Ultrasound guidance for regional anaesthesia

Editor—As a proponent of ultrasound-guided regional anaesthesia, I enjoyed reading the excellent case series by Karmakar and colleagues. However, I have reservations about a number of the conclusions drawn in the accompanying editorial by Hopkins. I agree that the recent regional anaesthesia ultrasound literature has many studies that lack equipoise. However, this fact surely reinforces our responsibility to strive for the best quality ultrasound studies in order to properly answer the question of whether ultrasound techniques really do have significant advantages. This is because there are many existing and very successful users of other regional anaesthesia techniques that correctly demand the highest quality evidence before committing to the significant expense and inconvenience of training with new technology. Although I would agree with Professor Hopkins that almost all radiologists would support the use of ultrasound for certain interventional techniques, this opinion has been developed through studies that exist in the radiology literature comparing ultrasound with other techniques for biopsy. In addition, radiology studies conclude that ultrasound is but one technique among many that can help achieve a predefined endpoint and that the optimal combination of a number of endpoints can improve success.

I also disagree that there are no inherent harmful effects of ultrasound guidance. Many of the newly developed ultrasound-guided approaches have significant potential for harm if the needle tip is not carefully visualized and followed during insertion. Superficial ultrasound techniques often use very different needle insertion points compared with traditional techniques where the needle insertion point and path has been carefully developed to avoid vital structures. If the needle tip is not clearly followed with ultrasound, significant potential for mishap can occur. With deeper blocks, local anaesthetic expansion can be difficult to appreciate using ultrasound and the usual precautions such as incremental injection and frequent aspiration remain very important. Inexperienced users of ultrasound can obtain a false sense of security and fail to continue to observe these precautions leading to potential for significant complications.

Until we have further evidence for the benefit of ultrasound-guided regional anaesthesia in enhancing success and reducing complications, it is premature to imply that ultrasound is a safer technique. We need to determine the ideal endpoint or combination of endpoints for different types of blocks. For many regional anaesthesia procedures, I believe, based on personal experience and reading the literature, that ultrasound will be the key component. However, until we have the best quality evidence to support this assumption we need to be very careful about making premature conclusions.

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C. J. L. McCartney has received payment of honoraria by SonoSite Corporation.

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Editor—Ultrasound guidance for regional anaesthesia is a very promising method, and there is much in the editorial from Hopkins that I agree with and support. However, the method must be investigated fully, not simply accepted as perfect as he proposes. Further, I would take issue with his third paragraph, one which I judge to be seriously incautious. To state, as he does, that a clinical aid will produce ‘successful nerve block with no complications’ is wildly over-optimistic, but to then state that ‘any deviation from this standard is an operator deficiency’ is an open invitation to the legal profession to pursue colleagues whose practice may otherwise be impeccable.

Almost any method can be highly successful in the hands of an enthusiastic expert, but the true test is how well it works in routine use. Those who, like me, have been involved in regional anaesthesia for more than three decades will remember that the introduction of nerve stimulators was supposed to eliminate block failure, but they did not, and the history of medicine as a whole is littered with the incautious introduction of ‘advances’ deemed too important for proper investigation, only for them to be shown, much later, to be less than advantageous. It is hard to see how the use of ultrasound could cause harm to patients, although it might engender excessive confidence and thus encourage incautious practice if not properly evaluated. Further, the equipment is expensive and justifying its cost will require the availability of high-quality evidence. Enthusiastic reports after use in what are really very small numbers of patients (all that we have to date) do not represent such evidence.

In regard to evidence, I do not accept that randomized controlled trials have no place in the evaluation of ultrasound for regional anaesthesia, although I agree that the incidence of complications is (or should be) too low for such comparison. The same might be true of block success rates, although I am not so sure, and properly designed and
Editor—We read the recent editorial by Hopkins on ultrasound guidance for regional anaesthesia and would like to take issue with some of the comments and conclusions. Although accepting that it is difficult to perform large scale, randomized studies in this area, it is dangerous to draw such one-sided opinions in the absence of such studies.

Medicine is littered with technologies which were introduced because logic indicated a benefit, but long-term studies showed otherwise. Pulmonary artery catheters were one and in the field of obstetrics, which Hopkins uses as an example to further his argument, clinicians find themselves in a medico-legal minefield as a result of lack of clear evidence for some medical interventions. To imply that ultrasound should offer 100% success and 0% complications is optimistic in the extreme and to suggest that failure to meet these standards is down to ‘operator deficiency’ goes against one of the founding principles of risk management; we all err from perfection. Those of us who perform hundreds of regional blocks a year with a high success rate (the 95% that Hopkins quotes) with no major complications, efficiently and with a minimum of equipment need good arguments for changing their established practice. We would not consider ourselves to be either intransigent or in the minority, as Hopkins would have us believe.

We feel that ultrasound guidance will be a useful addition to the armamentarium of the anaesthetist practising regional anaesthesia, but to so strongly imply that those not using this method are medically negligent is a dangerous and unhelpful step.

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Editor—After reading the editorial by Hopkins, I have a few remarks. First, Hopkins states that he found only one paper, in children, that demonstrated an improved success, but then appears to recommend its use as a gold standard. Is it because he was sponsored by SonoSite? After all, the use of new technologies should be based on evidence. Why not in this case? If we accept this as fact then we are on the wrong path, this includes the BJA. I challenge the idea that ultrasound should be used always, as it is not good for our residents. They do not learn to use landmarks, nerve stimulation, and good clinical approach of the patients who rely on the expertise of the doctor. We also force hospitals with a smaller budget to invest in techniques which are not better than the ‘old’ techniques.

The presumed disadvantage of nerve stimulation that injection in the nerve sheath is dangerous might not be the truth as described by Bigeleisen. The last remark is based on Hopkins statement that more research and development is necessary in ultrasound-guided regional techniques. I suggest that more research is required to prove that ultrasound is better than nerve stimulation. If this is so, then it is time to re-evaluate the use of regional techniques assisted by nerve stimulation.

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Editor—I am pleased that my editorial has generated debate of the status of ultrasound guidance in regional anaesthesia. I welcome the opportunity to respond to comments concerning the evidence base for the use of ultrasound, the relative contribution to block failure of a particular technique vs human deficiencies, and the motives behind my article.

In my editorial, I questioned the need for randomized controlled trials of ultrasound-guided vs other regional techniques. To varying degrees McCartney, van Velzen, Wildsmith, and Cornforth and Hargreaves would appear to believe that a randomized controlled comparison of ultrasound with other regional techniques is the only form of evidence that we should accept. But let us consider what makes a successful regional block. Can we not agree that this is placement of a needle adjacent to each of the relevant nerves and injection of sufficient local anaesthetic around them, while avoiding causing damage to nerves and other structures with the needle or injecting the local anaesthetic in the wrong place? To argue otherwise is to imply that there is some magical or mystical component to regional anaesthesia. I believe that there is sufficient evidence from experts in ultrasound-guided needle placement to inform us that this can be achieved with greater rates of success than even the most successful series of nerve blocks using nerve stimulator confirmation of needle placement. To ignore this substantial body of evidence for the accuracy of ultrasound-guided needle placement is akin to arguing that parachutes should not be used because
their efficacy has not been demonstrated in a randomized controlled trial.\textsuperscript{10}

The evidence for the accuracy of ultrasound-guided needle placement comes in the form of prospective trials, observational studies, and consensus statements in the radiology literature. McCartney cites, for example, a prospective study of 2403 imaging-guided (ultrasound or stereotactic) breast biopsies that demonstrated an accuracy of \textgreater99\% using ultrasound.\textsuperscript{4} McCartney is wrong, however, in suggesting that Dillon and colleagues\textsuperscript{5} provide evidence that ‘the optimal combination of a number of endpoints can improve success’. This study compared ultrasound-guided breast biopsy with clinically or stereotactically guided biopsy. The success rate was significantly higher in the ultrasound group compared with the others and the authors comment on the advantage of real-time imaging using ultrasound. This study\textsuperscript{5} refers to one disadvantage of ultrasound in this context compared with stereotactic guidance, which concerns the inability to visualize breast biopsy sites that consist solely of calcified tissue using ultrasound: this is of no relevance to ultrasound-guided regional anaesthesia.

I enjoyed Professor Wildsmith’s letter, but I did not state ‘a clinical aid (ultrasound) will produce’ successful nerve block with no complications. I did in fact suggest that ‘with appropriate training, experience and performance, ultrasound techniques have the potential to produce successful nerve block with no complications secondary to needle misplacement in all cases’. This is a fundamental difference to other currently employed regional techniques in which the technology does not reliably confirm perineural location of the needle, nor does it permit identification of anatomical variation. Consequently, block success and avoidance of complications with these techniques always include an element of luck, an element that ultrasound guidance eliminates. McCartney disagrees with my assertion that there are no inherent harmful effects of ultrasound guidance and yet all the examples he gives are from cases that I am aware of, that a more significant factor is direct needle damage secondary to multiple needleings of a nerve. I abandoned using nerve stimulators in conjunction with ultrasound when I observed that it was not unusual for the needle tip to be directly adjacent to a nerve trunk while not eliciting a response to the nerve stimulator, illustrating the lack of sensitivity of nerve stimulation.\textsuperscript{11} I believe this is a potential drawback of ‘blind techniques’.

Declarati on of interest

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Monitoring cardiac output with Flo Trac Vigileo™

Editor—In their recent article, Mayer and colleagues compared a new device for semi-invasive determination of cardiac output (CO) (Flo Trac/Vigileo™, Edwards Lifesciences, Irvine, CA, USA) with the bolus thermodilution technique, considered the clinical ‘gold standard’. They performed their measurements intraoperatively and after operation in patients undergoing cardiac surgery and concluded that the new device, based on arterial pressure waveform analysis, does not appear to adequately measure continuous CO and is not recommended for routine use. In fact, the authors found an overall percentage error of 45.9% in CO measurement compared with the ‘gold standard’ method.

We have been using the new device, Flo Trac/Vigileo™ in the last 2 yr in medical and cardiac surgical patients requiring continuous CO monitoring during ICU stay. In our experience, Vigileo™ and the thermodilution method show no significant differences in CO measurements in patients with ventricular dysfunction without valve disease.

We think that the different experience Mayer and colleagues report is due to several reasons. First, the authors included many patients (about 50% of study group) with mitral and aortic insufficiency, whose arterial compliance and impedance are of course different from patients without valve regurgitation. These differences lead to relevant changes in arterial waveform characteristics and in ventriculo-arterial coupling, which well explain the bias between Vigileo™ technology and thermodilution described by Mayer. Secondly, they did not exclude the presence of tricuspid insufficiency in their study group, even in patients with mitral disease, so accepting a potential pitfall in thermodilution measurements.

In our opinion, Flo Trac sensor and pulmonary artery thermodilution catheter have different indications due to the different information they can provide. In severely haemodynamically compromised patients, we agree that pulmonary thermodilution catheter remains the best option for monitoring CO and cardiac pressures. Whereas in less compromised patients, requiring flow monitoring, in our experience the new device Vigileo™ provides a valuable set of information, with reliable CO calculation by Flo Trac sensor plus ScO2 measurement if the Presep central venous oximetry catheter (Edwards Lifesciences) is added, with the great advantage of less invasiveness in comparison with pulmonary artery catheterization.

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Editor—we thank Guarracino and colleagues for their interest in our article.1 We agree with the authors that the Vigileo™ system and the pulmonary artery catheter (PAC) might have different clinical indications in the near future. Nevertheless, every new tool to measure CO has to be validated before it is introduced in clinical practice.

We disagree, however, with the valve theory as a possible explanation for the bias raised in their letter. First, it was described in the methods section of our article that patients undergoing mitral or aortic valve surgery were excluded from analysis if they showed echocardiographic signs of aortic or mitral dysfunction after surgery. Secondly, we did not state in the article that 50% of the included patients had mitral or aortic insufficiency. Patients scheduled for valve replacement in fact included aortic and mitral valve stenosis and insufficiency at about the same percentage. Thirdly, during preparation of our manuscript, we analysed patients undergoing valve surgery and coronary artery bypass grafting (CABG) separately. CABG patients (valve dysfunction before operation excluded) showed a percentage error of 45.1% with a bias of 0.44 litre min⁻¹ m⁻² and a precision of 1.24 litre min⁻¹ m⁻² which was comparable with patients undergoing valve surgery. Aortic and mitral valve dysfunction hence did neither bias thermodilution nor Vigileo™ results in our study.