causing an inability of the epiglottis to retroflex adequately during deglutition. The epiglottis then acted as an impediment to swallowing, instead of moving out of the way of the food bolus. Cervical osteophytes were one cause of pharyngeal crowding. However, pharyngeal crowding was also seen after anterior cervical spine surgery due to an increase in pharyngeal wall thickness. We therefore included both groups under “cervical spine pathology.”

Dysphagia after anterior cervical spine surgery may occur from a variety of factors, including disruption of the pharyngeal plexus, scarring of the fascial planes, and prominent hardware. However, a subset of these patients continues to have symptomatic dysphagia due to epiglottic dysfunction despite swallow therapy and an adequate time given for healing, with no therapeutic alternatives. Thus, for patients with food bolus obstruction due to epiglottic dysfunction from pharyngeal crowding and with minimal to no laryngeal penetration during the swallow, partial epiglottoplasty is a low-risk alternative to anterior cervical spine surgery.

In regard to the level of the osteophytes causing epiglottic dysfunction, these are present at the level of the epiglottis and are evident upon review of the modified barium swallow study or fiberoptic endoscopic evaluation of swallowing (Figure 1 in article1). Certainly, osteophytes may cause dysphagia without causing epiglottic dysfunction. These cervical osteophytes and those that are located lower, around the level of the cricoid cartilage, are better treated with an open resection of the osteophytes. With regard to those osteophytes causing epiglottic dysfunction, however—based on our personal, unpublished observations—the size and number of osteophytes do not appear to be as important as the functional impairment of the epiglottis due to them. Thus, we carefully review the preoperative swallow study and focus on the functional impairment caused by osteophytes at the level of the epiglottis. If the osteophytes do not cause epiglottic dysfunction (or dysphagia in some other way), then they do not need treatment, no matter their size or number.

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A Revision in Evaluating the Results of Intratympanic Otoprotective Injections against Cisplatin-Induced Ototoxicity
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This letter is written in response to the article entitled “Cisplatin-Induced Ototoxicity and the Effects of Intratympanic Diltiazem in a Mouse Model,” by Naples and Parham.1 Recently, some researchers have focused on evaluating the effectiveness of intratympanic injections of agents such as N-acetyl cysteine, D-l-methionine, lactate, vitamin C, and diltiazem against cisplatin-induced ototoxicity in patients receiving chemotherapy. The possibility of cytokine-related mechanism of this side effect has evolved researchers to also try intratympanic injections of corticosteroids such as dexamethasone and betamethasone.2-5 Here, I want to discuss the methodological shortcomings of such studies and to criticize their design.

First, almost all of these studies usually use 1 of the 2 ears as the control and the other as the case to compare the otoprotective effect of the injections before and after the course of chemotherapy with cisplatin. Although this design seems logical and the idea is praiseworthy, some evidence implies that the ototoxicity of cisplatin may not necessarily be the same bilaterally; therefore, it is not comparable in 2 ears as case and control.4

Second, the last audiodiagnostic test in such studies is usually done immediately or a short while after the last injection of the otoprotective agent, while there is evidence that cisplatin-induced ototoxicity can occur a long time after the last dose of cisplatin. As Peleva et al showed a rate of 70% ototoxicity in a 60-month follow-up, it was 48% immediately after the last dose of cisplatin.5

Third, pure tone audiometry and distortion product otoacoustic emission, which are the most common audiodiagnostic tests used in such studies, are able to detect changes in only a limited spectrum of frequencies, while there is evidence showing that cisplatin-induced ototoxicity may occur in very high frequencies, something between 10 and 20 kHz, which needs special tests, such as extended high-frequency pure-tone audiometry. For example, in one study in 2014, 7 patients out of 10 showed ototoxic changes after receiving cisplatin, from whom 4 patients
showed significant changes in frequencies >8 kHz that were not detected with the routine pure tone audiometry and distortion product otoacoustic emission audiodiagnostic tests.4

In conclusion, although the idea of such studies about the otoprotective effect of intratympanic injections against cisplatin-induced ototoxicity seems magnificent and deserves more modifications, a careful revision in evaluating their results and moving them forward from animal models to human studies is suggested.

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Response to “A Revision in Evaluating the Results of Intratympanic Otoprotective Injections against Cisplatin-Induced Ototoxicity”

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No sponsorships or competing interests have been disclosed for this article.

We thank you for your interest and comments regarding our article “Cisplatin-Induced Ototoxicity and the Effects of Intratympanic Diltiazem in a Mouse Model.” We are grateful for the opportunity to address your comments. We acknowledge some of the shortcomings of working with animal models, and we share the objective of refining animal models of cisplatin ototoxicity.

The first point that you suggest is that cisplatin ototoxicity is not the same bilaterally, thus making comparison between a treatment ear and a control ear of the same mouse unreliable. We are aware that cisplatin may not have the same ototoxic effect on each ear. We were able to account for this in the key portion of our study (phase II), where otoprotective effects of intratympanic diltiazem and saline controls were evaluated in separate animals, not in contralateral ears.

Second, you raise the point that that the ototoxic effects of cisplatin can occur long after the last dose is administered. Many notable cisplatin animal model studies do not evaluate its long-term effects. In fact, we are one of the few laboratories that has explored the effects of cisplatin on audiodiagnostic testing over an extended period. Our laboratory’s experience has shown that in a mouse model, ototoxic effects from cisplatin stabilize after 3 weeks, and audiodiagnostic evaluations carried out up to 90 days do not show progression of ototoxicity.3,4

Finally, you discuss the concept that traditional audiometric measures, such as pure tone audiometry and distortion product otoacoustic emissions, may not be adequate measures of cisplatin ototoxicity, as the high frequencies are often more sensitive to the effects of cisplatin. We certainly agree with this point. We recognize the limitations of traditional audiometric measurements, and we are currently evaluating the utility of an outer hair cell–specific protein, prestin, as an otologic biomarker that has the potential to act as early indicator of inner ear damage that presents before audiometric changes.5 We anticipate that this current work will afford new, more sensitive techniques to detect effects of inner ear damage.

In conclusion, we agree that there is more work that needs to be performed, and ultimately, we hope to have the opportunity to translate some of our work to the clinical setting. This is a clinical problem that has remained unsolved, and one of our goals with this work was to demonstrate that novel therapeutic agents such as diltiazem may provide a paradigm shift in approach to potential therapies. Although the mouse model has certain limitations, it provides a medium through which novel concepts can be evaluated. We look forward to contributing more to this topic in the future.

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