Letters to the Editor

A Christmas thought from Professor NA Wright: HLA-DR expression in liver disease

It was with the greatest consternation that I detected, in the recent paper by Barbatis *et al*¹ a reference to a practice totally abhorrent to any right thinking pathologist, let alone a member of the Association of Clinical Pathologists: the introduction plainly refers, "in cases of liver transplantation rejection ... focal HLA-DR was identified on bile duct epithelium." Although the reference given is to an Italian work, the implication is plain: people who eat transplanted livers express class II antigens on their bile duct epithelium. Before reinforcing my disgust at such practices, my natural curiosity prompts several questions of the authors: who is doing the eating, and whose transplanted livers are involved?

Can it be (nay!) that health professionals, jaded by paté de foie, truffled hare terrine, and salmi of partridge are rousing their gastronomic accidia by consuming resected or post mortem transplanted livers? If so, can we be enfranchised of the recipe? Personally, I prefer calf's liver barbecued with fresh sage leaves, but perhaps the presence of a rejecting lymphoid infiltrate adds a certain piquancy to the culinary occasion? Are the authors suggesting a new cooking method, buried in the text among the intricacies of the three stage immunoperoxidase technique, which I am unable to discern? On the other hand, however great the delicacy, I would not be prepared to sample it if, as the authors suggest, my bile duct epithelium would be prepared for immunological attack. Alternatively, are the authors alluding to a revolutionary concept, that autophagy of a patient's transplanted liver, of necessity obtained at biopsy, leads to expression of class II antigens on his bile duct epithelium?

Both these proposals of course require a mechanism; my meagre knowledge of immunology indicates that gamma interferon is responsible for the induction of HLA-DR expression on epithelial cells. Can we safely assume that absorption from the gut of proteins from transplanted liver leads to the (local) release of gamma interferon close to bile duct epithelium? My worry then becomes—is this phenomenon confined to transplanted tissue, or must we eschew our indulgence in all hepatic parenchyma?

I hope you realise, Dr Barbatis, that you and your colleagues have raised genuine shock, horror, and major doubts about the safety of such eating practices. Perhaps this letter might persuade you to look further

into these startling claims, so as to set our minds at rest.

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Reference

- 1 Barbatis C, Kelly P, Greveson J, Heryet A, McGee J O'D. *J Clin Pathol* 1987;40:879-84.

Isolated arteritis of the ovarian hilum

The histological and immunohistological features and the anatomical location of arteritis in a unique case of necrotising arteritis apparently confirmed to the ovarian hilum

suggests that this is the female counterpart of isolated arteritis of the epididymis.¹

A 36 year old woman had undergone hysterectomy for menorrhagia some two years previously and had since been complaining of intermittent pelvic pain. She had an unremarkable obstetric history (para 4 + 0), had had no serious illnesses, was taking no medication, and was in good general health. At laparoscopy, a left tubal ovarian mass with adhesions was noted and a vaginal vault granulation was diathermied. The patient was keen to have her ovaries removed and a bilateral salpingo-oophorectomy was performed three days after the laparoscopy. The operation was uncomplicated and the patient made an uneventful postoperative recovery.

In routine histological sections of the right ovary there was a focus of necrotising vasculitis which was apparently confined to two or three small muscular arterial branches close to the rete ovarii. The arteritic lesions were characterised by focal fibrinoid necrosis

Fig 1: Ovarian hilar arteritis. Acute arteritis with fibrinoid necrosis and inflammation affecting part of the circumference of a small muscular arterial branch.