HEPARIN CONTAMINATION OF SPECIMENS COLLECTED FROM ARTERIAL CANNULAE

Sir,—The investigation by Haynes and colleagues [1] of the accuracy of coagulation studies using arterial cannula samples raised the interesting question of contamination with heparin flush.

They cited Lew, Hutchinson and Lin [2], stating that accurate results may be obtainable when 5 ml of blood is withdrawn and discarded before sampling. Consequently, they withdrew 5.6 ml of blood before collecting the sample.

In unpublished observations made on cardiac surgical patients in theatre before heparinization, we found that, after the deadspace was discarded, sampling from a proximal port at 15 cm from the cannula necessitated 3 ml of blood to be wasted before achieving a “clean” sample. If blood was sampled at 150 cm from the cannula, deadspace plus 7 ml of blood had to be wasted to give an uncontaminated sample. We also used heparin 2 ml unit [1] in our flush; the coagulation tests in this small study were active partial thromboplastin time and thrombelastography.

In the light of Haynes and colleagues’ conclusion that “a blood sample from an arterial cannula may give clinically misleading information because of contamination with small amounts of heparin”, it would seem inevitable that contamination would occur if sampling was made at a distance of about 200 cm from the cannula. However, their point of sampling was not stated.

In our study, deadspace plus 7 ml of blood had to be wasted to give an uncontaminated sample. We also used heparin 2 ml unit [1] in our flush; the coagulation tests in this small study were active partial thromboplastin time and thrombelastography.

The misleading use of correlation coefficients in the comparison of two measurement techniques has been highlighted by Bland and Altman [2]. Regression analysis is used to predict one variable from another (i.e. y from x), whereas correlation measures the strength of a relation between two variables. This is not the same as agreement, nor does it describe accuracy.

Bland and Altman then described a method of measuring agreement between two measurement techniques. This involves using the average of the two measurements as the best estimate of the true value, and then obtaining the difference between this average and each of the measured values. A mean difference and SD are then obtained. These are the “bias” and “limits of agreement” of the measurement techniques.


Sir,—The method of estimation of PEEP was not a prime consideration of our study, but merits further comment. Paw,0 is a measurement of end-expiratory alveolar pressure under dynamic conditions, while Poc is measured under static conditions. To average the two variables to obtain the “best estimate of the true value”, as suggested, would be misleading. Nonetheless, the data have been examined by the method of Bland and Altman [1] and the two measurements are in “reasonable” agreement. The bias mean difference is 0.06 (so 0.17) kPa (fig. 1).

Poc (as a measure of whole-lung static PEEP) was used in our study to analyse the data of hyperinflation and effects of PEEPs as this is the standard method for the estimation of PEEP [2] and allows comparison with previous work. Poc has a high degree of reproducibility as the measurement is dependent on the resolution of the pressure transducer alone, whereas Paw,0 depends on the resolution of pressure, flow and the rate of data acquisition.


Effect of PEEP on Hyperinflation

Sir,—We congratulate Dr. Tan and colleagues on their well designed and informative study concerning the effects of PEEP in patients with obstructive airways disease [1].

While we have no argument with their findings or the conclusions drawn, we have noted a fundamental statistical error.

Two methods were used to measure intrinsic PEEP; these were airway pressure at the onset of inspiratory flow (Paw,0) and airway pressure obtained with expiratory port occlusion at end-expiration (Poc). The authors then calculated a correlation coefficient between the two variables and concluded they were “in reasonable agreement (r = 0.87)”. Having reached this conclusion, they referred to only one measurement (Poc) in further analyses.

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C. B. BERRY
P. S. MYLES
Alfred Hospital
Præhran, Victoria, Australia

CORRESPONDENCE

PERIPHERAL NERVE INJURY CAUSED BY INJECTION NEEDLES

Sir,—One reason for the study by Rice and McMahon [1] seems to be to question an earlier experimental study on the acute effects of needle point trauma by Selander, Dhunér and Lundborg [2]. When criticizing that study, Rice and McMahon unfortunately made inappropriate comparisons. They studied the amount and long-term pathology of injuries in the rat monofascicular sciatic nerve after penetrating it with long- (12°) or short- (27°) bevel injection needles, whereas Selander’s group aimed primarily at the frequency and acute morphology of nerve fascicle lesions after piercing the rabbit multifascicular sciatic nerve with a long- (14°) or short- (45°) bevel needle. The experiments by Selander, Dhunér and Lundborg [2] showed that the nerve fascicles easily slid or rolled away from the needle point both in vivo and in vitro, especially when the 45° bevel was used. This led to a significantly smaller frequency of fascicle injury after piercing the nerve with the 45° or short-bevel (SB) compared with the 14° or long-bevel (LB) needle.

When a needle hits a nerve, paresthesiae are elicited and the (awake) patient normally reacts to this. The paresthesia informs the anaesthetist that the needle is in close contact with the nerve and that, if advanced further, the needle may injure the nerve, and it seems that most anaesthetists agree with this view [3-6]. Selander, Edshage and Wölf [7] and Plevak, Linstromberg and Danielson [6] found that the use of a paresthesia searching technique for axillary plexus blocks (with long-bevel needles) increased the frequency of post-anaesthetic nerve injuries compared with a non-paresthesia technique. The use of short-bevel needles may press or push the nerve away, thereby giving the patient and the informed anaesthetist more time to react to a paresthesia before the needle penetrates and possibly injures the nerve.

Rice and McMahon [1] used a short-bevel needle with a bevel angle of only 27°. Their short-bevel needle was also 13° thicker than the 12° long-bevelled needle used. To injure the single fascicle of the rat sciatic nerve “the needle was introduced into the exposed nerve at mid thigh level at an angle of 45° and left undisturbed for 10 min”. This would not be possible unless the animal or patient was deeply asleep! It apparently implies that each nerve was severely injured by the needle, and under such unconventional conditions it may not be surprising that the 27° needle caused more severe lesions than the 12°. However, as seen in Table I of their paper, the acute lesion on day 1 was worst after the nerve was pierced with the transversely orientated long-bevel needle, as in Selander’s study.

In the histological study, something termed “intraneural disruption” was assessed and “this was defined as the degree of disruption to the internal elements of the nerve and included such factors as the reaction at the site of injury, the gliosis... scored on a 0-5 scale...”. Other morphological changes studied were “evidence of axonal degeneration” and “evidence of disorganized regeneration of fibres”, both without closer description and scored only on a present or absent basis—that is, 1-100% = yes; 0 = no. In order to create a global assessment of nerve fascicle injury (which had sufficient security to enable statistical analysis...) “observations of epineural disruption” (my italics) were included, also without a word of definition. No microscopic pictures were included to illustrate these changes.

The statistical handling of these histopathological features was puzzling: “…a table of high and low score point allocation was constructed”. The summed injury score was “allocated to the low score table if the epineural or intraneural score was less than 2.5 or when no evidence of axonal degeneration or disorganized fibre regeneration was observed. The converse applied to point allocation to the high score group” (my italics). It seems as if this system makes a variety of interpretations and a high degree of bias possible.

In their Discussion, Rice and McMahon did not discuss the relevance and validity of their results. The authors seem not to understand the fundamental differences in study design and needle penetration techniques between their study and that of Selander, Dhunér and Lundborg. In the latter study, only a quick needle penetration of a multifascicular nerve was made, to study the frequency of fascicular injury, whereas Rice and McMahon stated: “The rat sciatic nerve used in our study consists of only one fascicle at mid-thigh level and nerve (fascicle) penetration was ensured in all cases”. How, then, is it possible to conclude that

R. E. JOHN
K. M. SHERRY
Royal Hampshire Hospital
Sheffield
