Reply: Lung surveillance following colorectal cancer pulmonary metastasectomy: Utilization of clinicopathologic risk factors to guide strategy

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Abbreviated legend for Central Picture:

PulMiCC survival: Red line-selected for surgery; blue-rejected; orange, green-randomised.

Sir

The paper by Deboever et al [1] on pulmonary metastasectomy (PM) for colorectal cancer (CRC) starts: 'The survival benefit associated with resection following colorectal pulmonary metastasis in selected patients is well accepted.' It is true that it is 'well accepted' but this acceptance is due to a professional consensus based solely on weak observational evidence and the systematic ignoring or dismissal of evidence from PulMiCC [2], the only randomized trial directly addressing this issue. This showed no difference in overall survival and had sufficient power to rule out a major survival benefit from PM [3]. It was nested within a much larger, prospective observational study of patients selected or turned down for surgery, suggesting that the major determinant of survival after PM is likely to be careful selection of patients with favorable prognostic factors, not the intervention [4] (Figure).

Deboever et al describe a retrospective study looking at time to reappearance of lung metastases after PM and they identified several factors associated with earlier local recurrence. They concluded that these high-risk patients should have early CT imaging for 'surveillance'. But there are problems with this paper.

Important information is missing. These are all probably highly selected patients, not representative of most patients in this situation. Their median age was 55 years and the majority had a single metastasis, but there was no description of known prognostic factors such as stage at first presentation nor of the time from surgery to PM. There is no date of data analysis which would indicate the maximum and minimum lengths of follow. There is no mention of attrition due to death or loss to follow up. Most reports suggest 10-20% of patients die within 2 years of PM and it is likely that all patients in this cohort were followed for at least 2 years. Did any die or become lost to follow up? Was the survival analysis actuarial?

Finally, there is no justification for a policy of surveillance for any patient after PM, especially those with the identified risk factors for early relapse. Deboever et al found that 52.3% of these highly selected patients developed a new pulmonary metastasis during the period of observation. These

patients clearly had occult metastases at the time of PM and now have disseminated disease. It is likely that the rest will also manifest new metastases somewhere eventually.

The evidence supporting the use of 'primary' PM is weak, and despite the authors' belief that a second PM 'may achieve survival benefit', there is no evidence to support that nor for early intervention with chemotherapy. We have shown that PM is associated with decreased lung function[4] and it is an intervention, even with modern anesthetic and surgical techniques, associated with real risks (including death). What is the point of any surveillance given the costs and probable increase in patient anxiety?

We suggest that the policy advocated by Deboever et al represents overinvestigation and overtreatment and is very unlikely to give patients any survival or quality of life benefit.

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