CORRESPONDENCE

with error bars of 2 SD, the results may be seen in perspective (fig. 1, lower panel).

The investigators described repeat testing for differences between the groups by Student's unpaired t test or by Mann–Whitney U test, and stated that they then confirmed the differences by analysis of variance or by Kruskal–Wallis. However, it is invalid to test repeatedly by Student's or by Mann–Whitney tests. A difference between groups has first been shown by analysis of variance or by Kruskal–Wallis.

The investigators' final assertion is unproven. They imply that a patient who has already had a pressor response to cannulation may have an exaggerated response to a further stimulus, and that this is the reason that high risk patients should receive local anaesthesia before cannulation. The investigators did not describe subsequent laryngoscopy, so do not know that a response would be exaggerated. Patients truly at high risk would almost certainly receive some prophylactic treatment before laryngoscopy, and "will never be included in the control group of an outcome study examining the efficacy of haemodynamic control at... intubation!" [2].

N. W. GOODMAN
University Department of Anaesthesia
Southmead Hospital, Bristol


Sir,—In the paper by Langham and Harrison on the pressor response to insertion of an 18-gauge venous cannula [1], the authors concluded that there was a significant response to venous cannulation, as judged by changes in mean arterial pressure (MAP) and rate–pressure product (RPP), which was obliterated by prior infiltration with local anaesthetic; they recommended that lignocaine should be considered before insertion of an i.v. cannula. There are several aspects of this paper which we would like to discuss further.

First, we were not told in which arm cannulation took place. Presumably, it was into the arm opposite to that used for arterial pressure readings. Injection of local anaesthetic into the same arm might, theoretically, affect the neighbouring microvasculature and interfere with arterial pressure readings by digital plethysmography. Second, we were not told if the cannulation site was standardized. It is possible that bias could have occurred if Investigator 1 was free to choose a location for cannulation. The methodology described does not prevent Investigator 1 using a large antecubital vein for one group and a small ventral wrist vein for the other. Third, the reader is unable to determine if patient bias influenced outcome. What information was the patients given? Were they aware that they would receive either a single injection or two injections over what time scale? The study would have been more powerful if it had included a placebo group given saline before cannulation. Fourth, precisely which moment was taken as time zero? Was this at the beginning of cannulation (local infiltration), during or immediately after? If time zero was taken as the start of local infiltration, what was the pressor response to insertion of the 25-gauge needle? Finally, and most importantly, the authors state that there was no significant differences between the two groups in baseline readings of MAP or heart rate (HR). This is strictly true, but it might have been more reasonable to state that there were differences in baseline values for MAP and HR, but that these were not statistically significant. Analysis of baseline values in Langham and Harrison's table I using Student's t test reveals that, in the local infiltration group, MAP was 6.6% greater (P = 0.05–0.1) and HR was 10.7% greater (P = 0.1–0.05). However, the baseline RPP was 4.7% greater and this was statistically significant (P < 0.05)—a point which the authors did not discuss. We would like to suggest that this difference in baseline RPP undermines the conclusions of this paper.

J. BRIMACOMBE
G. CLARKE
P. TUCKER
Cairns Base Hospital
Cairns 4870, Australia


Sir,—Dr Goodman will be aware that the order in which statistical tests are performed has no influence on the interpretation of results. The differences were apparent when analysis of variance or Kruskal–Wallis were performed; the Student's unpaired t test and Mann–Whitney U test identified the times at which these differences occurred.

It is recognized that, despite random allocation, cardiovascular variables may differ when baseline recordings are made in two groups of patients. The technique of normalizing data by analysis of change from the baseline value is well recognized and this technique was used because of the difference in baseline values and not because of any intention to exaggerate differences between the groups.

Dr Brimacombe and his colleagues have drawn to our attention an omission in our paper; they stated correctly that site of cannulation was not noted. The cannulation site was standardized, being the dorsum of the non–dominant hand. The Finapres monitor was sited on the middle finger of the contralateral hand; any proposed effect on the "neighbouring microvasculature" of the local anaesthetic infiltration would therefore not be relevant. Consent was obtained from the patients on the night before surgery, at which time they were told that they may receive one or two "scratches" in the back of their hand. As clearly stated in the paper, time zero was taken as the time of the first stimulus to the patient, be it local infiltration or cannula insertion. The lack of a pressor response to the insertion of the 25-gauge needle is apparent from the results presented.

We agree with Dr Goodman that the most important reason to use local anaesthetic infiltration before venous cannulation is humanitarian and our previous studies [1, 2] had already addressed this issue. The fact that a measurable pressor response occurred in normal patients may imply that a clinically important response occurs, for example in those with pre-existing hypertension, and this is currently being studied.

B. T. LANGHAM
Glenfield General Hospital
Leicester

D. A. HARRISON
Royal Hallamshire Hospital
Sheffield


THE MANAGEMENT OF ACUTE POISONING

Sir,—We read with interest the review article by Collee and Hanson [1], but wish to raise a number of points. We would question the statements that "in unconscious patients organic brain damage should always be suspected if the history of poisoning is unsatisfactory and the depth of the coma does not improve within 12 h" and that "any patient whose level of consciousness appears inappropriately depressed for the history of the poisoning should be suspected of having an intracranial haemorrhage." In our experience, a satisfactory history of the poisoning in an unconscious patient is rare. In this situation it is essential to exclude a correctable metabolic or intracranial cause for the coma. Even in the presence of a good history, a high degree of suspicion should be maintained, intoxicated patients are often the victims of trauma. It is mandatory to exclude intracranial pathology before ascribing a depressed state of consciousness to the effects of drugs or alcohol. With appropriate neurological examination and the ready availability of CT scanning, we have the means to avoid repeating the lessons of the past [2].

From our experiences with intoxicated patients in North American hospitals, we recognize that the agitated, aggressive, stuporous or comatose intoxicated patient poses many problems in assessment and treatment. Under these circumstances an aggresive approach to patient management may be appropriate. Rapid sequence induction, tracheal intubation and control of
ventilation prevent the patient injuring themselves or others, reduces the chance of secondary brain injury, facilitates investigation (CT scanning), allows monitoring of vital signs and provides optimal conditions for any intervention. Second, we were surprised that the review did not mention the possible use of dantrolene in the management of hyperthermia associated with acute intoxication. The role of this drug in the treatment of hyperthermia secondary to intoxication has been underestimated in the past, but many recent reports of its use in this potentially fatal complication suggest that it may lead to improved outcome [3, 4].

Third, the list of cardiovascular complications of acute poisoning does not include myocardial ischaemia or infarction; these are serious omissions. Reports of more than 50 cases of acute myocardial infarction complicating poisoning have been published. Seventy percent of the victims had no significant cardiac history, the majority being young males who were regular users of cocaine [5]. Recognition of this potential complication has direct consequences on the choice of agents used to manage the cardiovascular complications of cocaine intoxication. Cocaine produces postsynaptic accumulation of catecholamines, resulting in increased sympathetic nervous system effects. Lignocaine is an illogical choice as an anti-arrhythmic in this situation: in common with cocaine, it is also a local anaesthetic and decreases the seizure threshold, and may precipitate convulsions. Propranolol is also an illogical choice; beta block alone may result in unopposed alpha adrenoceptor activity, thus worsening coronary vasoconstriction and hypertension [6]. There are two logical choices available for controlling the cardiovascular effects of cocaine toxicity: labetolol, which the authors did mention, and esmolol, a beta,-adrenoceptor (cardioselective) blocker; both have been reported as being of use in cocaine toxicity [7, 8].

Finally, we were confused by the advice given for the treatment of salicylate poisoning. The fluid regimen called for 1 litre of saline 1 mol litre⁻¹ or 0.3 mol litre⁻¹ every 4-6 h, but that one should not give more than 300 mmol of sodium per 24 h (sic). The initial instruction would result in the administration of 1000 mmol of sodium. One litre of 0.3 mol litre⁻¹ every 4-6 h would provide 1800–1200 mmol of sodium in 24 h, both prescriptions providing considerably more than the stipulated maximum dosage.

M. J. PARR
T. M. CRAFT
South Western Regional Health Authority
Plymouth and Bristol


Sir,—Thank you for the opportunity to reply to this letter. The critique of our article by Drs Parr and Craft is helpful, and we agree with several of the issues they raise. Certainly the exclusion of intracranial pathology in cases of acute coma is mandatory, but the circumstances in which an “aggressive approach” to patient management is justified are difficult to define precisely. Clearly, a full clinical assessment by appropriate specialists is preferable to a blanket policy of CT scanning.

Regarding the role of dantrolene in the management of hyperthermia following acute intoxication, we feel insufficient evidence exists at present to recommend its use. In addition to the two case reports cited in the letter from Drs Parr and Craft [1, 2], we discovered only two others before submitting our manuscript [3, 4]. Hyperthermia occurring after 3,4-methylenedioxymethamphetamine—‘Ecstasy’—may be related to its effect on central serotonergic and dopaminergic pathways, or to the long-term consequences of its abuse. The mechanism is not yet known. The prolonged exercise and dehydration resulting from all-night dancing suggest that, in many cases, hyperthermia after Ecstasy may resemble heatstroke in its pathogenesis. Such cases would not be expected to respond to dantrolene [5], nor do the central mechanisms provide an easy explanation of why dantrolene should be expected to be an effective treatment if neuromuscular blockers fail. In one report of its use [6], an initial dose of 1 mg kg⁻¹ failed to reduce the patient’s temperature, which remained increased until after a third dose. Dr Parr’s own case report of the use of dantrolene in the treatment of a fatal theophylline overdose remains (as far as we know) the only account of its use in hyperthermia of that aetiology. It seems prudent to wait further results. Meanwhile, we feel it more appropriate to emphasize the importance of general supportive measures.

We thank Drs Parr and Craft for pointing out the omission of myocardial ischaemia in the general section on the cardiovascular complications of acute poisoning. Regarding the management of arrhythmias secondary to cocaine poisoning, the role of beta, antagonists is undisputed. The fluid regimen in salicylate poisoning was converted to S.I. units in accordance with editorial guidelines, and it is indeed confusing. An erratum appeared in the August issue of British Journal of Anaesthesia (p. 331).

G. C. HANSON
G. G. COLLEE
Royal Free Hospital
London