

Letters to the Editor

Thyroid papillary carcinoma in lateral neck cyst: missed primary tumour or ectopic thyroid carcinoma within a branchial cyst? *J. Laryngol Otol* 2000; 114: 716–9.

Dear Sirs,

I read with great interest the report by Sidhu *et al.*¹ which described a papillary thyroid carcinoma arising in a branchial cyst. The authors indicate five points which show that their diagnosis is correct. I disagree with each of these “proofs” as follows

1. “The fine needle aspirate yielded cholesterol crystals and foamy macrophages typical of a branchial cyst. Fine needle aspiration of a lymph node replaced by papillary thyroid carcinoma characteristically yields dark brown fluid.” In my experience **cystic** papillary carcinomas both in the thyroid and in cystic nodal metastases can contain cholesterol crystals and macrophages. This is a reflection of old haemorrhage into the cyst.^{2,3}

2. “The cyst lining consisted of an epithelial layer and a subepithelial collection of lymphoid tissue typical of a branchial cyst.” I suggest rather that the lymphoid tissue represented lymph node. No mention is made of the presence or absence of nodal architecture, such as a subcapsular sinus; admittedly if a lymph node is totally or almost totally replaced by tumour, underlying architecture may be difficult to discern. However, if lymphoid tissue was present careful examination should be helpful in defining whether it was a lymph node.

The cyst lining was columnar and not squamous or respiratory. I suggest that the columnar lining is part of the thyroid tumour which became cystic. Indeed this epithelium did stain for thyroglobulin.

3. “Normal thyroid was found within the cyst wall adjacent to the focus of papillary carcinoma.” Metastatic papillary carcinoma can show normal appearing thyroid follicles in nodes. This is not “normal” thyroid but very well differentiated thyroid carcinoma. Did the authors perform immunostains for high molecular weight cytokeratin which may help if positive to define this “normal thyroid” as cancer.⁴

4. “There was no evidence of carcinoma in the total thyroidectomy specimen which was embedded and “examined at 1 mm cuts”. There was no connection noted between the cystic neck mass and the pharynx or hyoid bone.” In my experience, the primary tumour in the thyroid in patients such as the one described can be submillimeter in size. Alternatively, the primary may have regressed so that only an area of scar or fibrous tissue remains. Without serial sectioning of all the blocks of the totally embedded thyroid the authors cannot be sure there was no thyroid primary.

5. “The other possibility was a laterally placed thyroglossal duct cyst.” This explanation is not valid since the lining of a thyroglossal cyst is respiratory and/or squamous and the thyroglossal duct cyst should not contain lymphoid tissue.

In summary, I believe that the diagnosis in this reports is open to question. I fear that such reports might lead to inadequate therapy of patients presenting as in this case with a cystic lateral neck mass containing papillary cancer. Hence no thyroidectomy would be performed if the gland were clinically and radiologically without nodules.

The five-year survival in this patient is not unexpected since appropriate therapy was given (total thyroidectomy); in fact he may be cured. However, five years may be insufficient follow-up. I have personally seen a patient who had a cystic papillary carcinoma metastasis in a lateral cervical lymph node. No thyroidectomy was undertaken because no lesions were identified in the gland. Nine years later recurrent cervical node metastases were found. Total thyroidectomy disclosed two separate papillary microcarcinomas (1 mm and <1 mm in size). Hence, very small papillary carcinomas can metastasize, be unrecognizable in the thyroid gland and be very slowly growing. The intriguing biological question is why this happens in some patients.

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Author's reply

Thank you for the opportunity to respond to the letter from Professor LiVolsi. She raises important issues which we felt we had addressed in the original manuscript which however, require reiteration.

1. Although FNA of a cystic papillary carcinoma may give rise to cholesterol crystals and foamy macrophages, this finding is more typical of a branchial cyst.

2. The subepithelial lymphoid tissue in our cyst wall had no architecture suggestive of a lymph gland. We do acknowledge that the lining of our cyst could represent either a branchial cyst or cystic degeneration in a lymph node replaced by papillary thyroid carcinoma. Indeed, the lining epithelium stained positive for thyroglobulin, which is suggestive that the latter had occurred. However, as we had referenced, this phenomenon has been reported before by Parham¹ in typical branchial cysts. He attributed this phenomenon to persistent remnants of branchial pouches 4/5 which had failed to fuse with the descending median thyroid anlage. The lateral contribution of branchial pouches 4/5 to thyroid development is being given increasing embryological and surgical importance in the thyroid literature as evidenced by the increasing number of articles published regarding the importance of the Lobe of Zuckerkandl in thyroid surgery.^{2–4} The 4th and 5th pouches, once they fuse with the median thyroid component, give rise to this lobe, seen in more than 60 per

cent of surgical series when looked for by an experienced thyroid surgeon. Increasingly, it is being used as a guide to safe identification of the recurrent laryngeal nerve.²⁻⁴

3. One of the great concerns facing all clinicians and pathologists is errors which can occur in the day to day management of patients and reporting of pathological specimens. We acknowledged in our manuscript that despite serial sectioning at 1 mm cuts, of embedded thyroid tissue, a primary focus of papillary carcinoma may have been missed. Accordingly, the patient was appropriately counselled and proceeded to have an uneventful total thyroidectomy.

We take this opportunity to allay Professor LiVolsi's fears and repeat that we are not advocating conservative management in this scenario. The present management was total thyroidectomy and will continue to be so. However, one should not be so entrenched in personal dogma and anecdotal evidence so as to be unwillingly to embrace alternative ideas. We have sought to examine an old problem in a new light, using cumulative evidence spanning several decades, while still adhering to established surgical principles. In doing so, we have not provided a rigid viewpoint, but have reviewed current evidence, so as to generate healthy debate. We believe we have been successful in this endeavour.

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Quality issues in otorhinology: Part I *J. Laryngol Otol* 2000; **114**: 817-20.

Dear Sirs,

We read with interest Prof. Browning's views on quality issues in training in otolaryngology.¹ As expected, he raised many points worthy of discussion.

We particularly note the assertions about the factors influencing trainers' 'informal' assessment of trainees. The mention of gender in this context is particularly disturbing and we wonder what evidence lies behind its inclusion. The authors have not made it clear whether the gender of the trainer or the trainee has the effect. The implication is that

a trainee's competence might be over estimated, but this is not clearly stated. We feel it would be constructive for the authors to clarify these points.

The article attempts to make a distinction between 'informal' assessment by trainers on a day to day basis, and the Specialty Examination, which is subject to external observation and other checks. The inference is that only the first is subject to any influence of gender – but if gender has an effect in one situation then how can we be sure it does not in another?

In the near future, the existing male domination of Otolaryngology will change. Women will soon make up more than 65 per cent of medical graduates. It is crucial to maintaining the quality of trainees and consultants of the future, that they are not discouraged from pursuing the surgical specialties. We fear that the suggestion that gender may influence assessment might be discouraging, especially if made as a 'throw-away' comment without evidence to back it up. We do note however that the article seems to suggest the 'gender effect' may work in favour of women!

We feel that the possibility of gender having an influence on the assessment of trainees should be subject to more open discussion, and that trainers and examiners should be made aware of this potential problem.

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References

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Author's reply

In response to Miss Richardson and Dr Redfern's letter, our editorial was just that – personal observations of the authors on quality issues in ORL training. Whilst we are sure there is ample literature on gender bias (not 'problem' as in your correspondents' letter) in the selection and assessment of trainees, we were simply observing that it was one of several factors that interplayed in ORL. Experience would suggest that on some occasions gender bias can be detrimental and positive in others. However we agree with your correspondents that gender bias, as all other types of bias should be more clearly identified and stated.

Overall, ORL has an excellent track record of encouraging female graduates into the specialty; few others could boast of having two female professors (25 per cent of the total) both of whom are extremely dynamic and are or have been on the Councils of a Royal College of Surgeons.

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