Intraoperative pulmonary tumour embolism during hepatectomy

To the Editor:

We were interested in the case of hepatic tumour embolism reported by Blanloeil *et al.*,¹ and their comment that it had not been previously reported to have occurred intraoperatively.

We would like to report a case of a nine-month-old infant who presented with a large hepatoblastoma. The patient was anaesthetized and cooled to 32° C, which is our usual practice for such operations. The operation proceeded satisfactorily with blood loss being replaced. The right hepatic artery, portal vein and hepatic duct were divided. During the dissection of the hepatic vein about 100 mls of blood was lost which was rapidly replaced. Shortly afterwards the blood pressure declined, hypoxia developed (PaO₂ 30 mmHg) and cardiovascular collapse occurred. Despite further blood transfusion and resuscitation for over an hour the patient failed to respond and died. At post-mortem multiple, large tumour emboli were found throughout the pulmonary vascular tree.

During hepatic resections hypothermia provides some cerebral protection should cardiac output decline. This may result from blood loss or decreased venous return if inferior vena caval occlusion becomes necessary to repair vena caval perforation. Placing slings around the inferior vena cava is a helpful safeguard in the event of such difficulty. Although it might be useful if embolisation is considered likely it would be hard to detect emboli until it was too late as it was in our case.

If tumour has already invaded the vena cava then operation using cardiopulmonary bypass would be necessary. There was no evidence from preoperative radiological investigations or ultrasound that the inferior vena cava was involved in our patient nor was there evidence at postmortem of tumour spread into the vena cava. We have successfully operated with bypass on another child with Wilms tumour invading the vena cava. In that case the intravascular tumour was removed as a solid plug.

Fortunately massive tumour embolism during hepatic resection is rare, this being the only one we have had in a series of 46 patients.

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Naloxone and its antagonism of anaesthesia and analgesia

To the Editor:

The paper by Freye *et al.* is of interest,¹ but I feel certain statements and assumptions made by these authors should be challenged.

They state that the opiate antagonist naloxone (Nx) can antagonise certain non-opiate-induced effects, but do not qualify this statement by adding that these effects may be governed by Nx-sensitive endogenous opioid activity. They then go on to cite studies where non-opiate substances such as diazepam, alcohol and nitrous oxide have been shown to exhibit Nx-sensitive actions.

They infer from this that Nx sensitivity may not necessarily give an indication of opiate involvement. Although Nx sensitivity *per se* does not give unequivocal proof of opiate or opioid activity,² it is most suggestive particularly at low doses of Nx. The examples that these authors cite are particularly misleading. First they do not make a clear distinction between anaesthesia and analgesia, which is most important³ since different mechanisms may mediate these two states.⁴

Second, alcohol,⁵ diazepam⁵ (via a Gaba-ergic-opioid link)⁶ and nitrous oxide (at analgesic concentration) may well have strong functional links to the opioid system. Nitrous oxide particularly has been shown both in vitro^{7,8} and in many animal⁹⁻¹¹ and human¹²⁻¹⁴ studies to have opioid activity at analgesic concentrations. The work^{4,15-17} cited to bolster their argument particularly in the case of N₂O is also unfortunate, since all these "so called" contradictory papers measured the effect of Nx on anaesthetic and not analgesic concentrations.

In a study where analgesia caused by N₂O was not reversed by Nx,¹⁸ the experimental design had not made sufficient allowance for the pharmacokinetic differences exhibited by these agents.¹⁹

Although Nx may well have a non-specific analeptic effect,¹ it would be incorrect to assume that none of the effects associated with anaesthesia, such as analgesia are mediated via naloxone-sensitive opioid mechanisms.

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REPLY

We welcome the comments of Dr. Gillman as they stimulate the exchange of ideas in a fast-moving field such as opioid activity. We would like to respond to his comments by stressing the following points.

His statement that we did not refer to a possible endogenous release through non-opioid compounds is not correct. We stated: "Antagonism of endogenous opioids (enkephalins or endorphins) at opiate binding