10% of Caucasians showing little or no benefit from its use.\(^5\)\(^6\) This therefore makes it difficult to explain the prescription of a pro-drug, where we can so poorly predict the response without genetically testing each individual patient.

The rationale given for using codeine in paediatrics is reported to include familiarity with the drug, the lack of controlled drug status, and ease of prescribing.\(^2\) It is administered in day surgery settings and prescribed for use in the community. However, recommendations for codeine in the postoperative setting have declined\(^7\) and the evidence for its use as an antitussive\(^8\) is lacking.

We propose that codeine is popular in the paediatric population because healthcare professionals perceive it to be a ‘safer’ choice than morphine. To investigate this further, we completed a snapshot survey in two local district general hospitals, examining staff views on the safety profile of codeine in paediatrics, with respect to other commonly used analgesics. Fifty-nine doctors and 25 nurses (n=84), spanning a wide range of specialties and levels of experience, completed a short questionnaire; 100% identified morphine as a drug they would worry about causing respiratory depression, only 42% felt the same about codeine (a pro-drug for morphine). Moreover, when asked about relative overall drug safety, 92% ranked codeine as safer than morphine and notably 88% ranked it safer than tramadol and 15% ranked it safer than ibuprofen.

Moderate pain should never be left untreated either in hospital or in the community and codeine has traditionally been used in this setting. Nevertheless, in the light of Niesters and colleagues’ paper,\(^1\) we need to question our practice with respect to the use of codeine in paediatric patients. Whether this should involve genetically testing patients before its prescription, treating codeine as a controlled drug, giving clear warnings of risk related to its use, and training to ensure adverse events are identified early or all of the above, remains to be debated. However, until a consensus is agreed, we as clinicians need to consider the use of this ‘safer’ analgesic very carefully.

Declaration of interest

None declared.

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Reply from the authors

Editor—We thank Drs Wynn-Jones, Casely, Laycock, and Bantel for their interest in our paper\(^1\) and useful comments regarding the use of codeine in the paediatric population. We fully agree with their remarks and believe that they are a useful addition to our paper.

In the adult population, the number of complications related to codeine is far less. In a review of case reports of patients receiving opioids for the treatment of chronic pain that developed respiratory depression, it became apparent that in just one of the reported 42 cases, codeine played a role.\(^2\) We agree that our study does not allow any conclusions regarding the incidence of respiratory depression from codeine (or of any other of the opioids for that matter), as we most probably have to rely on a limited number of published case reports. Still the difference between the adult and paediatric population is striking. Irrespective, given that codeine is a prodrug that relies on a polymorphic enzyme system with a large variability in efficacy, in common with Drs Wynn-Jones, Casely, Laycock, and Bantel, we support constructive debate over the need for codeine in the armamentarium of physicians involved in the treatment of pain.

Finally, as stated above, we are pleased to see that our review of case reports has prompted a lively and informed discussion of opioid-induced respiratory depression in clinical practice and hope to receive many thoughts from our colleagues.

Declaration of interest

None declared.

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Post-spinal hypotension in parturients undergoing lower segment Caesarean section: ‘preoperative anxiety’ or ‘anti-hyperbaricity’

Editor—I read with interest the study by Orbach-Zinger and colleagues that claimed a link between preoperative anxiety and post-spinal hypotension in term-parturients undergoing lower segment Caesarean section. The article seemed to overly credit correlative association of a subjective phenomenon (patient anxiety) with post-spinal systolic arterial pressure, which, peculiarly, was not backed by its objective counterpart (salivary amylase). They probably missed the actual scientific basis involving ‘cause (spinal anaesthesia)–effect (hypotension)’ temporality in that either the spinal anaesthesia was conducted as the manuscript detailed or they undertook certain precautions that were not mentioned.

It is known that the addition of fentanyl to the hyperbaric bupivacaine results in relative negation of hyperbaricity and higher/unpredictable cephalic spread of the local anaesthetic. In practice, for the queer reason that befalls scientific contention, the ‘anti-hyperbaricity’ resulting from ambitious attempts at decreasing bupivacaine dose while fentanyl remaining the same has escaped contentious deliberation. Further, the practice of active aspiration of unsure/variable volume of cerebrospinal fluid at body temperature to confirm the spinal needle position adds to the ‘anti-hyperbaric’ effect. Since the study did not reflect intent to specifically entertain and address the possibility of ‘anti-hyperbaric’ nature of the co-solution leading to post-spinal hypotension, despite careful fluid loading (pre-/co-loading) and spinal anaesthesia administration in the sitting position, many study participants experienced post-spinal hypotension. Additionally, the immediate position change (sitting–supine) following the spinal block may have also complemented the ‘anti-hyperbaric’ effect.

The evidence that a defined preset spinal anaesthesia technique resulted in uniform achievement of block adequacy (T-4 level, bilaterally) in the majority of participants despite patient characteristic variability (body height, spine lengths), paradoxically, supports anti-hyperbaricity phenomenon and the related post-spinal hypotension. I am not as sure as to whether or not the patients, after they got to the supine position, were made to fold their legs for related procedures (urinary catheterization, vaginal examination to assess cervical dilatation status/fetal-head descent) such that the cephalic movement of the co-solution was in excess of what the inadvertent ‘anti-hyperbaricity’ effect may have induced.

Therefore, it is difficult to assign the effect (post-spinal hypotension) solely to preoperative anxiety in the parturients. The absence of correlation of the salivary amylase, an indirect yet objective parameter of anxiety, with the post-spinal hypotension suggests that within the scope of the methods described in the study, the post-spinal hypotension may have been a random event induced by ‘anti-hyperbaricity’ rather than ‘preoperative patient anxiety’.

In conclusion, the correlation of ‘anxiety’, a fairly common element in term-parturients, with post-spinal hypotension may still have credible impact on obstetric spinal anaesthesia practice, provided they are backed by unbiased results of scientifically supple controlled investigations.

Declaration of interest
None declared.

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