set against the considerable reduction in PONV that has been demonstrated in those patients given ondansetron [3]. We agree that expensive antiemetic agents should be reserved for those most at risk and our study provided some evidence for targeting those with previous PONV after anaesthesia.

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Calcium channel inhibitors and perioperative myocardial ischaemia in cardiac surgery

Sir,—We read with great interest the paper by Ho, Parisi and Shragge [1] reporting a case of failure to wean from cardiopulmonary bypass after myocardial revascularization, treated successfully with intra-aortic injection of verapamil. The authors excluded hypoxaemia, anaemia and coronary insufficiency caused by inadequate revascularization. They came to the conclusion that the most convincing evidence suggesting that coronary spasm was the main cause of the profound ventricular failure was the rapid resolution of myocardial dysfunction and ischaemic changes immediately after injection of verapamil. We recently reported our experience in 12 similar patients and circumstances [2]. However, there are some aspects of the present paper which we would like to discuss further. As we stressed recently, when ventricular dysfunction and ischaemia occur in the absence of any technical or anatomical abnormalities, both coronary spasm and low reflow phenomenon should be considered. Low reflow phenomenon, or impairment of post-ischaemic coronary flow, is known to be at least endothelium-dependent [3]. Experimental data showed that potassium concentration in cardioplegic solution, in addition to ischaemia and reperfusion, is known to play a major role in endothelial damage, leading to a decrease in coronary flow [3, 4]. It is not known if some patients or animals are more sensitive to one or other of these factors.

Whatsoever the case, it has been shown clearly that low coronary reflow can be treated pharmacologically leading to enhanced post-ischaemic mechanical function. Furthermore, we found a positive correlation between enhancement of coronary flow during reperfusion and improved mechanical function [4].

Thus experimental data support strongly the clinical observation reported by Ho, Parisi and Shragge and us [1,2]. Nevertheless, we think that if a calcium channel inhibitor is used as the last resort to treat ventricular dysfunction and ischaemia after cardiac surgery in order to wean the patient from bypass, the drug preferred would be a less negative inotropic and more coronary selective drug [2].

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Morphine and droperidol combined in patient-controlled analgesia: reduction of nausea and vomiting

Sir,—Sharma and Davies [1] showed that the addition of droperidol to morphine given via a patient-controlled analgesia system reduced postoperative nausea and vomiting (PONV) in patients undergoing hysterectomy. We are surprised that despite concern about sedative and extrapyramidal side effects, sedation was not assessed more formally. In a similar study [2], we found that the addition of droperidol caused significantly greater sedation at 24 h. We used verbal rating scores by masked observers. None of our patients experienced extrapyramidal side effects, although they received a comparable mean dose of droperidol in the first 24 h (including a peroperative prophylactic dose of 2.5 mg).

In our study, patients received morphine as the sole analgesic agent, both during and after operation, and patients who experienced PONV within the first 1 h of surgery were not excluded as we felt this would not give the full picture of this common and distressing phenomenon. We demonstrated a reduction in the need for rescue antiemetic treatment from 59.3% to 31.0% over the 24-h period after surgery.

In their discussion, Sharma and Davies stated that "the mixture of morphine and droperidol appeared to be stable, as both drugs remained clinically effective and no precipitate was visible after 24 h". We wish to point out that the stability of morphine and droperidol mixed in plastic syringes has been confirmed using HPLC on samples over a 14-day period [3].

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Outliers and extrapolation: child's age and infusion rates of mivacurium

Sir,—Drs Mereotoja and Ollkola have misunderstood my worries about their work on child's age and infusion rates of mivacurium [1,2]. I was not suggesting that they should have removed the outlying point, but that the point made an important difference to the statistical interpretation of their results. Similarly, I was not suggesting that it is valid to predict by extrapolating beyond the measured range but, if extrapolation beyond a measured range does not make sense, then the results should, again, have to be interpreted differently. Extrapolating their line for 50% block gives an infusion rate of zero for maintenance of that block in patients aged 28 yr. This (if their gradient is the true one) is a mathematical fact and is a nonsense. Therefore, if there is a