Table 1 Mean FEV_1%/FVC and FEV_1 values from 28 authors and from the study of Albers and colleagues[1]

	FEV_1/FVC (%)			FEV1 (l)		
Age (year)	43	48	Decline	43	48	Decline
Mean	80.2	79.2	1.0	3.486	3.350	0.135
Range	73.8–84.1	72.4–83.5	0.4–1.6	3.10–3.82	2.95–3.66	0.11–0.16
Mean[1]	84.5	79.3	5.2	3.532	3.335	0.197
SD[1]	9.8	8.8	–	0.833	0.806	–

FEV_1, forced expiratory volume in 1 s; FVC, forced vital capacity.

without baseline abnormalities. In 5 years, forced expiratory volume in 1 s (FEV_1) fell by 200 ml and FEV_1%/vital capacity (VC) by 5.2%, remarkably large declines for such subjects. We computed predicted values at ages 43 and 48 years according to 28 authors who had published predicted values for FEV_1%/forced vital capacity (FVC) for Caucasians, and 30 who had done so for FEV_1.[2] The results are shown in table 1; the values reported by Albers and colleagues[1] are in the bottom two rows.

The decline in FEV_1/FVC during the study period was more than five times the expected average drop; the fall in FEV_1 was larger than expected. If the non-smokers declined at an expected rate (135 ml in 5 years) and we attribute the excess decline to smokers, the decline in smokers must have been 340 ml; as a minority of smokers exhibit an accelerated decline in FEV_1 leading to COPD,[3] a limited number of smokers must have had a decline in FEV_1 far in excess of this. In that case, one would expect an increase in the scatter, but the SD of FEV_1 did not increase. The decreased scatter in FEV_1/VC over the 5 year period suggests that the group became more homogeneous, which makes it unlikely that the excess decline was caused by a subgroup. In any study, the ratio of average FEV_1 and average VC is not exactly equal to the average FEV_1/VC ratio; however, in 4557 observations from a random sample of a Dutch population, the difference was very small: 0.7623 vs 0.7635. Thus if we reconstruct the VC in the study of Albers and colleagues[1] from FEV_1 and FEV_1/VC, VC at baseline was about 4.18 l, and 5 years later 4.21 l, so there was at best a trivial change.

One wonders whether these unusual findings are caused by problems with data collection which would invalidate the conclusions of this study. The authors state that variation in spirometer performance was assessed and accounted for; this merits a more detailed account. Measurements were performed according to the American Thoracic Society standards,[4] but the study started prior to that report; were measurements recalculated?

P H Quanjer,[1] I Steenbruggen,[2] G Ruppel,[3] M P Swanney[4]

[1] Erasmus University, Rotterdam, The Netherlands; [2] Isala Klinieken, Zwolle, The Netherlands; [3] St Louis University Hospital, St Louis, Missouri, USA; [4] Respiratory Physiology Laboratory, Christchurch Hospital, New Zealand

Correspondence to: Dr P H Quanjer, Erasmus University, Rotterdam, The Netherlands; pquanjer@xs4all.nl

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Authors' response

We thank Quanjer *et al* for their detailed comments on our study. The decline in lung function is a key aspect of COPD. The DIMCA study has been one of the first that focused on patients in the early stage of COPD and collected data covering a period of 10 years. It is therefore important to review the quality and reliability of our data.

In response to the comments raised by Quanjer *et al* we would like to stress the following. Our paper[1] reported on the first 5 years of follow-up of our study population. For the baseline and year 10 measurements, the same spirometer (Microspiro HI-298 spirometer; Chest Corporation, Japan) was used. Because a different spirometer was used at year 5 (Fukuda Sangyo spiro analyzer ST-250, Japan), equipment performance was assessed before as well as after lung function measurements in all participants had been completed. As we observed a systematic linear deviation in the lung function indices compared with the original spirometer, we considered it necessary to account for this. Further support for the reliability of our data was found in the follow-up of our study subjects. After 10 years, lung function was reassessed using the same spirometer that was used at baseline, and all assessments at year 10 were performed by the same lung function technician who performed the baseline assessments. We have now analysed the 10 year follow-up data and observed a further lung function decline that was fully in line with the pattern presented at year 5. Although the 10 year data have not been published to date (Mieke Albers thesis: COPD in primary care. Aspects of secondary prevention; chapters 6 and 7), the group of subjects without baseline abnormalities showed a decline in forced expiratory volume in 1 s (FEV_1) amounting to 348 ml over the 10 year observation period. Over the 5 year period, this decline amounted to 197 ml. The decline in FEV_1/vital capacity (VC) was 10.8% after 10 years of follow-up and 5.2% after 5 years of follow-up.

Given the quality of the measurements and consistency of the pattern over time, we do not think there are reasons to doubt our findings. Quanjer *et al* point to the use of FEV_1/FVC. It is to be expected that our findings would have been arithmetically different had we used this ratio instead of the FEV_1/VC ratio. But the systematic difference still leaves the prediction in decline intact. For that reason, we do not believe there were fundamental flaws in our study, although we agree that the decline is relatively high compared with findings in previous population cohorts. We have no explanation for this.

Quanjer *et al* are correct in that it would have been more appropriate to refer to the 1987 update of the American Thoracic Society statement on the standardisation of spirometry. At the time, this guideline served as the basis of procedures in the lung function laboratory of the University Lung Centre Dekkerswald, where all of our study subjects were measured.

M Albers,[1] T Schermer,[1] Y Heijdra,[2] J Molema,[2] R Akkermans,[1] C van Weel[1]

[1] Department of General Practice, Radboud University Nijmegen Medical Centre, The Netherlands; [2] Department of Pulmonology, Radboud University Nijmegen Medical Centre, The Netherlands

Correspondence to: Professor C van Weel, Department of General Practice, 117-HAG, PO Box 9101, Nijmegen, The Netherlands; c.vanweel@hag.umcn.nl

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No role for routine CT scans in paediatric empyemas

In the paper by Jaffe *et al*, the authors describe the CT findings of 31 patients with thoracic empyema who had three investigations (chest radiography, CT scan and ultrasound scan).[1] They correctly conclude that routine CT scanning has no role for children with empyema treated with urokinase and percutaneous chest drainage. It is interesting to note that CT scanning is becoming popular as nearly half the subjects

had a chest CT scan on referral. They fail, however, to describe a role for chest CT, but do imply that it may be indicated for patients undergoing video-assisted thoracoscopic drainage (VATS). There is no evidence in the current literature supporting the use of CT scans before VATS. The British Thoracic Society guidelines do not recommend routine CT scans in children with empyema.[2]

In our centre all patients with empyema requiring intervention undergo VATS (approximately 40/year). We would suggest that chest CT scanning is not indicated before VATS in nearly all cases. We have found chest CT scans to be helpful, however, in situations where the patient has not responded to appropriate treatment with antibiotics and VATS. In this situation the possibilities are reaccumulation of pleural fluid, abscess formation or more extensive parenchymal involvement, differential diagnoses that are distinguished by CT scanning and information that is critical to the decision to reoperate (or not).

In addition, Jaffe *et al* do not take the opportunity to critically examine the role of chest ultrasound scans in patients with empyema. In our experience, clinical examination and chest radiography can determine the presence of pleural fluid. If the purpose of the ultrasound scan is to determine whether the fluid is simple (a parapneumonic effusion) or organised (empyema), this can be achieved more simply with a lateral decubitus or erect chest radiograph. The decision to undertake definitive management with urokinase or VATS is determined by the presence of unremitting infection and/or fluid volume in the pleural space. It is an outdated paradigm that the distinction between simple and organised pleural fluid makes any difference to subsequent treatment or outcome. The main use for ultrasound scanning should be for those children who are found to have a unilateral whiteout on the chest radiograph at presentation and for whom the distinction between pleural space and parenchymal disease is difficult to make.

J Massie, N Pillarisetti, S Ranganathan

Department of Respiratory Medicine, Royal Children's Hospital, Melbourne, Australia

Correspondence to: Associate Professor J Massie, Department of Respiratory Medicine, Royal Children's Hospital, Parkville, Melbourne 3052, Australia; john.massie@rch.org.au

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Author's response

We thank Massie *et al* for correctly questioning the clinical need for routine chest CT scanning before performing video-assisted thoracoscopic surgery (VATS). Our study was pragmatically designed to reflect clinical practice in our institute, where thoracic surgeons routinely request a pre-operative CT scan for use as a "road map" when performing minimally invasive endoscopic surgery where direct visual access is limited. This helps to plan and assist in placement of the ports and instruments in order to decrease risk and avoid potential complications such as bronchopleural fistula which would result as a consequence of puncturing the lung parenchyma in close proximity to the pleura. We agree with them that there is no evidence base to support this practice in terms of risk, and our study was not designed to answer this question.

The principle of providing surgical "road maps" (which cross-sectional imaging now provides) is prevalent in many areas of cardiothoracic imaging where CT and MRI are added as an adjunct to echocardiography and ultrasound scans in order to enhance anatomical (and, indeed, sometimes functional) information to enhance quality and provide a safer more informed patient journey.

We are surprised that Massie *et al* advocate the use of a lateral decubitus chest radiograph in place of an ultrasound scan which is not, in fact, a recommendation of the BTS guidelines. Indeed, this would be a retrograde step in terms of the quality of information and the radiation burden, and should only be advocated where there is no access to ultrasound.

As discussed in our paper, ultrasound is an invaluable tool as it is cheap, mobile, easy to use, can differentiate transonic from purulent fluid, solid lung from fluid and enables the radiologist to mark the spot for chest drain insertion. Although it has been used to stage the disease, we agree that it is not useful in predicting the clinical outcome as was evident in our study. Importantly, ultrasound does not carry a radiation burden.

One of the key messages we had hoped to emphasise in our study is the critical need to reduce exposure of children to unnecessary radiation. With this in mind, we disagree with Massie *et al* and continue to advocate the use of ultrasound as the most important imaging modality in managing children with empyema. The BTS guidelines also support this view.

A Jaffe,[1] A D Calder,[2] C M Owens,[2] S Stanojevic,[3] S Sonnappa[3,4]

[1] Sydney Children's Hospital, Randwick and University of New South Wales, Sydney, Australia; [2] Department of Radiology, Great Ormond Streat Hospital for Children NHS Trust, London, UK; [3] Portex Anaesthesia, Intensive Therapy and Respiratory Unit, Institute of Child Health, London, UK; [4] Department of Respiratory Medicine, Great Ormond Street Hospital for Children NHS Trust, London, UK

Correspondence to: Dr A Jaffe, Department of Respiratory Medicine, Sydney Children's Hospital, High Street, Randwick, Sydney, NSW 2031, Australia; adam.jaffe@sesiahs.health.nsw.gov.au

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CORRECTIONS

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