LETTERS TO THE EDITOR

Validity of Risks of Complication Following Thyroidectomy

To the Editor:—The recent publication of the paper entitled "Risks of Complication Following Thyroidectomy," by Burge et al.,¹ raises interesting and perhaps distressing issues related to the media environment surrounding current clinical research and the quality of this research. This paper's conclusion is that "patients undergoing thyroidectomy by an otolaryngologist may be at a higher risk of permanent postoperative hypoparathyroidism than patients who undergo thyroidectomy by a general surgeon."

Several years ago Dr. Burge studied the outcomes of thyroidectomies done by general surgeons and otolaryngologists operating at three hospitals in Albuquerque, New Mexico. The results were published in an abstract entitled "Choice of Surgical Specialty Determines the Risk of Persistent Post-Thyroidectomy Hypoparathyroidism," coauthored with Michael W. Johnsen, PhD, and presented at the February 1996 Western Regional Meeting of the American Federation for Clinical Research.²

Unfortunately, *Physician's Weekly* picked up the publication of this abstract and stated in a front page article that "thyroidectomies done by Albuquerque otolaryngologists were more likely to result in postop hypoparathyroidism than those done by general surgeons." This statement is incorrect and was occasioned by errors in the design of the study, in data analysis, and in the wording of the abstract. Complaints were therefore lodged with Dr. Burge about the nature of this work and the study was revised and somewhat enlarged. However, significant errors in study design and in the interpretation of data persist in the article published by the *Journal of General Internal Medicine*. In Table 2, for example, the relative numbers of partial thyroidectomies done by otolaryngologists and general surgeons are not stated, even though this is relevant to the development of postoperative hypoparathyroidism. Furthermore, 90% of the patients treated by otolaryngologists had thyroid cancer compared with 68% of the patients treated by general surgeons and complications are more likely to develop in patients with advanced cancer. Most important, as reviews of this article published previously have noted,^{3,4} no data are presented on the relative experience of the two groups of surgeons studied with respect to thyroidectomy procedures. Therefore, the design of the study by Burge et al. is seriously flawed because the type of surgery done and the types of patients treated by the otolaryngologists and general surgeons involved in the study differ and there is no allowance made for the relative experience of the surgeon.

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It is obvious that an enormous undertaking would be required to demonstrate that the rate of persistent hypoparathyroidism after thyroidectomy for the entire population of otolaryngologists is greater than the rate for the entire population of general surgeons. Large and diverse study populations would be necessary for this research. In fact, Dr. Burge pursued a more simplified line of investigation, which entailed a retrospective comparison of the outcomes of thyroidectomies done by general surgeons and otolaryngologists who were direct competitors in a restricted geographic area. During the course of this study, Dr. Burge abandoned the scientific objectivity that is essential to good research. The title of the original abstract related to this work testifies to this lack of objectivity since the data in the abstract could not possibly support such a conclusion. In the present article the scope of the study has been expanded and the conclusions are less definitive and far-reaching, but the lack of objectivity persists. For example, Figure 3, entitled "Postulated chain of events leading to persistent postoperative hypoparathyroidism," implies that referral to an otolaryngologist is part of the causative chain of events leading to persistent postoperative hypoparathyroidism. Therefore, while the work is set in a seemingly more reasonable light, the design of the study by Burge et al. remains questionable and the principal conclusion is not justified by the limited data on two restricted populations of surgeons.

Differences of opinion and scientific controversy play an important role in our quest to improve the understanding of disease states and the management of affected patients. However, there is a point when controversial opinions based on poor science cross the line of propriety and become irresponsible. The methodology of this paper by Burge et al. is so seriously flawed that the conclusion it states is not merely controversial but rather plainly irresponsible. It would appear, therefore, that the paper by Burge et al., far from advancing knowledge, has in fact needlessly maligned an entire surgical speciality. This most unfortunate outcome might have been prevented by truly rigorous scientific review.—**FRED HERZON, MD**, Chief, Division of Head & Neck and Reconstructive Surgery, University of New Mexico Health Sciences Center, Albuquerque.

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2. Johnsen MW, Burge MR. Choice of surgical specialty for thyroidectomy partially determines the risk for post-operative hypoparathyroidism. *J Invest Med*. 1996;44(suppl 1):104A. Abstract.
3. Piccirillo JF. Know thy surgeon: the importance of surgeon-specific information. *J Gen Intern Med*. 1998;13:58-9. Editorial.
4. Sherman SI. The risks of thyroidectomy: words of caution for referring physicians. *J Gen Intern Med*. 1998;13:60-1. Editorial.

In reply:—Dr. Herzon believes that our recently published retrospective analysis of risk factors for persistent postthyroidectomy hypoparathyroidism¹ is seriously flawed and not deserving of publication. Although his criticism is not without merit, it is important to note that all of the study limitations mentioned by Dr. Herzon were discussed in the published article. Specifically,

the rationale for including results from subtotal thyroidectomies is detailed on page 26, and the fact that the difference in the occurrence of postoperative hypoparathyroidism persists between general surgery and otolaryngology when subtotal thyroidectomies are removed from the analysis is discussed on page 27. Similarly, the finding of a persistent difference between the surgical specialties in this outcome variable when only patients with thyroid carcinoma were analyzed is discussed on page 28. The lack of data addressing surgical experience in the study is discussed on page 30.

Dr. Herzon's assertion that there was a lack of objectivity in the design, execution, and analysis of data from this study constitutes a serious charge. Although the rationale behind this charge is not entirely clear, it seems that the bulk of his complaint stems from the publication of the preliminary study findings in abstract form two years ago,² and the subsequent capsule review of this abstract in a lay publication (*Tripping over the little neighbor*. Physician's Weekly, March 4, 1996, Vol XIII (9):1). I agree that the dissemination of preliminary and incomplete information regarding this study by the lay press is regrettable, but the abstract itself accurately describes the status of the study at that time and was deserving of presentation at the Western Regional Meeting of the American Federation for Medical Research. This study was motivated by a desire to explore factors that may affect the occurrence of postoperative hypoparathyroidism in endocrinology practice. As patients with permanent hypoparathyroidism require lifelong medical therapy and careful management, such a study is appropriate and ultimately aimed at improving patient care. The fact that Dr. Herzon takes issue with the conclusions of both the published abstract and the full paper mainly reflects disagreement over interpretation of the study findings rather than the findings themselves. Moreover, the inclusion of "Otolaryngology" in the postulated chain of events leading to persistent postoperative hypoparathyroidism (Figure 3) stems directly from the study data. Although I believe the conclusions of this study are balanced and supported by the data, Dr. Herzon is entitled to his interpretation of the data and his opinion on the merits of the study. The charge of bias and the insinuation that the findings of the study were somehow preordained, however, is frivolous, unsubstantiated, and contrary to the spirit of peer review.

Although it is not the author's responsibility to defend the peer review process for any scientific journal, it is appropriate to comment on the rigorous review received by this study because the initial review stimulated further studies that ultimately improved the quality of the published report. The initial manuscript describing this study was submitted to the *Journal of General Internal Medicine* in February 1996. The initial review by the *Journal* conveyed interest in the manuscript but expressed several

methodologic and statistical concerns. As a result, the study was expanded by adding a fourth hospital to the analysis (substantially increasing the study sample size), by revisiting all of the original study records to collect additional data requested by the reviewers, and by enlisting the aid of a biostatistical consultant (Dr. Qualls), who ultimately served as senior author on the manuscript. After submission of the revised manuscript in April 1997, reviewers again requested substantial revision and clarification of several statistical points and data tables. Only after these two revisions were completed was the manuscript accepted for publication in September 1997. Despite a significant expansion in the size of the study and the depth and breadth of the study subject matter, and despite the additional analyses requested by the reviewers, the main study findings and conclusions remained unchanged. Upon publication, the study report was accompanied by two separate editorials (authored by an otolaryngologist and an oncologist) who offered further possible interpretations of the study.^{3,4}

Finally, no single study is flawless or definitive. Medical knowledge is ultimately increased only by posing appropriate questions and by attempting to address them as completely as possible given the available resources. The fact that certain avenues of medical research are difficult to pursue, controversial, or likely to result only in partial answers should not hinder attempts to ask difficult questions. Furthermore, physicians must be willing to evaluate themselves and their performance, to reevaluate their methods, and to consider the possibility for improvement. Only through such rigorous assessment will patient care improve. Nevertheless, only a large body of scientific inquiry, obtained over time and over a broad demographic range, can suitably answer questions that pertain directly to patient care. Continued investigation into the outcomes of thyroid surgery will ultimately result in consensus over the proper care of patients with thyroid disease.—**MARK R. BURGE, MD**, Department of Medicine, University of New Mexico Health Sciences Center, Albuquerque.

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1. Burge MR, Zeise TM, Johnsen MW, Conway MJ, Qualls CR. Risks of complication following thyroidectomy. *J Gen Intern Med*. 1998; 13:24-31.
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