relationship between plasma concentration and effect. We would agree that the assumption of first-order intercompartmental drug transfer is a simplification of what must obtain in the muscle in vivo, and it may be that further refinement of the link model will bring closer agreement between observations and model predictions. Such refinement would add further model parameters, and, in our view, these will require simultaneous justification from both structural and statistical points of view.

We are delighted by the interest which Dr Fisher has shown in our work, but we fail to see that there is a flaw in the justification for the threshold model which we have proposed.

C. J. R. PARKER
J. M. HUNTER
Royal Liverpool University Hospital
Liverpool


SEDATION WITH PROPOFOL DURING REGIONAL ANAESTHESIA

Sir,—Diprivan (propofol) has been used extensively to sedate patients undergoing procedures with regional anaesthesia. However, its use to provide sedation, as distinct from anaesthesia, in association with regional anaesthesia is not an approved indication in every country. Recent published information [1] has indicated that, in some instances, sedation with propofol in patients given regional anaesthesia for surgical procedures can be associated with bradypnoea or hypoxaemia, or both. A reduction in oxygen saturation has been reported by Patterson and colleagues in patients given propofol for sedation whilst undergoing upper gastrointestinal endoscopy [2]. In view of these observations, oxygen saturation should be monitored in all such patients, with oxygen supplementation readily available for provision where clinically indicated.

ICI also wish to remind clinicians that propofol should be given only by those trained in anaesthesia and not those directly involved in the conduct of surgical or diagnostic procedures.

B. D. C. ARNOLD
ICI Pharmaceuticals
Macclesfield


ASPIRIN, BLEEDING TIME AND CENTRAL NEURAL BLOCK

Sir,—We write in support of the views expressed by Drs O’Kelly and Lawes [1] on the limited usefulness of the bleeding time as a clinical screening test in patients receiving aspirin therapy who are candidates for central neural blocks. Our own detailed views have been published elsewhere [2] and we shall not repeat them here. However, we are concerned that Dr McDonald maintains her dogmatic support [3] of the use of the bleeding time and that Professor Bromage suggests that it may be medically-legal imprudent not to perform this test [4]. We would remind them that there are two entirely separate matters being considered here. First, does aspirin therapy increase the risk of spinal haematoma? Second, is the bleeding time a clinically useful indicator of the amount of platelet dysfunction produced by aspirin therapy? Even if the answer to the latter question is in the affirmative, this does not imply that aspirin therapy increases the risk of spinal haematoma. That question can only be answered by long-term epidemiological studies. At the moment there are no data, only opinions.

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DEATH CERTIFICATION

Sir,—I found Dr Baskett’s review of cardiopulmonary resuscitation helpful and informative [1]. However, he perpetuates a common misconception about certification of death. He states that (non-medical personnel) “are not qualified to certify death”. In the U.K., this is incorrect. If Dr Baskett cares to examine a death certificate, he will find that there is still space to indicate that the body was not seen after death by a medical practitioner. Statements such as his will encourage bystanders, professional or not, to undertake futile, undignified and psychologically hurtful cardiopulmonary resuscitation when it is plain that it would be pointless.

It is unhelpful for an expert reviewer to preach the uncritical application of cardiopulmonary resuscitation, which is already widespread and too often inappropriately attempted.

A. C. SKINNER
Warrington


Sir,—Thank you for asking me to comment on Andrew Skinner’s letter. He may be technically correct in his review of the contents of the death certificate. However, I was dealing with the reality of U.K. practice, particularly with nurses in hospital who require prior instructions from a senior doctor in charge of the patient before implementing a “do not resuscitate” order. I was urging that consultants and other senior doctors make such a decision before their patients suffer cardiac arrest, and communicate their instruction to the nursing staff. Doctors would seem to be abandoning their responsibility if they left such decisions to the nursing staff. More extensive use of a hospital “do not resuscitate” policy for patients in whom resuscitation is inappropriate should go some way towards achieving Dr Skinner’s (and my own) aim of reducing the number of futile and undignified resuscitation attempts.

The situation outside hospital is slightly different because, in the majority of cases, prior medical instructions have not been issued. There may be a few instances in which the family doctor has given guidance to the relatives. However, I believe that the ambulance service, in the absence of knowledge of the patient’s prior health status, should be instructed to proceed with resuscitation with the exception of obvious cases such as decapitation, incineration or rigor mortis, etc.

P. J. F. BASKETT
Frenchay Hospital
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KETOROLAC POSTOPERATIVE ANALGESIA

Sir,—I read with interest the article “Ketorolac for postoperative analgesia after orthopaedic surgery” [1]. Currently, we are undertaking a similar study investigating the morphine-sparing effect of ketoprofen after total knee replacement. I wish to make the following comments:

First, a fixed i.m. dose of morphine is not the ideal way to determine the amount of opioid needed to investigate a morphine-sparing effect of any drug [2]. The total dose of morphine given by