Conservative management of a transdiaphragmatic fistula

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Abstract
Case reports of transdiaphragmatic fistulas connecting subphrenic collections and empyemas are uncommon. We report the rare complication of a fistulous connection between a subphrenic collection and the bronchial tree.

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Keywords: transdiaphragmatic fistula; subphrenic abscess

Case report
An 82 year old man was admitted following an episode of coffee ground vomiting. He had experienced a dull ache in the epigastrium for one week and had a history of a duodenal ulcer 20 years previously. He looked pale with a regular pulse of 100 bpm and there was mild epigastric tenderness. His haemoglobin was 123 g/l.

Gastroscopic examination showed a dilated stomach with fluid residue and the pylorus was narrowed to a pinhole; the duodenum could not be seen. He was started on lansoprazole and over the following week his condition stabilised. His haemoglobin fell to 107 g/l but he did not pass melaena.

He then became unwell with a pyrexia of 38°C, was tachypnoeic, and developed coarse crackles in the right lower chest. A diagnosis of right basal pneumonia was made and he was treated with intravenous cefuroxime and clarithromycin. Blood cultures were taken which subsequently grew a coagulase negative Staphylococcus aureus and Enterococcus spp., thought to be a skin contaminant.

His temperature settled and his chest radiograph showed only patchy shadowing at the right base. An abdominal ultrasound scan, requested because of an increase in the serum level of alkaline phosphatase, was normal. His condition improved over the next four weeks and his chest signs and symptoms resolved. He had no further vomiting and was discharged home.

Six weeks later he was re-admitted with a haemoglobin of 59 g/l. He was transfused with six units of blood and discharged home three days later with a haemoglobin of 134 g/l. A barium enema examination was ordered to investigate further the cause of his recurrent anaemia.

Two weeks later he was re-admitted with a one week history of a cough productive of purulent sputum. On examination he was apyrexial, had peripheral oedema, and the right base was dull to percussion and associated with decreased breath sounds. Abdominal examination was normal.

His haemoglobin was 125 g/l, white cell count 6.5 × 10^9/l, and chest radiography showed cardiac failure with upper lobe venous diversion and bilateral pleural effusions, with a homogenous opacity at the right base which was thought not to be entirely caused by the effusion.

He was started on co-amoxiclav and erythromycin but attempted intercostal aspiration of the pleural fluid was unsuccessful. A barium enema was performed and indicated early diverticular disease in the sigmoid colon and a tiny outpouching at the hepatic flexure with some possible tethering. The significance of this was uncertain but possible causes included the previous perforation of a colonic diverticulum.

Bronchoscopic examination of the right bronchial tree indicated some inflammatory changes in the right lower lobe. A foul faeculent smell was noted and the possibility of a transdiaphragmatic fistula was raised. An abdominal ultrasound examination demonstrated a 12 cm collection of fluid lateral to the liver, suggestive of a subphrenic abscess. Later that day a pigtail drainage catheter was inserted into the subphrenic collection and thick pus was drained.

Two days later a tubogram was performed to assess the size of the abscess cavity. A small subphrenic cavity was demonstrated before the patient started to cough up contrast medium. Radiography showed a fistula between the subphrenic cavity and the bronchial tree (fig 1).

Pus from the subphrenic abscess cavity yielded heavy growths of methicillin resistant Staphylococcus aureus and Enterococcus spp. Both sputum and bronchial washings yielded methicillin resistant S. aureus. The patient was treated with teicoplanin and the cavity drained externally. Subsequent tubograms over the following week showed that the fistula had closed and the abscess cavity became smaller.

Following drainage of the abscess the patient made a good recovery and his chest symptoms resolved. He was discharged home and was well when seen two months later.

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Mechanism of osteoporosis in patients with cystic fibrosis

In a recent issue of Thorax Haworth and coworkers reported that low bone mineral density—that is, osteoporosis—commonly occurred in patients with cystic fibrosis. These results supported previous reports of osteopenia in patients with cystic fibrosis. However, the mechanism of bone loss in these patients has not been elucidated.

Although the pathogenesis of osteoporosis is probably multifactorial in patients with cystic fibrosis, the increased production of cytokines—primarily tumour necrosis factor-α (TNF-α)—may play a critical role in adult patients. Among the important factors implicated in the pathogenesis of bone loss are circulating cytokines such as TNF-α, interleukin (IL)-1, and IL-6. TNF-α is a potent inhibitor of bone collagen synthesis and stimulator of osteoclastic bone resorption, the net effect of which is to cause bone loss. Experimental animal studies have also shown that the neutralising antibody to TNF-α slowed the bone elongation rate and bone marrow hyperplasia, and decreased trabecular bone volume. It has been reported that the production of TNF-α by lung macrophages is increased in patients with cystic fibrosis. The increased production of TNF-α is also implicated in the pathogenesis of weight loss and cachexia in various diseases. Because body weight is associated with bone mineral content in normal subjects and those with cystic fibrosis, the weight loss or cachexia associated with increased production of TNF-α may also be involved in the pathogenesis of bone mineral deficit in these patients. Taken together, the increased production of cytokines, particularly TNF-α, may be a contributing risk factor for bone loss in patients with cystic fibrosis. It is therefore reasonable to assume that the inflammatory cytokines may prevent the development of osteoporosis. The putative mechanism of bone loss is partly explained by the recent study in which treatment with an oral corticosteroid reduced both pamidronate induced bone pain and the level of TNF-α in patients with cystic fibrosis. The measurement of serum levels of cytokines including TNF-α may therefore provide a means of identifying cystic fibrosis patients who are at risk of rapid bone loss.

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3 Teramoto S, Matsuse T, Ouchi Y. Increased cytokines may be responsible for the pamidronate-induced bone pain in adult patients with cystic fibrosis. Lancet 1999; 352:1753–4.

AUTHORS’ REPLY We thank Shinji Teramoto for his continued interest in our work and note his current (and previous) comments about the possible role of proinflammatory cytokines in the development of low bone mineral density in patients with cystic fibrosis. As stated in our discussion, four proinflammatory cytokines: interleukin 1, interleukin 6 may influence osteoclast activity in patients with cystic fibrosis. In fact, the significant negative relationship between the mean bone mineral density T score and the C reactive protein concentration in our study provides the first evidence of an association between chronic pulmonary inflammation/infarction and low bone mineral density in the cystic fibrosis population.

Cystic fibrosis patients with low bone mineral density do not necessarily have osteoporosis. It is important to emphasise that the precise histomorphometric characteristics of cystic fibrosis bone have not been comprehensively described. In our study 38% of patients were vitamin D insufficient, which may predispose them to osteoporosis, but 7% of patients had 25-hydroxyvitamin D levels associated with osteomalacia. Thus, some patients could have both osteoporosis and osteomalacia. Furthermore, a recent report suggested that the bone disease of cystic fibrosis was complex and possibly unique.

We have previously reported that bone pain is common in patients with cystic fibrosis after intravenous pamidronate administration, and that it might be prevented by the concomitant use of oral corticosteroids. It is important to clarify that this was a retrospective observation and has not been evaluated prospectively.

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1 Teramoto S, Matsuse T, Ouchi Y. Increased production of TNF-α may play a role in osteoporosis in cystic fibrosis. Chest 1997; 112:574.
Spirometry in primary care

Contrary to Dr Pearson’s assertion in his reply in the May 1999 issue of Thorax, spirometry and forced expiratory flow volume in one second (FEV1) are not yet here to stay in general practice.1 The evidence is just not good enough. We were concerned by the inaccuracies in his letter which simply serve to underpin our unease with the chronic obstructive pulmonary disease (COPD) guidelines. We will come to these later but, firstly, how is FEV1 related to peak expiratory flow rate (PEFR) in patients with COPD? Is FEV1 really essential in the day to day management of COPD? Spirometry is central to the accurate diagnosis of COPD in many patients. The ratio of FEV1 to FVC is indispensable in differentiating restrictive from obstructive patterns of collapse during forced expiration whereas PEFR represents the maximal flow rate (usually attained in the first 100 milliseconds). Both are derived from the flow rate, but the nub of the argument is what happens after the peak is reached. Pearson suggests that one needs to go, not to epidemiology, but to physiology to understand this issue. He disagrees considerably from the flow-volume diagram (albeit mislabelled) which he has own offer additional information about COPD which is not available with PEFR. FEV1, represents the volume of air expired during the first second of forced expiration whereas PEFR represents the maximal flow rate which is certainly the case in the diagram (albeit mislabelled) which he presents. However, his flow-volume trace differs considerably from the flow-volume curves produced by many other researchers and this is the lack of sound epidemiological evidence to support his argument which makes his conclusions seem unsound.2 Patients with obstructive airway disease demonstrate some degree of airway collapse during forced expiration which is partly explained by the loss of elastic support of alveoli and respiratory bronchioles. In fact, the pattern of the flow-volume curve can be almost identical for asthma and COPD and this is the reason for the close correlation observed between FEV1 and PEFR.2 When the absolute values of FEV1 and PEFR are compared, the correlation is high (r = 0.78–0.95).3 Little work has been done on the correlation between PEFR and FEV1 for FEV1 values of less than one litre. It is conceivable that the strength of correlation will be reduced at low levels of FEV1 or PEFR. But what is the significance of this possibility? Symptoms and signs rather than FEV1 guide the management of COPD in the majority of cases. We have little treatment to offer at present to patients with advanced COPD.

While spirometry offers a significant advantage to primary care physicians and nurses in the diagnosis of COPD, it is unlikely to have a role in the day to day management of this disease. The provision of spirometry in primary care would have significant financial and organisational implications which cannot be justified on current evidence. On this basis, open access to lung function laboratories seems preferable to a primary care based service. Spirometry in primary care risks inaccuracy from flawed lung and operator techniques due to infrequent use and potential failure to calibrate an electronic spirometer regularly.

Finally, a brief word about Dr Pearson’s reply to our letter. We were disappointed that he accused us of misquoting from Kelly and Gibson.4 Dr Pearson is simply wrong in stating that there were 10 subjects with COPD. Kelly and Gibson mentioned eight patients (with a positive steroid trial and therefore presumably not COPD), and in these the correlation coefficient between individual FEV1 and PEFR values remains high (r = 0.98). We were also surprised that he criticised the quotation of the paper by Richie from the Lancet on the basis of the date of publication.5 This has surely nothing to do with the validity of the data. Furthermore, Lebowitz’s observation that the FEV1 and PEFR show close correlation in healthy individuals emphasises the reliability and reproducibility of PEFR6.7

There is no evidence that PEFR is more reproducible on a single occasion than PEFR. The papers by Malo et al and Verschelden et al relate to home recordings and hardly seem relevant to this issue. We refer Dr Pearson to another paper by Malo in which he showed that FEV1, is similar to PEFR in terms of non-valid recordings.8 Finally, FEV1 has not been shown to be superior to PEFR as a prognostic tool and, in fact, the evidence suggests that PEFR may be a better indicator of short-term mortality.9

The basis for using spirometry rather than peak flow in the day to day management of COPD demands a more rigorous approach than is evident from either the COPD guidelines or Dr Pearson’s letter. The issue is far from resolved and current evidence is an inadequate basis on which to recommend the widespread practice of spirometric testing in primary care.

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AUTHOR’S REPLY

Dr White and Nolan agree that spirometry is essential for the diagnosis of COPD and, on this basis alone, I would stand by the recommendations and guidelines that GP need to have access to spirometry. The COPD guidelines set out three options: GP owned spirometers, a mobile service visiting practices intermittently, or open access services at hospital. Which ever option is preferred locally, the spirometric tests must be performed by trained staff to ensure adequate quality control. Single measures of peak expiratory flow rate (PEFR) are not adequate. It is colleagues in primary care who have encouraged those involved in guidelines to believe that PEFR would provide an appropriate measure in their practice. The BTS COPD consortium has been promoting spirometry for the diagnosis of COPD since, without a correct diagnosis, the chance of accurate treatment is low. How the service is provided is a matter of local choice, not for national dictate.

With regard to the day to day management of COPD after the diagnosis has been objectively made, Drs White and Nolan are concerned that spirometry may become part of the work. In fact, no-one is recommending frequent regular spirometric testing. Serial measurements of FEV1, can and do provide evidence of progression over the years, but cannot detect changes over periods of less than a few years with any degree of confidence because the rate of decline is small relative to the coefficient of variability for the measurement. Since PEF is even more variable, it is of even less value to use PEF as an outcome measure, except in those with an asthmatic element where large changes may be apparent within days. The assessments of a bronchodilator drug outcome in clinical COPD practice are therefore subjective responses. Research studies employ more objective health status measures.

I do not wish to get into an argument about the relative merits of PEF and FEV1, which could fill many pages. I will continue to disagree on many of their points. I will confine myself to two issues.

Anyone who measures PEFR and spirometry on a regular basis will recognise that PEFR is a much more limited and more variable measure.1 The issue has probably been considered too obvious to justify formal studies. It is important to be careful, when looking at such comparator data as do exist, to ensure the correct method of statistical analysis has been performed. Bland and Altman set out the reasoning clearly, and their arguments are particularly relevant to the use of correlation coefficients such as PEF and FEV1. For two measures that are measured on the same predictors (age, sex and height), there will always be a linear regression correlation present as long as patients of different age, sex, and height are included.

Secondly, the use of PEFR in asthma is of greatest value when recorded as serial measurements several times per day. The repeated measures compensate for the high variability of individual readings and provide data on symptomatic episodes outwith the clinic and on large changes of airway size that are typical of asthma. Serial PEFR is less helpful in COPD with its reduced natural variability of airflow dimensions. The second paper by Malo compared home spirometry with home PEFR and noted poor compliance for both. The comparison that would be relevant is with supervised spirometry recorded by trained staff, which is why the guidelines emphasise the need for staff training.

Finally, I am heartened by data presented at the recent British Thoracic Society meeting in which spirometry was used in primary care as a screening tool in a research project. It offered spirometry to all adults over 45 attending the surgery (smokers and non-smokers) and detected 6% of those studied as having undiagnosed symptomatic COPD, a high yield of treatable disease from...
An inexpensive programme. Other primary care studies are in progress. I remain of the view that, in time, all GPs will have easy access to spirometry and be able to interpret the results as efficiently as they presently measure and manage blood pressure or blood sugar.

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Asthma guidelines

One problem with large studies that focus on a limited number of patient relevant outcomes endorsed by Tattersfield and Harrison is that the mean response to a given drug may hide a wide variability in individual response. This is particularly likely to occur in patients with symptomatic asthma a discrete treatment with low/moderate doses of inhaled corticosteroids. Studies have shown marked heterogeneity of airway inflammation and disordered airway function in these patients and there are wide differences in the effect of the candidate drugs on these measures. Smaller crossover studies comparing the effects of different treatments in patients who have been extensively characterised are needed to establish whether important heterogeneity of response occurs. Such studies may uncover easily identifiable patient characteristics that predict a response to an individual drug.

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AUTHORS’ REPLY Patients with asthma clearly vary in their response to drugs and it is possible that smaller detailed studies will allow the identification of patients more likely to respond to a particular drug. For such a test to be useful in practice, however, it would need to be easily accessible, reliable, and have a very high predictive value. History suggests that it is often more practicable for the patient to undergo a trial of a drug, rather than to undergo an indirect assessment which is unlikely to be 100% predictive of the response and hence may result in some patients not receiving a drug from which they would benefit.

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Respiratory questionnaires in COPD

The use of health status as an outcome measure in chronic obstructive pulmonary disease (COPD) is becoming more popular. We therefore welcome the publication of information which improves the choice of appropriate questionnaire. The recent paper by Rutten-van Molken and colleagues could have made an important contribution to this area. However, we are seriously concerned about the validity of their comparison between the St George’s Questionnaire (SGRQ) and the Chronic Respiratory Questionnaire (CRQ).

The clinical usefulness of the CRQ is limited because it is interviewer led while the SGRQ is self-administered. The development of a validated self-administered version of the CRQ would be a major advantage for clinical trials and clinical practice. The authors give the impression that a validated self-administered version of the CRQ already exists and have used this in their study. However, such a version is not described in the original reference as claimed and has never been disseminated by publication.

We have recently been working with the original authors of the CRQ to develop and validate a self-reported CRQ and the results have so far only been published in abstract form. We therefore believe that the results described in the paper are devalued by the misleading implication that a self-report version of the CRQ has been correctly developed.

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1 Rutten-van Molken M, Roos B, Van Noord JA. An empirical comparison of the St George’s Respiratory Questionnaire (SGRQ) and the Chronic Respiratory Disease Questionnaire (CRQ) in a clinical setting. Thorax 1999;54:995–1003.
3 Williams J, Singh S, Morgan MDL. A comparison between a self-reported Chronic Respiratory Questionnaire (CRQ-SR) and the conventional interviewer led CRQ. Eur Respir J 1999;14(Suppl 30):116s.

AUTHORS’ REPLY We thank Dr Henze for this correction. As in the case of formoterol, there is no scientific reasoning given by the IOC for fenoterol not to be permitted by inhalation, in contrast to salbutamol, terbutaline, and salmeterol. There are no specific pharmacological or pharmacodynamic characteristics of inhaled fenoterol or formoterol that would predict a relevant stimulating effect on the respiratory system. For clarity we reproduce here the correct list of permitted respiratory drugs.

Athletes and fenoterol

In a review of doping in athletes by Dr Dekhuizen published recently in Thorax the substance fenoterol was included in table 2 which listed respiratory drugs permitted by the International Olympic Committee (IOC). However, according to the IOC fenoterol is prohibited. Only salbutamol, salmeterol, and terbutaline are permitted by inhaler to prevent or treat asthma or exercise induced asthma. A written notification is necessary.

To prevent positive doping cases caused by misinformation of athletes and their advisors which might cause questions of regrress, it is necessary to print a correction.

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AUTHORS’ REPLY We thank Dr Henze for this correction. As in the case of formoterol, there is no scientific reasoning given by the IOC for fenoterol not to be permitted by inhalation, in contrast to salbutamol, terbutaline, and salmeterol. There are no specific pharmacological or pharmacodynamic characteristics of inhaled fenoterol or formoterol that would predict a relevant stimulating effect on the respiratory system. For clarity we reproduce here the correct list of permitted respiratory drugs.
Asthma education

Drs Neville and Higgins ask what more can be done to provide better asthma care. They mention the importance of education but, before we can teach patients, we must resolve our own confusion about treatment.

Evidence shows that it is important to stamp out the inflammatory process in the bronchial tree as soon as the diagnosis of asthma has been confirmed, yet patients are still prescribed a short acting β agonist bronchodilator as the drug of first choice. This is given partly as a diagnostic test and achieves symptom control with full reversibility and optimal lung function. The dose is then gradually reduced to the minimum necessary to maintain optimum lung function and freedom from symptoms. Beta agonist bronchodilators are not prescribed initially but kept in reserve for emergencies.

The latter approach is being increasingly adopted by asthma nurses and many thinking doctors but some still misinterpret the BTS guidelines and allow their patients to become addicted to β agonists. Perhaps we need guidance on the use of the guidelines?


gate the step most appropriate to the initial severity or greater, evidence certainly exists in asthma of moderate severity or greater, but the situation is less straightforward in patients with mild asthma where the case is unproven. Since there are valid reasons, aired elsewhere, for not using inhaled steroids unless necessary, it is fair to ask for some proof before committing patients to long term therapy in this way.

In addition to the lack of evidence, the approach advocated by Dr Strube assumes a certainty of diagnosis which many would feel to be unrealistic at the mild end of the asthma spectrum. It is easy to write that inhaled steroids should be started “as soon as the diagnosis has been confirmed by PFR monitoring”, but this is an insensitive test which is least likely to confirm asthma in those in whom the need for inhaled steroids is most debatable.

A further part of his argument is that the current guidelines on introduction of inhaled steroids are misused. This, as Mike Rudolf pointed out in the published debate with Dr Strube, is irrelevant to the main question; if the guidelines are being misinterpreted, the remedy is to attack the misinterpretation, not the guideline.

We would point out that we are not attempting to prove Dr Strube wrong. We cannot do so, any more than he can prove that he is right. What is important is that guidelines are based on the best evidence available, and at the moment we lack the information needed to resolve the issue. If and when evidence is strong enough, the recommendation will appear as Dr Strube wishes. Until then, this is a question of faith rather than fact.

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1 Neville RG, Higgins BG. Providing better asthma care: where there is left to do? Thorax 1999;54:813–817.

AUTHORS’ REPLY Dr Strube’s letter addresses an interesting and important question. It is an issue which deserves proper debate, and most Thorax readers will recognise that that is exactly what it has had in the recent pages of the BMJ. Because of this, and because the question has, we would suggest, only the most tenuous link to our article, we will reply only briefly. Dr Strube makes the case for using inhaled steroids in all asthma with conviction, but his supporting arguments are a mixture of circumstantial evidence and his own perception of the psychology of asthmatic patients. There is simply no direct trial evidence to show benefit from blanket administration of inhaled steroids to all new asthmatics. Good evidence certainly exists in asthma of moderate severity or greater, but the situation is less

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Spirometry in primary care

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