yielded a substantial amount of information about the variation of this particular anaesthetic effect.

It is important that comprehensive reviews identify uncertainties in knowledge. It is also important that they define accurately the basis for the uncertainties. In the case of the ventilatory response to hypoxaemia during anaesthesia, uncertainties certainly exist, but they would seem to arise not so much from the type and number of subjects studied previously as from the fact that all reported studies have been undertaken by a single group—our group at the University of Western Ontario. To my knowledge, none of the findings of the present review have been corroborated or refuted by other investigators. Although there are obvious ethical constraints in studying physiological responses to hypoxaemia in anaesthetized humans (as alluded to in Dr Nunn’s review) and appreciating these constraints need not preclude useful research, as was pointed out elsewhere [12].

I do hope that other investigators recognize the need for further study of the ventilatory response to hypoxaemia during anaesthesia and after anaesthesia in humans (as alluded to in Dr Nunn’s review). These constraints need not preclude useful research, as was pointed out elsewhere [12].

I refer to this uncertainty as the nature of the informed consent and minimizing the number of patients required to establish the finding. I know that Dr Knill and his Ethics Committee did give these matters the most careful consideration.

R. L. Knill
London, Ontario

REFERENCES
1. Nunn JF. Effects of anaesthesia on respiration. British Journal of Anaesthesia 1990; 65: 54–62.

Sir,—My friend Dr Knill has already pointed out to me that I have misrepresented his studies in my review [1]. I can do no more than present my unreserved and public apology for mistakenly relying on my memory of his classic papers, which I had read some years ago. He had indeed studied adequate numbers of patients, and reached his conclusions beyond any reasonable doubt.

Nevertheless, as Dr Knill himself states, there has been a reluctance for these crucially important findings to be confirmed in other centres. No doubt one factor has been the ethical constraint inherent in reducing the arterial Pco₂ of anaesthetized patients to 6 kPa (45 mm Hg) for experimental purposes. I am not saying that such a study is “unethical”, but rather that one should not embark upon such work without the most careful consideration of the risk:benefit ratio, the nature of the informed consent and minimizing the number of patients required to establish the finding. I know that Dr Knill and his Ethics Committee did give these matters the most careful consideration.

J. F. Nunn
Harrow

REFERENCES
1. Nunn JF. Effects of anaesthesia on respiration. British Journal of Anaesthesia 1990; 65: 54–62.

REGIONAL ANAESTHESIA REQUIRES ATTENTION TO DETAIL

Sir,—Two papers [1, 2] published in the March 1991 issue of British Journal of Anaesthesia are clear reminders of the importance of attention to detail when performing regional anaesthetic techniques, but both require some further comment.

Levins claims that “standard” methods of identifying L4 are not particularly accurate, but I would suggest that the method he used—“dropping an imaginary vertical line”—was not the standard practice of experienced practitioners. His subjects were cadavers, and presumably they were in an approximately anatomical position. I would suggest that the construction of Tuffier’s line in a patient with hips and spinal column flexed maximally is “standard” practice. It is what is advocated by all the texts on my shelf. As a regular user of thoracic extradural block, I must also take great issue with the bald statement that the spinal cord is at “considerably more risk of damage” when needles are inserted at higher levels. Like all practical procedures in medicine, “it is not what you do, it is the way that you do it”!

Perhaps the same message is relevant to the description by Lee and Parry of a case of meningitis after spinal anaesthesia for Caesarean section. Although other possibilities must not be lost sight of, I would agree with their assessment that this was probably a bacterial infection and it seems very likely that the infection was introduced at the time of lumbar puncture. I