Entitlement to hospital treatment in the UK

Sir,

Our review 'Entitlement to Hospital treatment in the UK' (vol. 72(3), 2011, p. 156) was written from a general perspective and it was disingenuous of the authors of the accompanying editorial to suggest any connection with private practice. We do not question the requirement for provision of 'immediate care' or the treatment of infectious diseases that might harm public health. There is, however, a genuine need to clarify where this 'category' of therapy ends and becomes 'necessary but not immediate' or indeed 'ongoing'. The point of our review was to stimulate debate on the complexity and obvious abuse of these regulations. The populations of the European Economic Area, Soviet Union and the world hardly require referencing. Simple maths reveals the minimum number of people entitled to health care in the UK (in excess of 825 million).

We would suggest that misinterpretation of NHS charging has 'harmed' the UK taxpayer more than any patient. There is no harm to a person who, in our hospital, always receives care but frequently has no ability to pay the invoice. Health tourism may not be obvious to some but is very obvious to those working general medicine 'takes' in London.

Two examples illustrate the flagrant abuse and the potential costs in terms of resources and money: a foreign 'student' who arrived at a local university with portable oxygen seeking a lung transplant; a non-resident patient who twice offered false identification when both receiving a cardiodefibrillator and returning to have it interrogated (the device had effectively 'chipped' her). Accurately identifying people is a major part of this problem.

We suggest the intention was a national health service, not international. There is need for simplification of these regulations. The message requires 'broadcasting' that the UK does not provide free health care to the world.

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Management of metabolic acidosis

Sir,

Ashworth et al (vol. 71(7), 2010, p. 417) have presented several interesting views on the management of metabolic acidosis. Surprisingly, the authors do not mention some aspects of metabolic acidosis which were discussed 4 years ago in this journal (Sharma and Hadebe, 2007a,b).

Ashworth et al's article contains some discrepancies with the literature. They write 'Acidosis depresses myocardial contractility'. This has been observed only in anaesthetized experimental animals and isolated organs; in humans low blood pH does not have negative effects on myocardial contractility (George et al, 1996; Maury et al, 1999; Vallet et al, 1999). Therefore, increasing low blood pH cannot improve the haemodynamic situation of the patient (as reported in Ashworth et al's references Mathieu et al, 1991; Andrade et al, 2007).

On the other hand, the glycolytic enzyme phosphofructokinase is pH dependent: its activity decreases with decreasing pH (Trivedi and Danforth, 1966) and, thus, glucose use in brain cells is impaired (Van Nimmen et al, 1984). Therefore, the clinical consequences of decreasing blood pH are drowsiness–stupor–coma–death in coma (Edge et al, 2006). Recovery of comatose patients with very low blood pH to full alertness is the result of increasing the low blood pH after infusions of alkalizing solutions (e.g. Ahmad and Beckett, 2002). Where are published reports on recovery of comatose patients with very low blood pH to full alertness without increasing the very low blood pH?

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