Correspondence

Editor—Thank you for the opportunity to reply to Drs Ng and Lo. We found a hypercoagulable profile in one group of patients \(^1\) but whether it was gelatin- or stress-mediated was not demonstrated as no tests were carried out to test this hypothesis. However, there are some clues which suggest gelatin-mediated hypercoagulability: gelatin used \(\text{in vivo}^{2,3}\) increased erythrocyte aggregates in blood because of increased blood viscosity at a low shear rate, decrease in primary aggregation time and increased partial dissociation threshold. These findings were obtained in a randomized manner, immediately after haemodilution and before surgery (i.e. before tissue damage). In addition, 20% haemodilution with gelatin\(^4\) increased intrinsic coagulability and speed of clot formation when assessed using the thrombelastogram (TEG), as it decreased \(r\) and \(k\) and increased \(\alpha\) angle. This \(\text{in vitro}\) study, which excluded extraneous factors such as stress response and tissue damage, seems to favour a hypercoagulable effect of gelatin.

Furthermore, because of the different materials and methods, we believe that it is not possible to compare the studies cited by Ng and Lo and our own. In both \(\text{in vitro}\) studies, there were several methodological differences: (i) no adjustment of pH and calcium concentration occurred to prevent changes caused by haemodilution with plasma substitutes in the study by Mortier and colleagues\(^5\); (ii) a different low molecular weight heparin was administrated the evening before by Mortier and colleagues\(^5\) and Egli and colleagues\(^6\); (iii) coagulation in the cup was activated with celite by Egli and colleagues\(^6\) while we used native blood; and (iv) TEG analysis began 6 min after blood sampling, while our TEG analysis always began within 3 min to avoid clot activation in the syringe.\(^6\) Moreover, at least 30% haemodilution of blood volume was performed in both studies, while we replaced, at most, blood loss of 20% of total blood volume (approximately assumed to be 70 ml kg\(^{-1}\) in an adult).

The \(\text{in vitro}\) study by Mortelmans and colleagues excludes by its design any comparison with our results: body temperature, of prime importance in haemostasis assessment, was not given throughout the study; albumin was used as a plasma substitute in both groups in addition to the studied starch; and blood substitution, including surgical blood loss and acute normovolaemic haemodilution, approached 80% of total blood volume 4 h after the beginning of the study, which was much greater than in our study.

These differences, and the fact that \(\text{in vitro}\) studies poorly reproduce the multiple \(\text{in vivo}\) interactions leading to coagulation, suggest that no comparison can be made between the two sets of results.

Nevertheless, whatever the cause of this hypercoagulable trend after moderate haemodilution with gelatin, our results suggest that the use of this starch in patients known to suffer from a hypercoagulable state, or prone to thromboembolic disease, is not recommended until more data are available.

Metformin and perioperative risk

Editor—Lactic acidosis is a rare but well recognized complication of biguanide therapy, a treatment used commonly in the management of type 2 diabetes. Metformin-associated lactic acidosis (MALA) has an average case incidence of 0.03 per 1000 patient years, which is 10–20 times lower than that of phenformin (which was withdrawn from several countries for this reason in the 1970s).\(^1\) Although rare, MALA remains a serious yet potentially avoidable complication of metformin therapy with a mortality of 50%.\(^1\)

The mechanism whereby metformin causes lactic acidosis is complex but is thought to be mainly a result of a shift in the intracellular redox potential away from aerobic to anaerobic metabolism, leading to an increase in cellular lactate production.\(^2\) As metformin is excreted by the kidneys, renal impairment is the major risk factor precipitating MALA, although other risk factors such as sepsis, acute myocardial infarction, hepatic impairment and respiratory conditions leading to hypoxaemia are also important. Nephrotoxic drugs have been implicated.\(^3\)

Although surgery has never been identified as a specific cause of MALA, metformin-treated patients are at risk of

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This includes hypotension related to induction of anaesthesia or blood loss, and conditions such as myocardial ischaemia and sepsis, which are more common in diabetic patients in the perioperative period. This was highlighted by Mercker and colleagues in a middle-aged diabetic man treated with low-dose metformin 500 mg once daily.\(^4\) After abdominal wall hernia repair, he developed pneumonia, respiratory failure and acute renal failure. This resulted in severe lactic acidosis with a fatal outcome.\(^4\)

There is currently little information in the anaesthesia literature regarding the perioperative management of metformin-treated diabetic patients, although it has been suggested that metformin therapy should be withheld 2 days before surgery.\(^3\) This recommendation is not supported by the pharmacokinetics of this biguanide. Metformin has a short half-life of less than 5.0 h\(^1\) and in the presence of normal renal function, most is excreted in less than 12 h. Hence it may be justifiable that metformin be withdrawn only 24 h before surgery, specifically when general anaesthesia is required. This should limit the hazard of MALA without compromising glycaemic control. During the perioperative period, insulin is the conventional therapy for glycaemic control. After operation, when the patient has resumed full oral intake, metformin can be recommenced provided renal function has remained normal and there are no postoperative complications.

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4. Mercker SK, Maier C, Neumann G, Wulf H. Lactic acidosis is a serious perioperative complication of antidiabetic biguanide medication with metformin. Anesthesiology 1997; 87: 1003–5

Acupressure and prevention of nausea and vomiting

Editor—In their study on acupressure and prevention of nausea and vomiting, Harmon and colleagues\(^1\) highlighted the problem of control treatments in acupuncture and acupressure studies. They suggested that the standard control in acupuncture research is ‘sham’ acupuncture and then explain, quite rightly, why this should not be so. In their study they used sham acupressure. As they admit, they do not know the mechanism of action of acupressure. Could it not be possible that the sham acupressure is having an effect that might be promoting nausea in the control group? They do not mention the site of the sham acupressure. As they were applying the acupressure simultaneously with induction of anaesthesia and removing the bands before recovery, would it not have been better to have had a ‘no treatment’ control group?

I am also puzzled by the data in Table 4. Fifty-two patients were studied in each group but only 44 and 39 patients were scored for nausea in the two groups. Surely as the main aim of the study was to detect nausea and vomiting, all 52 patients should have been scored? If not, then the results are meaningless, if the authors’ power analysis is correct.

The final assertion that acupressure at the P6 point was effective in preventing nausea and vomiting after laparoscopy must be incorrect, as I do not think a 19% nausea and vomiting rate equates with prevention.

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Editor—Thank you for the opportunity to reply to Dr Coe. The use of a control in acupuncture studies is probably the most debated aspect of acupuncture research. Despite Dr Coe’s assertions, we do consider ‘sham acupuncture’ to be appropriate. Sham acupuncture may have a specific effect,\(^1\) particularly in analgesia research when point location is less important than in nausea and vomiting studies.\(^2\) However, in a letter, Lewith and Vincent have described sham acupuncture as a valid control in nausea and vomiting studies.\(^3\)

In our study, acupressure bands in the control group were placed on the dorsum of the right forearm. Alkaissi, Stalnert and Kalman\(^4\) would disagree with Dr Coe’s assertion that sham acupressure could be responsible for an increased incidence of vomiting. In their study they found no difference in vomiting between the sham acupressure group and a no treatment group. Acupressure, as described in our methods section, was applied before induction of anaesthesia. A ‘no treatment’ group would have prevented blinding of the study.

As described in our methods section, if a patient had both nausea and vomiting, this was scored as vomiting. This method of scoring creates a ‘nausea only’ group. Comparing nausea between the groups of patients who vomited was not included in this study. Power analysis was