

Preference is given to letters commenting on contributions published recently in the *JRSM*. They should not exceed 300 words and should be typed double spaced

Continuous fetal heart rate monitoring

It was heartening to read an article by a senior obstetrician—Mary Macintosh—describing the difficulty experienced by the Confidential Enquiry into Stillbirths and Deaths in Infancy when assessing the interpretation of fetal heart rate monitoring patterns during labour (January 2001, *JRSM*, pp. 14–16). Like her, I have experienced difficulty in drawing robust causal inferences from observational studies¹, and I agree with her that the existing randomized controlled trials (RCTs) can not rule out the possibility that mortality is lower with continuous fetal heart rate monitoring than with intermittent auscultation. Data from the Cochrane systematic review², for example, are compatible with a 40% reduction in the odds of intrapartum and first week deaths of normally formed babies—a reduction that many people would consider important even though such deaths are now very rare indeed.

What prospects are there for obtaining unbiased and more precise estimates of the effects of continuous monitoring on mortality and, perhaps, cerebral palsy? Sample size certainly presents a challenge, but this should not be regarded as insuperable. More than a decade ago, a multinational RCT quickly showed, after cluster randomizing 70 000 women, that routine formal fetal movement counting is unlikely to be a very effective way of reducing fetal deaths³. Such multicentre studies not only make large studies easier to mount, they also provide evidence that may be more widely applicable. The recently published trial of elective caesarean section for breech presentation at term exemplifies this impressively⁴.

Pending decisions about whether further randomized trials are feasible, the existing evidence² can still inform practice. The reduction in neonatal seizures associated with continuous fetal heart rate monitoring has only been seen in controlled trials in which this screening test was used in conjunction with an assessment of fetal acid–base status to rule out ‘false positives’². There is no robust evidence to support the use of continuous fetal heart rate monitoring alone, which simply increases the use of caesarean section, with no evidence of any compensating beneficial effects.

Just as long as the evidence Mary Macintosh has reviewed remains so slim, it will be difficult to know what should constitute ‘quality’ in the interpretation of fetal heart rate traces. She mentions that ‘a national evidence-based guideline funded by the Department of Health is in preparation’. I hope that it will be recognized that the most rational starting place for developing guidelines for interpreting fetal heart rate traces is the practice within

those controlled trials, such as the Dublin study⁵, which have shown a beneficial effect on neonatal seizures.

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Colo-broncho-cutaneous fistula complicating traumatic diaphragmatic rupture

Dr Biswas and colleagues (February 2001 *JRSM*, pp. 88–89) indicate that the main message of their case report is that traumatic diaphragmatic hernia can be associated with serious intrathoracic complications, particularly if the colon is ruptured. However, I venture to suggest that this particular case report has a much more important message relating to the failure to follow cumulated experience on how to manage intestinal fistulation following breakdown of intestinal anastomotic repair. It is noteworthy that the previously fit 45-year-old man had two further colonic resections after his initial resection had broken down and formed a fistula. At his second operation, resection was undertaken after failure of attempts at percutaneous drainage and parenteral nutrition to close the fistula. Anastomosis was carried out, despite the fact that there were abscess cavities in the subphrenic space. To those experienced in fistula surgery it would come as no surprise that this second anastomosis, performed in an adverse environment, also broke down and formed a fistula. Despite this, at the third laparotomy yet another anastomosis was carried out although it was considered prudent to ‘protect’ it by a defunctioning loop ileostomy.

Although biochemical details are not given, it is almost certain that this patient would have been hypoalbuminaemic at the time of his second and third operations, a finding known to be predictive of anastomotic failure and other complications. However, even if the albumin was in the normal range it has long been recognized by those units with considerable experience of dealing with recurrent

fistulation following an anastomotic leakage that anastomoses placed in a septic environment almost invariably lead to further leakage. Exteriorization of both ends of the colon should have been the preferred option at the first operation, but certainly no later than the second operation. Though the management pathway for recurrent fistulation given above has been known for many years¹, the message concerning exteriorization still fails to get through.

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Author's reply

The problem with any short case report is that only points relevant to the issue being raised can be highlighted and many of the other factors, important as they are, cannot fully be addressed. Clearly as Sir Miles realizes the intention of the presentation was to point out the serious intrathoracic complication.

By the time the patient was referred to St Mark's Hospital the sepsis was very localized in the left subphrenic/supraphrenic area and was rather akin to a localized abscess in association with, for example, an area of Crohn's disease. The serum albumin was normal. Contrary to Sir Miles' assertion—a misunderstanding due to the brevity of the report—the anastomosis was not 'placed in a septic environment'. It was nowhere near the localized sepsis in the left upper quadrant but was in a clean uncontaminated abdomen.

There was no anastomotic failure after the repeat operation, in which the bowel was defunctioned. Death was unrelated to the abdomen, but was found at *post mortem* to be due to respiratory failure. 'Pace, Sir Miles'.

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Leechcraft

Professor Carter (January 2001 *JRSM*, pp. 32–42) says that, by the end of the nineteenth century, leeches had gone out of fashion. I am able to report the use of leeches in Britain by orthodox medical practitioners as late as 1934. They were used on me personally when I was a medical student in Cambridge, so it seems fair to conclude that a doctor prescribing them in the 1930s did not expose himself to criticism or to suspicion of witchcraft.

A dental extraction of a lower-jaw premolar had been followed by acute sepsis of the vacant cavity which spread rapidly to the surrounding tissues. With the diagnosis of Ludwig's angina I was put into the University Nursing Home where I soon became seriously ill with my whole neck an indurated mass. I was unable even to turn my head. My case was pronounced serious and it was said that further surgery would be fatal.

The reaction of my tutor was to bring to my bedside a bottle of a particularly precious champagne. My mother obtained the presence of the chaplain, who administered the last rites. If an *exitus lethalis* was to be avoided the case called for some new and decisive intervention. My very able general practitioner remembered that one of the pharmacies in the town stocked leeches. They were applied, and sixty-five years later I am able to write you this letter.

On recovery I went to see the pharmacist. He said that his predecessor had told him that those looking for leeches were always desperate so he kept a small stock obtained from a good supplier—a fenland farmer. He always went to that same farm; they were clean and reliable. The pharmacy kept this stock in an aquarium: 'When they want them they always seem pretty frantic', he told me. I wonder if any old established chemist is still standing by with good fenland leeches.

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Traditional Chinese Medicine and the House of Lords

Professor Ernst categorizes the House of Lords Science and Technology Select Committee report on complementary and alternative medicine as 'something for everyone' (February 2001 *JRSM*, pp. 55–56); but the report perpetrates a contradictory and ill-informed assessment of traditional Chinese medicine (TCM). The Committee's assessment is contradictory because, while the report affirms that there is scientific evidence for acupuncture and herbal medicine, it claims there is none for TCM. Acupuncture and herbal medicine form the basis of TCM, so how can this be?

The Select Committee did not seek evidence from a professionally qualified practitioner of TCM; indeed, in some instances it chose to listen to less than reliable witnesses and as a result received misleading and inaccurate information. There is a vast amount of pharmacological and clinical research into the therapeutic properties of Chinese herbs. They contain many of the active compounds found in Western pharmacopoeias.

The first controlled clinical trial of traditional Chinese medical plants in widespread non-exudative atopic eczema

took place back in 1989. It was conducted in the UK and demonstrated a credible scientific base for the clinical efficacy of TCM^{1,2}. The evidence from this clinical trial not only proved the effectiveness of treatment, but also indicated that Chinese herbs have platelet antiaggregatory, analgesic and antipyretic properties³. Chinese herbs are also associated with immunological changes⁴. Furthermore, a vast number of scientific articles support TCM with evidence of efficacy based on the results of well designed trials in the USA. Many of these have been set up or endorsed by the World Health Organization.

The effectiveness of TCM in treating infertility is also creating much interest in the UK, from both patients and fertility specialists. A controlled clinical study is urgently required. Without it, bodies like the Select Committee will continue to deem TCM unscientific and therefore unprofessional. But if they believe that science is about experience and discovery, research and confirmation then it follows that every discipline should be treated fairly.

Of course, safety is of prime importance in any clinical practice. It is therefore particularly distressing to know that people without a high standard of training are being allowed to practise complementary and alternative medicine (CAM) legally. If the Lords' report really is intended to protect the health of the public, it is indeed essential that CAM becomes well regulated, with good educational standards and training. As part of the process, the committee should revise its classification of both Chinese herbal medicine and TCM without delay.

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John Hunter's teachings on gunshot wounds

Professor Ellis (January 2001 *JRSM*, pp. 43–45) quotes from Sir Everard Home that Hunter's poor health led to his enlisting in the Army in 1760. This is the popular view also cited by Beasley¹. It is possible that this is an erroneous opinion akin to that previously perpetuated regarding the cause of Hunter's death—as syphilitic aortitis acquired in the dissecting room, when in fact he died of ischaemic heart disease. An alternative view proposed by Dobson² is that Hunter enlisted in the Army primarily to advance his career. This seems more plausible considering the great risks from both injury and disease attendant on military service—a topic considered by Dr Gordon Cook in the subsequent issue of the *JRSM*.

Another point, as discussed by Oppenheimer³, is the controversy surrounding the relationship between Home and Hunter. Home, who destroyed Hunter's manuscripts and has been accused of plagiarism, might well have had nefarious reasons for impugning Hunter's health.

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