Sputum induction for the diagnosis of pulmonary tuberculosis [letter]

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LETTERS TO
THE EDITOR

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The editors will decide, as before, whether to also publish it in a future paper issue.

Sedation versus general anaesthesia for MRI scanning in children
EDITOR,—We read with interest the article concerning the sedation of children for magnetic resonance imaging (MRI), and would like to support Dr Bray’s view that general anaesthesia is a safer and more reliable method of managing children undergoing this procedure.

In our trust we have a large number of children undergoing MRI scanning, the great majority of whom have general anaesthesia. We have three or four planned half day general anaesthesia sessions per week, all covered by a consultant paediatric anaesthetist. We do still occasionally sedate patients when they require a short scan; because of the urgency it is not possible to schedule them into a fixed general anaesthesia session.

Previously, we relied mainly on sedation techniques, but found a large failure rate due to restless patients moving during the scan. In fact, since general anaesthesia has superseded sedation, the quality of scan has markedly improved and scan times have been reduced.

For patients undergoing cardiac MRI scans, periods of breath holding are required during several scan sequences; this would be impossible to achieve unless the patient was paralysed and ventilated.

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The debate between sedation and anaesthesia for children undergoing MRI

EDITOR,—Drs Lawson and Bray have presented arguments for and against deep sedation of children by non-anaesthetists. We would like to contribute to the debate by expanding on issues which have influenced and encouraged the development of a nurse led sedation service for magnetic resonance imaging (MRI) at our hospital.

There continues to be a huge demand for MRI and as a result we have had to meet the challenge of providing a sedation and anaesthesia service with limited resources. With safety in mind, in 1996 we sought funding for sufficient staffing to justify an anaesthesia service as the only service for one MRI scanner, for four days a week. Funding was refused because of high costs, and because the option of improved sedation by non-anaesthetists had not been fully explored. Fortunately, we have been successful in developing our nurse led sedation service and have needed only a modest increase in anaesthesia sessions from two in 1996 to three currently. We now have two MRI scanners and a total of eight days a week of clinical service, and we are able to look back and reflect that if we had held the philosophy that only anaesthesia was safe enough this would have severely limited any expansion and flexibility in the totality of the anaesthetic service we provide to the hospital.

We believe we have developed a sedation service by non-anaesthetists that is safe and effective.

Everyone seems to agree that conscious sedation, where the patient can be roused by verbal command, is safe for non-anaesthetists but is impractical for imaging in small children because they must be “asleep” to be MRI safe. Furthermore, if anaesthetists are available they are more cost effective when administering anaesthesia than supervising sedation.

We believe that our nurse led sedation service is safe because we have developed a protocol that makes any airway or breathing problem extremely unlikely and, if it should occur, our nurses have sufficient resuscitation skills to cope until help arrives. Reducing the risks to acceptable levels depends on the strict adherence to exclusion criteria, the characteristics of the drug regimen, and finally, but most crucially, the judgement, skills, and experience of the nurses. Our nurses are carefully assessed after an initial training period, and those who are accepted as sedationists receive regular retraining and reassessment. They work to strict protocols devised by a multidisciplinary team consisting of radiologists, anaesthetists, paediatricians, senior nurses, and radiographers. If such a strictly controlled system is not developed, or suitable people cannot be found to implement it, we have no doubt that an anaesthesia service is safer. The references quoted by Dr Bray show that accidents can happen if good practice is not followed.

Our latest figures are encouraging. We have sedated almost 3500 children according to our published sedation guidelines and so far no child has required the use of any airway or breathing device. Oxygen saturation has not dropped below 87%. Can anaesthesia, including postoperative recovery by nurses, match these statistics?

It is fair to suggest that a sedation service might be made even safer with anaesthetists present throughout the procedure. Nevertheless in our hospital, we do not believe that such an expense could be justified. Furthermore, if anaesthetists are not available they are more cost effective when administering anaesthesia than supervising sedation.

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Investigation of sudden unexpected deaths in infancy
EDITOR,—The CESDI study report on sudden unexpected deaths in infancy1 and the paper in this journal by Ward, Platt, et al,2 emphasise the importance of thorough inves-
tigation of all sudden infant deaths if the true cause is to be found.

The history of an apparent life threatening event emerges as a significant risk factor for sudden unexpected death and this, together with symptoms of ill health including sweating in the 24 hours before death, suggests that death in these cases may be due to a metabolic cause in a vulnerable infant.

Although inherited metabolic diseases (IMD) are rare because of the reduction in preventable causes following the “back to sleep campaign”, they are now likely to form a higher proportion of all sudden unexpected infant deaths, and accurate diagnosis of any

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index case could well prevent death in a subsequent sibling.

Factors suggesting IMD include consanguineous parents and previous infant death in the family. Although a history of hypogamnatoxia or developmental delay and organomegaly may occur, these disorders can cause death without significant prodromal symptoms and can be precipitated in a previously healthy infant by a stress such as infection. Investigation may be limited if muscle or liver biopsy is not available, but some require fibroblasts or other tissue for analysis. It should be possible to perform a skin biopsy for fibroblast culture in most district hospitals.

The CESDI study acknowledged that lack of information was a major impediment to determining the true cause of death and makes recommendations for investigations and procedures following sudden deaths in infancy. It is disappointing that they make no reference to the collection of specimens or procedures to be followed by staff in the accident and emergency department, or wherever the death is confirmed.

It is important that paediatric residents are aware of the urgency and have a protocol for investigation and collection of specimens that has been agreed with the local coroner. In the West Midlands, we have written guidelines for managing sudden unexpected death in infancy to ensure that vital evidence of IMD, infection, or non-accidental injury is not lost.

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Table 1 Lymphocyte counts and subsets in a child with a large cystic hygroma in the neck

Lymphocyte counts (x10^9). Normal ranges in brackets

<table>
<thead>
<tr>
<th>Age at test (years)</th>
<th>CD3 (0.69 to 2.25)</th>
<th>CD4 (0.41 to 1.41)</th>
<th>CD8 (0.28 to 1.20)</th>
<th>CD19 (0.05 to 0.41)</th>
<th>CD16/56 (0.04 to 0.87)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>0.52</td>
<td>0.14</td>
<td>0.33</td>
<td>0.03</td>
<td>0.20</td>
</tr>
<tr>
<td>2</td>
<td>0.25</td>
<td>0.14</td>
<td>0.17</td>
<td>0.11</td>
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</tr>
<tr>
<td>9</td>
<td>0.05</td>
<td>0.03</td>
<td>0.03</td>
<td>0.03</td>
<td>0.04</td>
</tr>
</tbody>
</table>

The authors gratefully acknowledge the contributions from many colleagues in Birmingham, Walsall, and Wolverhampton to the development of these guidelines.


Infant air travel, bronchiolitis, and the environment

EDITOR—Probably like most doctors looking after children, we feel uneasy when asked whether it would “be alright” to take small children and infants on plane journeys for holidays. Controversy continues as to whether flying might be harmful for infants,1,2 and it is questionable whether infants benefit from weekend breaks or long distance holidays in search of better weather. However, we suspect that the air travelling population will get increasingly younger and we will be asked more frequently. As long as solid data about the safety of plane journeys for infants are lacking, anecdotal experience will be the only basis of advice.

In this context we would like to report the case of an 11 week old twin boy, corrected age 6 weeks for prematurity of 35 weeks, who was admitted to the Accident and Emergency Department of our hospital directly from an aeroplane after an emergency landing at Manchester Airport. Shortly after take off from London Gatwick for Florida the infant stopped breathing and went blue. On the plane resuscitation was attempted by the parents, a stewardess, and a paramedically trained fellow passenger. With oxygen and mouth to mouth breathing the baby’s colour improved and the plane started a surprise landing in Manchester. On arrival the
picture was typical of bronchiolitis and respiratory syncytial virus (RSV) infection was subsequently confirmed. There was a three day history of coryzal symptoms and “sniffliness”, for which a family doctor was consulted. The family had understood that the good weather in Florida would “do him good”.

Although this infant’s RSV infection might have resulted in apnoea, hypoaxaemia, and hospitalisation anyway, it seems likely that lower oxygen pressures in the aeroplane will have aggravated the symptoms. For this family the Christmas period was spent in a paediatric ward in Manchester and not in a holiday resort in Florida. Although we have no information from the airline, we assume that for the emergency landing the plane would have to empty its tanks, filled for a transatlantic distance, in order to achieve a safe landing weight. We presume these tanks will have been emptied over the Irish Sea. In addition to the potential harmful episode to the child and the inconvenience for the family, this infant’s flight probably also caused significant environmental damage.

We accept the contention of Ward Platt et al that any danger from air travel must be very small, but that may not be so for infants who are unwell, and some evidence based guidelines on this subject might be helpful. In the meantime we wonder if we should regard suspicion of bronchiolitis as reason to advise against flying.

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Recommendations for using MMR vaccine in children allergic to egg should be consistent

EDITOR,—Two reviews of measles, mumps, and rubella (MMR) vaccine and egg allergy have recently been published. One appears in the Royal College of Paediatrics and Child Health’s own journal (Archives of Diseases in Childhood), the other has been endorsed by the Committee on Infection and Immunisation of the Royal College of Paediatrics and Child Health. The two articles differ in their recommendations of which children should be given MMR under supervision in hospital. Which of these expert opinions should paediatricians and general practitioners follow? Were the authors of the two articles aware of each others’ conclusions? Could the editorial boards of the two journals (which have members common to both) not have informed the authors?

These recommendations also differ from Department of Health advice, which also differs from that given by the Health Education Authority. This debate might be settled if a consensus can be agreed and published in the next edition of Immunisation against infectious disease.

In the mean time a pragmatic approach is needed. That is to offer MMR under supervision in hospital to children who have had a severe allergic reaction to egg and a child whose general practitioners, practice nurses, or parents are unhappy for them to be given MMR elsewhere.

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Dr Lakshman and Dr Finn comment:

We note Riordan’s response to our editorial on the issue of MMR vaccine and allergy and the recommendations put forward by Khakoo and Lack on this topic. While we agree that conflicting advice creates confusion, we cannot agree with his proposed “pragmatic approach”. This amounts to a pointless waste of time and resources—greater than that proposed by anyone else to date—which will simply stoke up unfounded concerns about this vaccine, while diverting people from the important necessity to prepare themselves to tackle cases of severe anaphylaxis which, on the rare occasions that they occur, will continue to do so in community clinic settings.


Dr Marcerevich, Editor in Chief of Archives of Disease in Childhood, comments:

Dr Riordan asks which expert opinion to follow. The answer surely lies in reading the papers carefully, seeking out any key references quoted, and deciding for oneself who has provided the best evidence. This should be the case for all guidelines, but we know that they are often absorbed undigested, which is why we endorse ADC erecting fairly firm barriers to their publication. Lakshman and Finn’s paper was commissioned by the editors as a leading article because, as practising paediatricians, we recognised that there were problems responding logically to requests to immunise children in hospital.

When we commissioned this paper we did not know that a college committee was embarking on an enquiry; we learned this only after our leading article had been peer reviewed and was set up for publication. Editors of ADC have long been saddened that many of our readers, including members and fellows of the RCPCH, prefer first to submit their papers elsewhere when we realise, of course, that the artificial constraints of the research assessment exercise result in some authors needing to collect Brownie points by publishing in journals with a higher impact factor, even if their research thereby reaches an inappropriate readership. In this sense, the BMJ is our competitor, not our partner, which is why editors do not tell each other what they have in the pipeline.

I realise that this cannot have been the case in this instance as the BMJ copied Khakoo and Lack’s paper from the specialist journal in which it originally appeared (which probably has a lower score than ADC and is read by far fewer paediatricians). Dr Riordan suggests seeking a consensus. Far better would be to undertake a full literature search of RCTs and subject it to a systematic review. The days of guidelines by GOBSAT (grand old boys sitting at table) are over. At this year’s annual scientific meeting of the RCPCH, the journal and the college’s quality of practice committee have forged a working relationship that should leave our readers less confused in future.

Sputum induction for the diagnosis of pulmonary tuberculosis

EDITOR,—We read with interest the study of Zari et al on the usefulness of sputum induction in infants and young children for the diagnosis of pulmonary tuberculosis. Bacteriological confirmation of pulmonary tuberculosis in infants and young children remains a problem because it is difficult to obtain sputum. Therefore, in young children, gastric lavage is the recommended method for the collection of respiratory secretions. Since the number of tubercle bacilli and the frequency of positive cultures in specimens recovered by gastric lavage are usually small, gastric washings are induced over the first three consecutive mornings to maximise the yield.

In this prospective study, children with acute pneumonia with a high risk of pulmonary tuberculosis were included. On 142 children both gastric lavage and sputum induction was performed. The yield of M tuberculosis in sputum and gastric lavage was compared, as was the amount of positive cultures in sputum and gastric lavage. The influence of HIV status on the yield was also determined.

The authors found more positive cultures in the induced sputa compared to the gastric lavages. Therefore they conclude that sputum induction was a more effective method than gastric lavage for culture of M tuberculosis. However, in order to compare the sensitivity of two diagnostic tests, one should perform both tests in all patients. In this study, 39 patients underwent only one gastric lavage, 77 patients had lavages on two consecutive mornings, and only 26 patients underwent all three gastric lavages.

We therefore disagree with the authors on one of the conclusions, that induced sputum is better than gastric lavage for the isolation of M tuberculosis in infants and children. In our opinion, in order to answer the question whether sputum induction is as good or better than gastric lavage, only the results
from the patients who underwent gastric lavage on three consecutive days should be used.

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Dr Zar, Tannenbaum, Apollos, Hanslo, and Hussey comment:

Dr Wiersma and colleagues suggest that the yield from a single sputum induction should be compared only with the results of those children who had three consecutive gastric lavages. Only 26 of our patients had three gastric lavages; among this subset however, four children were culture positive on sputum while only three were positive on gastric lavage.

Although the yield from gastric lavage is improved with increasing number of specimens, it is frequently not feasible to perform this procedure on three consecutive days, particularly in developing countries with limited medical resources. Moreover, performing three repeated gastric lavages may be very unpleasant, both to the child and the health worker. In practice, even in tertiary institutions such as those in which our study was performed, obtaining three sequential gastric lavages is rarely feasible.

The yield from sputum induction may also be increased with increasing number of specimens. Therefore we would submit that the yield from consecutive gastric lavages should be compared with that of repeated induced sputa. Data from studies of adult patients using paired specimens of induced sputa and gastric aspirates have reported a higher yield from sputa than from gastric aspirates.

In our study, the findings that a single induced sputum specimen yielded M tuberculosis more frequently than repeated gastric lavages (in the majority of children) further strengthens our conclusion.


BOOK REVIEWS


Any publication dropping onto the doormat of a paediatrician in Manchester at present that has a front cover showing a brand new children’s hospital under a crystal clear blue sky had better be good if it hopes to receive an even handed review. Having resisted the initial desire to emigrate, I settled down to investigate whether the contents of the text matched the glossy cover. My first impulse for purchasing a book is based on the initial impression gained from a quick flick through the pages. So far so good. This handbook has a concise list of contents, well structured chapters covering the usual general paediatric topics, as well as eye catching sections on infant feeding and nutrition, pain management, the adolescent patient, and psychiatry. However, the ultimate test for any book that claims to be “a useful and practical guide for the management of sick children” is whether it proves to be just that. Many authors have claimed the above, but have produced texts that are too brief to be clinically useful, or in too much depth to provide clear and direct advice in times of trouble.

I can, without reservation, say that this publication definitely delivers. The details on clinical features, investigation, and subsequent treatment are pitched at just the right level to make it eminently useful. It enables you to confidently handle the vomiting diabetic child, develop a logical approach to the prescription of antibiotics in the pyrexial child, as well as manage less common problems such as febrile neutropenia and acute adrenal insufficiency. The chapters on fluid management, endocrinology, and infection are worth particular praise. The colour coded pages for the most important information, and drug doses are only included when essential or relevant. Any criticisms I have are minor, but would include a rather too brief chapter on cardiology and the inclusion of a section on neonatology that might have been better left to a more specialist text.

I would definitely buy this book for myself, as well as recommending it to colleagues, both junior and more senior. It has the potential to become a valued member of any acute department and I suspect it will secure a well deserved corner in the handbook market.


Descartes, the father of modern philosophy, in his pivotal work Discourse on method, published in 1637, set himself the task of doubting everything. From that starting point, he then tried to find a solid base of certainty. Over the years, epidemiologists have attempted to follow the Cartesian approach, not least those epidemiologists who are interested in that heterogeneous group of conditions called the “cerebral palsies”. To doubt everything requires imagination and courage, a setting aside of traditional wisdom to establish certainties requires scientific rigour, clear thinking and sheer hard work.

For years, the conventional wisdom has been that most children with cerebral palsy have suffered brain damage at birth, which has led to a movement disorder of varying severity with or without additional intellectual and sensory deficit. This view has been challenged from time to time, but in the last 15 years, dissenting voices have been loud and persistent. There is now a wide acceptance that not more than 10% of cerebral palsy in term babies is due to adverse intrapartum events. This recognition has opened the way to thinking about the alternative and infinitely more complex pathways that lead to this clinical entity. The problem now is to define these, to unravel the sequences of events, their temporal order, their interdependence, and their relative importance.

The authors of Cerebral palsies: epidemiology and causal pathways have taken up this challenge. In a systematic, lucid, way, they give the current data on cerebral palsy frequency, the current thinking on risk factors, and present for us a series of hypothetical causal pathways, most of which have an appealing biological plausibility. The authors are to be congratulated for their imagination and clear thinking. It is an elegantly written book, a landmark in the ongoing saga of the epidemiology of the cerebral palsies. In turn, they have thrown down a challenge for us—to test the possible pathways using sound methodological approaches, some of which, I suspect, have yet to be developed. The research agenda in this field appears to have set for a number of years to come.

But perhaps the most exciting prospect is that this painstaking epidemiological work will be much enhanced by the advances in two rapidly developing fields. First, neuroimaging techniques now provide a powerful tool for assessing the timing and the structural and metabolic changes in brain injury. Secondly, there is an increased understanding of the complex biochemical changes that both underpin the maternal, fetal, and neonatal response to infection or hypoxia, and the genetic factors which modify this response. This opens up exciting new preventive and treatment possibilities. The next edition of this book will almost certainly need to draw on a wide multidisciplinary expertise in order to encompass these major advances in our understanding of the pathophysiology of the cerebral palsies.

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