complications and the potential neurotoxicity of intrathecal magnesium has not been adequately addressed in animals. Morrison and colleagues rightly qualify their review with the caveat that magnesium sulphate is not currently licensed in the UK for intrathecal administration. We urge clinicians to consider these concerns. Rather than proceeding with additional RCTs to answer the questions raised by this meta-analysis, we advocate additional basic science studies to strengthen our understanding of the risk-to-benefit balance from the use of this intrathecal adjunct.

Declaration of interest
None declared.

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Reply from the authors
Editor—We thank Dr Albrecht and colleagues for their thoughtful comments regarding our recent meta-analysis of the effect of intrathecal magnesium on the duration of spinal anaesthesia. They are justified in raising concerns regarding the safety of using intrathecal magnesium and, in particular, of neurotoxicity. We did mention neurotoxicity within the Discussion section, including the study from Saeki and colleagues which demonstrated the destruction of laminae V–VII at doses of magnesium 1 mg kg−1 or more. In contrast, a study from Chanimov and colleagues demonstrated no histological differences in the spinal cord when serial intrathecal injections of up to 12.6% magnesium sulphate, lidocaine, or saline were given to rats.

Other animal studies have also shown conflicting evidence regarding neurotoxicity. In contrast to Saeki and colleagues, Simpson and colleagues demonstrated that administering intrathecal magnesium before thoracic aortic cross-clamping reduced the incidence of paraplegia from spinal cord ischaemia in animals. A recent study has shown, however, that in animals, 15% magnesium sulphate given via the intrathecal route is associated with some degeneration of the mitochondria, and neuronal degenerative changes on repeated injections. The trials included within our meta-analysis either used 50% magnesium or 15% magnesium sulphate. There were no reported events suggestive of neurotoxicity within these or any other studies in our meta-analysis, although undoubtedly the length of follow-up may not have been sufficient to uncover all the possible neurological effects. Given the inconsistency regarding the potential neurotoxicity, we would advocate that the safety profile of magnesium sulphate should perhaps be confirmed with further well-designed safe dose finding studies in clinical research.

Dr Albrecht and colleagues also raise the issue of disorientation after epidural administration of magnesium sulphate that was administered as an infusion inadvertently. We did not investigate the effect of continuous epidural magnesium sulphate; hence, we cannot speculate as to whether the symptoms are applicable to the intrathecal route. Furthermore, the authors of the case report attributed the symptoms to a supra-therapeutic serum magnesium concentration having received 9 g in just over 1 h.

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Evidence-based language

Editor—Chooi and colleagues1 have once again drawn our attention to the noxious power of ill-chosen words.2 Our specialty is beginning to realize that sophisticated anaesthetic techniques can be sabotaged by simple, thoughtless errors in communication. We now believe that there is an opportunity for the Royal College of Anaesthetists to embrace the current evidence and thus catalyse two desirable developments.

First, widespread adoption of ‘evidence based language’ has the capacity to attenuate the distress of invasive medical procedures. Lang and Laser’s3 excellent textbook catalogues a wide range of linguistic and behavioural approaches (supported by data) with the potential to alleviate pain and anxiety. It remains depressingly common to hear anaesthetists using expressions such as ‘bee sting’ and ‘sharp scratch’. These clumsy verbal relics from the 1970s were asserted, without evidence, to be beneficial. Dutt-Gupta and colleagues4 demonstrated that the opposite was true. We are concerned that trainees are still being taught to use these obsolete language rituals.

Secondly, we should consider the implications of careless language for informed consent on the day of surgery. In the weeks preceding elective surgery, it is entirely sensible for patients to receive as much balanced information as possible. However, we suggest that the day of surgery should be handled differently. For decades, anaesthetists have acquiesced under constant threat from our friends in the legal profession. In consequence, one hears ever more exhaustive lists of complications being discussed in the minutes before induction of anaesthesia. Patients are naturally tuned in to hear the worrying bits. And so, ironically, in an era when anaesthesia has never been safer, patients have never been more convinced that they are doomed! Officious recital of endless lurid complications on the day of surgery is, in our opinion, both professionally clumsy and ethically questionable. We feel that our College is ideally placed to confront the lawyers and to spell out the deleterious effects of legalistic defensiveness on our patients.

Language matters to patients. Rarely does an opportunity to alleviate distress with zero cost present itself. It is time to follow the evidence.

Declaration of interest

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Intravenous or perineural dexamethasone for interscalene brachial plexus block: the equivalence not yet proven

Editor—We read with interest the article by Desmet and colleagues1 comparing i.v. and perineural dexamethasone. The results of the study suggest that both i.v. and perineural dexamethasone are equivalent in increasing the analgesic duration of single-shot interscalene block (ISB). Although the results of the study are important to clinical practice, especially with the recent evidence of neurotoxicity in animal studies on perineural dexamethasone,2 a few methodological issues of the study need clarification.

The hypothesis of the study was that dexamethasone prolongs the duration of single-shot ISB regardless of the route of administration. Based on their hypothesis, the study is appropriate for an equivalence design. The inclusion of a ropivacaine-only group is not necessary for hypothesis testing, as i.v. dexamethasone has been shown to have analgesia effect in the postoperative period.3

There is no indication in the article specifying that the dexamethasone used during the study was preservative-free or not, since the preservatives may have neurotoxic properties.4

The authors have tested motor blockade of fingers to define block success rate along with pain scores. This cannot be considered as a valid method of testing ISB as a recent study has shown that only 15% (95% confidence interval, 6–33%) of individuals receiving ISB had surgical block of the hand and forearm.5

The authors have used 30 ml of 0.5% ropivacaine for the block. This certainly is not the standard of care in our practice since the use of lower volume of local anaesthetic under

References


