We thank Dr. Paraskevas for his interest in our recent publications [1,2] as regards preoperative statin use and outcomes following cardiac surgery. For the benefit of Dr. Paraskevas, and more importantly, the journal readership, I would like to make the following points. Our first analysis and publication [1] looked at the entire cohort of patients in our database, including patients with stable angina and both valvular and isolated coronary artery diseases. This publication was available online on 6 November 2004. Somewhat perplexed and disappointed with the negative results with regards to any benefit of preoperative statin use on short-term outcomes in this heterogeneous patient population, we pondered whether there may be a subset of patients who may derive benefit from preoperative statin use. In this regard, and as outlined in the Introduction section of the second paper, which was available online on 23 March 2005 [2], we hypothesized that it may be the patients who present specifically with unstable angina who are likely, if any, to gain a benefit. Thus, the patient groups reported and the results of the analyses with respect to statistical parameters are distinctively different in the two papers. By necessity, there is an overlap in the Materials and Methods section with respect to patient selection and the Data Analysis and Statistics section because these methodologies indeed were the same! I take great exception to the claim that ‘most of the Results sections are identical’. Clearly, we have analyzed two very different patient populations in our two manuscripts (reason for which is based on sound biologic rationale) and the logistic regression models and the results with respect to the various ‘odds ratios’ and ‘p values’ are not the same. Because we were interested in the potential benefits of statins with regard to their ‘pleiotropic’ effects, the outcomes of interest analyzed in both studies remained the same. Similarly, there is an overlap in the Discussion section because the rationale for assuming a benefit for statins (and potential explanations for our observed negative results in both studies) does not change. Furthermore, the limitations of both studies are expected to be the same because the statistical methods and analyses applied were similar. Finally, and again by necessity, the Reference section is similar because during the time frame of submitting our manuscripts, no new citations of relevance were found. Contrary to Dr. Paraskevas’ contention, we believe our second manuscript [2], published in this Journal, adds importantly to our initial observations [1] by showing that preoperative statin use is not associated with improved short-term outcomes in a group of patients (unstable angina) who theoretically should gain the most!

References


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Letter to the Editor

Chronic obstructive pulmonary disease, surgery and pulmonary rehabilitation

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We have read with interest the report from Bobbio et al. [1] regarding the changes in pulmonary function tests and cardio-pulmonary exercise capacity in patients affected by chronic obstructive pulmonary disease (COPD) who have undergone a lobar pulmonary resection.

Interestingly, the authors have demonstrated that COPD patients undergoing lobectomy may be found, 3 months after surgery, to have a persistent, significant exercise capacity loss in the absence of modifications of dynamic lung volumes (i.e. forced expiratory volume in 1 s—FEV1).

We would like to congratulate the authors for the extensive, precise and thoughtful analysis and would amicably invite them, on the basis of their experience, to briefly comment on the possible role of pulmonary rehabilitation (PR) in the post-operative status of lung-resected patients, eventually affected by COPD.

Nowadays, in fact, there is body of evidence regarding the efficacy of pulmonary rehabilitation in the comprehensive management of patients with COPD and PR programs are practiced worldwide [2].

Positive results in terms of improvement in dyspnoea, exercise capacity and quality of life (QoL) are recognised in chronic obstructive and non-obstructive pulmonary diseases, including COPD, cystic fibrosis and restrictive thoracic disease [3]. Moreover, PR is becoming a crucial component of the overall treating strategy in high-risk surgical patients (i.e. lung volume reduction surgery—LVRS and lung transplantation).

Particularly, PR was part of the preoperative phase in those candidates who met criteria for the large multicentre National Emphysema Treatment Trial (NETT) Research Group trial [4].

On the one hand, there are few data regarding the effectiveness of PR programs in the post-operative period of patients who have undergone lung resection (LR) for non-small cell lung cancer (NSCLC). On the other hand, thoracotomy and parenchymal resection “per se” are known to have an important effect on pulmonary function and QoL.

Our Group has a timely interest in this issue [5]: in our experience, we have substantially confirmed that an inpatient post-operative PR protocol improves significantly dyspnoea, exercise capacity and quality of life in patients who have undergone a parenchymal resection for cancer. As well we have confirmed that even if there is not a significant effect of PR on the dynamic lung volumes (FEV1) and blood gases values, the post-operative recovery of rehabilitated
patients is more rapid in respect of those who have not been rehabilitated. Our updated evidence (data submitted for publication) further testifies that there is a possible beneficial role of post-operative PR following lung resection for NSCLC, in either COPD or non-COPD patients, especially in those cases with a poorer function at the moment of operation.

We would really appreciate an authors’ reply to these comments.

References


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Reply to the Letter to the Editor

Reply to Ferri et al.

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We would like to thank Dr Ferri and his colleagues for their attention to our work as well as for their comments and questions. On the basis of their personal experience and with the support of a literature review, they conclude that postoperative pulmonary rehabilitation (PR) should take a central place in the comprehensive management of all patients undergoing lung resection and not only in those with underlying COPD [1,2].

In the experience of authors who attempted to quantify the permanent loss of VO2,max after lung resection, a time-point of six months after surgery was generally used to assess the postoperative functional recovery, and in these studies a specific postoperative PR was generally not provided [3,4]. As reported by Bollinger and colleagues, the modifications in exercise capacity in the subgroup of patients with an impaired preoperative pulmonary function are similar to that found in patients with a preoperative normal lung function.

The results of our study, when compared to other reports, also failed to find a difference, in terms of loss of VO2,max between COPD and healthy individuals at a time-point of functional evaluation established at three months post-operatively [5]; however, in our study we observed a significant impairment of postoperative VE/VCO2 slope between minute ventilation and CO2 production in these COPD patients and also, through Visual Analogue Scale analysis, that exercise performance was more limited by dyspnea than by leg fatigue.

On the basis of these data it seems possible to affirm that surgical trauma and loss of lung parenchyma are the principal causes of postoperative deconditioning after lung resection, both in COPD and in non-COPD patients; the characteristics of the postoperative limitation to exercise in COPD patients, however, seems mainly due to ventilatory limitation rather than to skeletal muscle deconditioning.

A postoperative PR program will probably be helpful in all patients undergoing lung resection, with the aim of quickly restoring the exercise tolerance and ameliorating the quality of life; however, it seems crucial to specifically tailor the PR program in the subgroup of patients with COPD in order to take into account individual ventilatory patterns.

Unfortunately, in our Department the assigning of patients to a postoperative PR program is not part of a specific protocol; the decision to assign patients is left to the care of the referral physician.

In conclusion, we firmly support Dr Ferri and his colleagues in their goal of designing a feasible and reproducible strategy to reverse the natural attitude of patients to deconditioning after lung resection. We hope to read shortly about their personal methods and results in postoperative PR of patients undergoing lung resection.

References


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