmetrical clipping.

In distinct contrast to limiting by saturation, limiting by compression is a time-dependent process that, according to hearing aid lore, is virtually distortionless. In fact, compressor circuit time constants cause significant distortion of the waveform whenever the gain is changed.

While the graphed input/output functions associated with circuits that limit by saturation or compression may, as in the Hawkins and Naidoo (1993) article, appear to resemble one another closely, the similarity ends there. Not only do the two forms of limiting involve very different physical processes, but they give rise to very different psychoacoustic perceptions, as the research of these authors, and others, amply demonstrates. Against this background, it is puzzling why anyone might choose to describe the two processes with a single term.

In a related vein, if taken literally and, again, if viewed in the context above, Fabry's (1993) assertion that output-compression instruments do not produce substantial amounts of distortion when in saturation is untenable. In a typical compressor limiting circuit, output signal levels that exceed a specified magnitude, often called the threshold of compression, are detected (in the strict, technical sense of the word) and converted into control voltages. These, in turn, reduce the gain of the circuit throughout a wide span of input signal levels, in effect preventing, or at least delaying, the onset of saturation. However, once the signal level exceeds the limits of the range of compression, saturation (or cutoff) occurs, with its inherent associated distortion, just as in any other amplifier.

The point that I have tried to make is, of course, that compression and saturation are two inherently different processes, and that the terms representing them are neither interchangeable nor synonymous. Lumping the two processes under a common term serves no useful purpose and invites confusion. If we are to be taken seriously by and communicate effectively with the engineering community, it is important that we take care to use established technical terms with precision.

S. Joseph Barry
Augusta, Georgia

Fabry DA. (1993). A clinical procedure described for evaluating compression hearing aids. *Hearing J* 46:25-30.

Hawkins DB, Naidoo SV. (1993). Comparison of sound quality and clarity with asymmetrical peak clipping and output limiting compression. *J Am Acad Audiol* 4:221-228.

Response to Dr. Barry

Dr. Barry is technically correct in his argument concerning the term "saturation." We have both used the term, perhaps inappropri-

ately, in a more generic sense (i.e., a circuit is "saturated" when the output remains constant with increases in input). With a limiter compressor such as the one used in the Hawkins and Naidoo (1993) article, hearing aid output remains essentially constant when the circuit departs from linear gain and enters compression. Webster's *New World Dictionary* (College Edition) defines saturation thus: "to be completely filled, charged, or treated with something else that no more can be taken up." In our minds, although not the engineering definition, the term saturation adequately describes what happens in both peak-clipping and output-limiting compression when maximum output is reached.

With regards to Dr. Barry's comment about compression hearing aids producing distortion, it was certainly not our intention to imply that compression hearing aids *never* produce harmonic or intermodulation distortion. The distinction was made in the context of distortion levels present when peak-clipping and compression circuits were driven at comparable input levels with similar SSPL90s. The bottom line remains that when all other factors are equivalent, peak-clipping devices produce more harmonic distortion than output compression when limiting occurs. The issue of *temporal distortion,* however, is an entirely different matter not appropriate to this discussion.

Our main reaction to Dr. Barry's letter is that while we acknowledge that precision in terminology is important, we believe his example is one of minor importance. We hope our lack of precision does not, as Dr. Barry implies, prevent our profession from being taken seriously.

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David A. Fabry
Rochester, New York

Barry SJ. (1994). Inappropriate use of term "saturation." *J Am Acad Audiol* 5: 356–357 (letter).

Hawkins DB, Naidoo SV. (1993). Comparison of sound quality and clarity with asymmetrical peak clipping and output limiting compression. *J Am Acad Audiol* 4: 221–228.