Intrathoracic extramedullary haematopoiesis complicated by massive haemothorax in alpha-thalassaemia

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Abstract

Intrathoracic extramedullary haematopoiesis (EMH) is a rare entity that is usually asymptomatic. A 44 year old man with alpha-thalassaemia is described who developed dyspnoea and massive left sided haemothorax. The haemoglobin disorder was established by Hgb H staining and haemoglobin electrophoretic studies. The DNA analysis revealed it to be a case of double heterozygous terminal codon mutation with the genotype $\omega^0\alpha^+$. Computed tomographic scanning and magnetic resonance imaging of the thorax showed multiple paravertebral masses which were found by thoracoscopic biopsy to be extramedullary haematopoiesis. Although no additional sclerosing pleuritis or low dose radiation therapy was given, the lungs expanded well and there has been no recurrence of haemothorax to date.

Intrathoracic EMH is a rare entity which is often located in the lower thoracic paraspinal area and is usually asymptomatic. We describe the case history of a patient who presented with alphathalassaemia complicated by haemothorax.

Case report

The patient, a 44 year old man, had a history of alpha-thalassaemia for some years. The disorder was diagnosed by positive haemoglobin H staining and haemoglobin electrophoretic studies (Hgb H 10.3%). DNA analysis showed a double heterozygous terminal codon mutation with genotype $\omega^0\alpha^+$. Computed tomographic scanning and magnetic resonance imaging of the thorax showed multiple paravertebral masses which were found by thoracoscopic biopsy to be extramedullary haematopoiesis. Although no additional sclerosing pleuritis or low dose radiation therapy was given, the lungs expanded well and there has been no recurrence of haemothorax to date.

Physical examination revealed a blood pressure of 146/76 mm Hg, pulse rate of 95/min, respiratory rate of 19/min, pale conjunctiva, icteric sclera, diminished left sided breathing sounds, and marked hepatosplenomegaly. Initial haematological examination showed a haemoglobin level of 6.8 g/dl, haematocrit value of 27.1%, mean corpuscular volume (MCV) of 71.9 fl, mean corpuscular haemoglobin concentration (MCHC) of 25.1 g/dl, red blood cell count of 3.4 $\times$ 10^12/l, white blood cell count of 14 $\times$ 10^9/l, platelet count of 2.7 $\times$ 10^11/l. Serum biochemical analysis gave the following values: iron 140 µg/dl, ferritin 374 ng/ml, total iron binding capacity (TIBC) 187 µg/dl, and total bilirubin 4.6 mg/dl. Chest radiography showed a massive left sided pleural effusion and posterior mediastinal masses (fig 1). Computed tomographic scanning of the chest revealed multiple lobulated paravertebral masses over the T spine with good contrast enhancement (fig 2). Magnetic resonance imaging of the thorax showed elongated lobulated paraspinal masses with isointensity to muscle on T1-weighted images and hyperintensity on T2-weighted images. Thoracocentesis revealed a bloody effusion with a protein level of 6500 mg/dl, sugar 5 mg/dl, red blood cell count 3.4 $\times$ 10^12/l, white blood cell count 2 $\times$ 10^9/l, and negative cytological results. Video-assisted
Intrathoracic extramedullary haematopoiesis with massive haemothorax in α-thalassaemia

Intrathoracic EMH commonly develops in the posteroinferior mediastinum, but has also been reported in the anterior mediastinum. It is usually asymptomatic and can be found by microscopic examination, but it may also present as a single or multiple large tumour with occasional symptoms of spinal cord compression or haemothorax.

As far as we are aware, only four patients with intrathoracic EMH complicated with haemothorax have been previously reported in the English literature. Smith et al. reported a case of thalassaemia intermedia who developed intrathoracic EMH and haemothorax which was treated with local radiation therapy to prevent recurrent bleeding. Muthuswamy and colleagues described a patient with hereditary spherocytosis who presented with post-traumatic intrathoracic haemorrhage due to intrathoracic EMH. Kupferschmid and colleagues reported a case of myelofibrosis with intrathoracic EMH. The complicated massive haemothorax did not respond to tetracycline pleurodesis and was ultimately controlled with low dose radiation therapy. Bartlett et al. described a case of agnogenic myeloid metaplasia with haemothorax. The intrathoracic EMH, confirmed by a technetium-99 bone marrow scan, was treated successfully with low dose irradiation to prevent recurrent haemothorax. In the case presented here, thoracoscopic biopsy and decortication were performed not only for the case presented here, thoracoscopic biopsy and decortication were performed not only for treatment of the thorax. Intrathoracic EMH commonly develops in the posteroinferior mediastinum, but has also been reported in the anterior mediastinum. It is usually asymptomatic and can be found by microscopic examination, but it may also present as a single or multiple large tumour with occasional symptoms of spinal cord compression or haemothorax.

In conclusion, based on the characteristic radiographic findings and radionuclide marrow scanning, it is important to recognise the possibility of intrathoracic EMH as a differential diagnosis of non-traumatic haemothorax, especially in patients with bone marrow insufficiency or chronic haemolytic anaemia. Although radiation therapy or sclerosing pleurodesis is suggested for recurrent haemothorax,
FEV1 from the PEF or vice versa.” This is a parameters has been reported by others.

In general practice, especially in the early stages.

The only citation among the 171 references is a paper by Kelly and Gibson.

The 1962 article by Ritchie dates from the infancy of FEV1 and long before COPD.

The 1962 article by Ritchie dates from the infancy of FEV1 and long before COPD.

The patient’s FEV1 is markedly reduced to 0.8 l (33%) while the PEF is relatively preserved at 5.7 l/s (340 l/min) which is 80% predicted. As expiration begins (point “a”) there is a rapid increase in expiratory flow until the flow becomes limited by the airway dimensions and peak flow is reached (point “b”). As expiration continues, the healthy subject in stable condition, flow decreases slowly and progressively until the residual volume is reached when flow ceases (point “c”). In the patient with COPD the initial rapid rise in expiratory flow is similar but, as the intrathoracic pressure increases, that pressure is transmitted to the segmental and other large airways which have lost the elastic attachments.

The most important and fundamental point is that PEF cannot differentiate between obstructive and restrictive patterns of abnor-

The FEV1 is a more reproducible measurement so that measurements on a single occasion can be of value whereas, to obtain similar accuracy with PEF, serial measurements are required. By inspecting the FEV1 traces it is possible to know whether a patient has performed the manoeuvre correctly, whereas no such confirmation exists for PEF. Even in asthma, studies of repeated measurements of serial PEF using computerised measurements confirm that up to 50% of readings may be non-valid.

The PEFR in managing COPD. They attack one specific aspect but unfortunately misquote the arguments in a paper by Kelly and Gibson.

The 1962 article by Ritchie dates from the infancy of FEV1 and long before COPD.


The majority of GP’s might be persuaded to resort to the use of spirometry in managing COPD.

In COPD the relationship between PEF and FEV1 is poor and it is not possible to predict FEV1 from the PEF or vice versa. This is a key issue for GPs who have to decide whether or not to purchase a spirometer, and whether they have the organisational capacity to cope with the maintenance, calibration, and interpretation demands of modern spirometers.

The close relationship between FEV1 and PEF is reassuring to us because the arguments put forward by the COPD guidelines seem counter-intuitive to GPs working daily with PEF in asthma. We recognise the role of spirometry as a whole in the diagnosis of COPD, especially in distinguishing primarily restrictive from obstructive disease. In the continuing management of COPD, however, we suspect that spirometry has little additional value to offer over PEF, but considerable practical disadvantages.

Letters to the Editor

FEV1 and PEF in COPD management

Chronic obstructive pulmonary disease (COPD) is a common disease usually treated in general practice, especially in the early stages. The recently published British Thoracic Society guidelines encourage a systematic approach to the management of COPD as is widely used in asthma. Lung function measurements are regarded as central to the correct implementation of the guidelines. The guidelines are unequivocal in advising the use of forced expiratory volume in one second (FEV1) rather than peak expiratory flow (PEF) in the management of COPD. ‘...in COPD the relationship between PEF and FEV1 is poor and it is not possible to predict FEV1 from the PEF or vice versa.’ This is a key issue for GPs who have to decide whether or not to purchase a spirometer, and whether they have the organisational capacity to cope with the maintenance, calibration, and interpretation demands of modern spirometers.

We have investigated the literature examining the relationship between FEV1 and PEF and exploring their use in COPD. We have been able to find substantive evidence to support the statement in the BTS guidelines regarding the superiority of FEV1 over PEF. The only citation among the 171 references offered in the guidelines to support their position is a paper by Kelly and Gibson. In fact, Kelly and Gibson state the opposite view and report a very strong correlation between FEV1 and PEF with an r value of 0.95 (p<0.001). A similarly strong relationship between the two parameters has been reported by others. 

The close relationship between FEV1 and PEF is reassuring to us because the arguments put forward by the COPD guidelines seem counter-intuitive to GPs working daily with PEF in asthma. We recognise the role of spirometry as a whole in the diagnosis of COPD, especially in distinguishing primarily restrictive from obstructive disease. In the continuing management of COPD, however, we suspect that spirometry has little additional value to offer over PEF, but considerable practical disadvantages.

AUTHORS’ REPLY Drs Nolan and White are concerned that GPs might be persuaded to resort to the use of spirometry in managing COPD. They attack one specific aspect but unfortunately misquote the arguments in a paper by Kelly and Gibson. In fact, Kelly and Gibson apply to a previous 61 patients undergoing routine testing (not all with COPD) and not to the 10 subjects with COPD and a positive steroid trial in whom the relation changes in PEF and FEV1 do not exhibit the same slopes. Liebowitz studied 10 healthy individuals and none with COPD. The 1962 Lancet article by Ritchie’s dates from the infancy of FEV1, and long before COPD was defined as a discrete entity.

The most important and fundamental point is that PEF cannot differentiate between obstructive and restrictive patterns of abnormal function. If the diagnosis is not made correctly then the GP cannot hope to manage the patient correctly. The implications of a restrictive defect will often necessitate referral to secondary care to assess the cause, whereas most cases of COPD are manageable within primary care. The FEV1 is a more reproducible measurement so that measurements on a single occasion can be of value whereas, to obtain similar accuracy with PEF, serial measurements are required. By inspecting the FEV1 traces it is possible to know whether a patient has performed the manoeuvre correctly, whereas no such confirmation exists for PEF. Even in asthma, studies of repeated measurements of serial PEF using computerised measurements confirm that up to 50% of readings may be non-valid.

To understand the relationship between the level of PEF and the level of FEV1, it is necessary to go—not to epidemiology—but to the physiology underlying the shape of the flow-volume loop in COPD. In the first draft of the guidelines we included a figure illustrating how the FEV1 could be reduced to 33% of predicted at a time when the PEF remains relatively preserved at 60% of predicted. The discrepancy arises because of the airway collapsibility present in COPD secondary to the loss of elastic tissue. The PEF is generated by the instantaneous flow of air leaving the trachea in the first 0.1 seconds of expiration, while the FEV1 includes air leaving the Airways through Airways that have collapsed after about 0.2 seconds of expiration (fig 1). In the example shown a patient with severe COPD (lower line) is compared with the predicted normal pattern (upper line). The patient’s FEV1 is markedly reduced to 0.8 l (33%) while the PEF is relatively preserved at 5.7 l/s (340 l/min) which is 80% predicted. As expiration begins (point “a”) there is a rapid increase in expiratory flow until the flow becomes limited by the Airways dimensions and peak flow is reached (point “b”). As expiration continues, the healthy subject in stable condition, flow decreases slowly and progressively until the residual volume is reached when flow ceases (point “c”). In the patient with COPD the initial rapid rise in expiratory flow is similar but, as the intrathoracic pressure increases, that pressure is transmitted to the segmental and other large Airways which have lost the elastic attachments. The Airways therefore “collapse” and obstruct the passage of air through those Airways. This results in the rapid reduction in flow after the peak has been attained (point “d”). Flow in the remainder of the expiration remains limited. The effect of the expiratory Airways collapsibility is shown by the time points marked. The subject with COPD reaches peak flow at about 100 ms, flow is at the very low level shown. As the Airways collapsibility varies between COPD patients, the relationship between PEF and FEV1 will also vary. Because PEF can be misleadingly optimistic it is severely limited as a diagnostic tool. This figure was edited out of the guidelines, possibly on the mistaken grounds that it was too obvious a point.
Health effects of passive smoking

Cook and Strachan are to be congratulated on their series of meta-analyses on the health effects of passive smoking. However, in their analysis of parental smoking and spirometric indices they gave as the main reason for excluding 19 out of 42 studies that met their primary criteria that they “provide some data, but insufficient to be included in the quantitative overview”. In the case of our own study they concluded that they were unable to transform our results to the desired effect measure. They used the “difference in outcome measure between the exposed and non-exposed children expressed as a percentage of the level in the non-exposed group” and reported that they were unable to do this with our results as we “reported differences in standard deviation scores with no baseline data”.

The standard deviation scores were calculated using the mean and standard deviation of the ratio of actual to lung function predicted for height, age and sex. Hence, the approximate percentage difference can be calculated by multiplying by the appropriate published standard deviation. Using an estimate of 15.6 cigarettes per day for the average amount smoked by parents at home, calculated from the same data for white English children in 1988,1 the effect of parental smoking on forced expiratory volume in one second (FEV1) was −0.37% (SE 0.51%) for boys and −0.18% (SE 0.76%) for girls. The wide confidence intervals on our estimates encompass the greater negative estimates of Cook et al., but inclusion of our results would have decreased our negative estimates for all four lung function parameters.

The approximation in assuming FEV1, percent predicted to be 100 for the unexposed group is no greater an assumption than combining studies using different definitions of parental smoking and different measures of mid-expiratory flow. We invite Cook et al. to update their estimates accordingly.

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AUTHORS’ REPLY The omission of the study by Rona and Chinn from our meta-analyses is not an indictment of their study, but simply a reflection of the way the data were presented. It arose because the standard deviation necessary to transform the estimates in their paper to percentage deficits was not provided in that paper but published elsewhere. This is unlikely to have occurred in any of the other studies excluded. Updating our estimates to include their study serves to emphasise the robustness of our estimates to exclusion of individual studies. The fixed effects estimate for percentage reduction in FEV1 amongst children in smoking households moved from −0.9% (95% CI −1.2 to −0.7) to −0.9% (95% CI −1.1 to −0.7) and the random effects estimate from −1.4% (95% CI −1.9 to −1.0) to −1.3% (95% CI −1.8 to −0.9).

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DAVID STRACHAN
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Investigation and management of persistent dry cough

McGarvey et al. suggest that full investigation of patients with persistent cough improves treatment. I reviewed 100 such patients seen consecutively. All had normal chest radiographs, two were current smokers, and their mean cough duration was 18.8 months (range one month to 20 years). Initial treatment was given based on the history of baseline and routine clinical examination with investigations reserved for patients not responding after one month. Thirty four patients failed to return after their initial appointment. Twenty were current smokers and all reported complete resolution of their symptoms. Clinical diagnoses in the 14 others were similar and they probably defaulted because of improvement, but none were reinvestigated from analysis. Investigations performed included radiology of the sinuses in 8%, bronchial provocation testing in 16%, and investigation for gastro-oesophageal reflux in 19%. The final diagnosis (table 1) was based on successful response to treatment. Asthma was uncommon (7%) but, as there were few treatment failures, it seems unlikely that asthma was missed. The awareness of asthma by GPs is high in Australia and had probably been treated by their GPs. Clinical outcomes were excellent with 79 patients (92%) reporting complete or almost complete resolution of cough in a mean of two months.

These results suggest that good outcomes can be achieved in most patients without routine investigation. The poor positive predictive values of symptoms quoted by McGarvey et al. reflect poor choice of historical features. These authors confirm that any cause of chronic cough increases the sensitivity of the cough reflex, and the finding that cough precipitated by GPs is high in Australia and had probably been treated by their GPs. Clinical outcomes were excellent with 79 patients (92%) reporting complete or almost complete resolution of cough in a mean of two months.

DIAGNOSTIC PROTOCOLS Table 1 Final diagnosis

<table>
<thead>
<tr>
<th>Diagnosis</th>
<th>Mean age (range)</th>
<th>M/F</th>
</tr>
</thead>
<tbody>
<tr>
<td>Rhinitis</td>
<td>24 (7–75)</td>
<td>8/16</td>
</tr>
<tr>
<td>Reflux</td>
<td>19 (50–69)</td>
<td>7/12</td>
</tr>
<tr>
<td>Postural</td>
<td>11 (95–7)</td>
<td>4/7</td>
</tr>
<tr>
<td>Reflux + rhinitis</td>
<td>6 (44–64)</td>
<td>4/1</td>
</tr>
<tr>
<td>Whooping cough</td>
<td>5 (37–64)</td>
<td>4/1</td>
</tr>
<tr>
<td>ACEI inhibitor (ACEI 1)</td>
<td>4 (69–79)</td>
<td>2/8</td>
</tr>
<tr>
<td>ACEI 1 + rhinitis + reflux</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td>ACEI 1 + reflux</td>
<td>2</td>
<td></td>
</tr>
<tr>
<td>ACEI 1 + reflux</td>
<td>3</td>
<td></td>
</tr>
<tr>
<td>Asthma</td>
<td>5 (30–68)</td>
<td>4/2</td>
</tr>
<tr>
<td>Asthma + rhinitis</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td>Interstitial lung disease</td>
<td>1 (78)</td>
<td>1/0</td>
</tr>
<tr>
<td>Chronic bronchitis</td>
<td>2 (72–81)</td>
<td>0/2</td>
</tr>
<tr>
<td>No diagnosis</td>
<td>2 (54–71)</td>
<td>0/2</td>
</tr>
</tbody>
</table>

GRACE SIMPSON
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AUTHORS’ REPLY We welcome Dr Simpson’s interesting comments. He describes a group of patients which appears to be rather different from the patients reported in our study. Firstly, our patient group had been troubled with cough for a longer period of time (mean cough duration 67 months (range 2–240)


compared with 18.8 months (range 1–240)). Secondly, application to his study of our exclusion criteria—that is, smokers, an abnormal chest radiograph, any preceding viral infections, and patients taking angiotensin converting enzyme inhibitors—would mean that 29 of the 86 patients (33%) he reviewed would not have been included in our study. Dr Simpson relies heavily on patient history in the evaluation of his patients. In our discussion, we highlighted the limitations of historical features, given the existence of both silent “reflux” and postnasal drip. We do not accept that the poor positive predictive values reflect a bad choice of historical features and believe there are no accurate discriminatory historical features that can be reliably applied to cough patients in general. This is supported by a study which specifically examined features in the clinical history and found that these were unlikely to be useful in diagnosing the cause of cough.1

While we agree that a randomised controlled trial may be one way to address the issue of how best to evaluate patients with cough, we suspect that Dr Simpson is describing a very different patient population from those referred to our cough clinic and that a less interventionist approach may not therefore be appropriate. In the meantime we feel a comprehensive protocol—which is consistent with the approach of the recent Consensus Panel Report of the American College of Chest Physicians2 continues to represent the optimum way to evaluate patients referred with chronic cough.

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Targeting DNase in cystic fibrosis

Recombinant human DNase is an expensive mucolytic which does not benefit all patients with cystic fibrosis. Company sponsored trials in unslected cystic fibrosis patients have documented wide variability in spiro-metric responses to the drug, but the data are presented in a way which prevents the clinician from assessing which patients are likely to benefit. We therefore read with interest the editorial by Dr Innes regarding the assessment of response to DNase in cystic fibrosis.3 However, whilst we agree that it is necessary to target DNase, we have reservations regarding the use of “n-of-1 trials” for this therapy. Dr Innes states that this approach has been used in Scotland and quotes a study unpublished at the time of writing in support of it. However, this study has already been heavily criticised since many patients refused to take part and others did not complete the trial periods. Furthermore, such studies are inherently time consuming and resource intensive.

We have adopted a different approach to ensure that DNase is prescribed in a rational fashion. Before it became available on the NHS we met with local purchasers to define selection criteria and a trial protocol. Following selection, those who have an improvement in forced expiratory volume in one second (FEV1) of >10% after a trial of DNase are defined as “responders” and remain on the drug. A review at two years has shown that, whilst responders maintain their improvement, non-responders are not disadvantaged.4 Thus, using this protocol we have been able to target DNase to those patients who obtain maximum benefit. This model has now been widely accepted by purchasers for adult and paediatric cystic fibrosis services in North Wales and the Northwest of England and, as such, we have no problems in obtaining funding for this very expensive product.

We suggest that Dr Innes and his colleagues abandon their “n-of-1 trials” and adopt our protocol for the use of DNase.

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AUTHOR’S REPLY We would urge Drs Ledson and Walshaw, before suggesting we abandon the Scottish n-of-1 DNase assessment protocol, to read it! The protocol has not been “heavily criticised” as they claim since the article they quote was also written without knowledge of the results (only recently published). In our experience it is very unusual for patients to refuse to undergo our assessment process.

Where do we agree in the need to test the DNase response in individual patients. However, we disagree on how this should be done. Ledson and Walshaw advocate an unblinded, open label, two week trial of DNase using an increase in FEV1 of >10% as the only end point. We contend that this is less than ideal because (a) double blinding and placebo control periods are needed to obviate bias, given the high expectations generated in patients and carers by new treatments for cystic fibrosis; (b) using the single end point of increased FEV1, may be less reliable than combining this with other measures including exercise capacity, oxygen saturation and symptom scores; (c) a criterion of a >10% increase in FEV1 is inherently unreliable since day-to-day variability in FEV1 is around 160 ml (95% CI) regardless of the magnitude of the FEV1; so it is easy for patients with a low FEV1 to achieve an increase of >10% by chance. Indeed, Ledson et al in the description of their own protocol quote the day-to-day variability of FEV1, in cystic fibrosis as “up to 13%”, so clearly some 10% increases will be spurious. We agree that DNase can and should be targeted to maximise benefit, but feel that this targeting should be made as objective as possible. This may be laborious for doctors, but it is not nearly as laborious for patients as consigning a non-responding to long term daily nebulised therapy on unreli able evidence. J ALASTAIR INNES
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Corrections

In the “Smoking Cessation Guidelines and their Cost Effectiveness” which was published as a supplement to the December issue of Thorax (December 1998;54(Suppl 5)), the name of one reviewer was inadvertently omitted from the list of reviewers on page 1 of Part 1:

Gay Sutherland, Clinical Psychologist, National Addiction Centre, Institute of Psychiatry, University of London, London, UK.