

Reply
To the Editor:

We thank Dr Korun for his comments [1] and are largely in agreement. Perhaps our effort at brevity and efficiency in the discussion section [2] failed to disclose why the analysis suggested by Dr Korun was not reported in the article. At the outset, our goal was to evaluate a cohort defined exclusively by intrauterine growth restriction (IUGR) status. Our challenge of course was how best to select a control group. We considered a consecutive series of patients encompassing the IUGR cohort and the use of multivariate analysis, controlling for several potentially important variables as well as weight at operation. Such an approach may have enabled the type of analysis suggested by Dr Korun. This of course introduced certain practical constraints given the period spanned by the IUGR cohort and the inclusion of all consecutive non-IUGR patients during this period (a tremendous amount of data) versus some method of exclusion of certain control patients and hence the potential introduction of bias based on the exclusion process. The matched cohort approach seemed to be a reasonable solution. However, to maintain rigor in the statistical analysis, we judged it important to adhere to the planned design and applicable statistical constraints.

Nevertheless, we informally attempted to discern the effects of prematurity and weight through several post hoc analyses. Our biostatisticians expressed concern about reporting these for 2 reasons: (1) intrinsic weakness of post hoc analyses and (2) probably more importantly, the limited numbers of patients in given strata and concern that a significant or nonsignificant result may simply be incorrect and lead to transmission of false information. We therefore limited our comments to the speculation that IUGR likely has an independent effect, perhaps with diminishing impact at the lower extremes of prematurity and weight. Discerning the independent effects of IUGR, prematurity, and low weight will undoubtedly require a much larger cohort and a study designed specifically for this goal.

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Video-Assisted Thoracoscopic Surgery Is Safe and Effective in the Treatment of Pulmonary Hydatid Cyst
To the Editor:

We read with great interest the article by Alpay and associates [1] reporting their 77 patients with pulmonary hydatid cyst. Their case series have provided an important contribution to the literature on the surgical treatment of lung hydatidosis.

Hydatid diseases still remain an important health problem in developing countries such as Turkey. Our region is an endemic area for hydatid cysts [2]. In our clinic, 23 patients with lung hydatid disease were treated by thoracoscopic surgery in the last 3 years. We began each operation for lung hydatid cyst with a video-assisted thoracoscopic surgery (VATS) technique during this period. Our surgical technique and results are similar to those of Alpay and associates [1]. In the VATS group, the mean operative duration was shorter and the drain removal was earlier. There was no operative mortality and no recurrence during our follow-up period.

The main advantage of minimally invasive techniques is their milder perioperative trauma and less discomfort for the patient as we avoid cutting a large length of the intercostal muscles and spreading the intercostal. The risk of breaking the ribs spread in the open method is also reduced to a minimum. That resulted in milder postsurgical pain and faster rehabilitation after the treatment, and hence, a shorter stay in hospital.

We do not agree with Alpay and associates about some contraindications to VATS in pulmonary hydatid cyst treatment. If peripheral multiple cysts are visible in a single lung, they can be removed by wedge resection or cystotomy and cap-ironnage, placing a new port if needed. We think that giant hydatid cyst (>10 cm) is not a contraindication. Peripheral or fissure localized giant cysts can be removed easily after aspiration. Surgeons should have good experience with minimal access surgery, including thoracoscopic suturing, before performing thoracoscopy for lung hydatid disease because cap-ironnage of a giant cyst will take a long time. Likewise, peripheral small cysts (<2 cm) can be removed easily. The most important relative contraindications are central localization, being too close to a hilar structure, and undetectable intrapulmonary deep cysts.

A minimally invasive approach to the management of lung hydatid cyst is safe and effective when performed by surgeons experienced in open and minimally invasive surgery and techniques. The optimal approach and procedures performed should be determined on an individualized basis and requires a thorough preoperative investigation.

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Reply
To the Editor:

We read the letter by Eroglu and colleagues [1] and thank the authors for their contribution. Hydatid cyst disease is still a challenging problem in developing countries. It is good to hear the videothoracoscopic approach to hydatid cyst disease is

becoming common among thoracic surgeons. We congratulate the authors for their work. However we would like to answer a few topics from the letter:

First, if there are peripheral multiple cysts in the lung, the authors said that it is okay to place a new port for cystotomy and capitonage. In our practice, we prefer to approach multiple lung cysts through a thoracotomy. For single cysts, we place 1 camera port and 1 utility incision for cystotomy and capitonage [2]. If we find a new cyst or cysts during the operation that had not been detected preoperatively by thorax computed tomography, we prefer a single-incision minithoracotomy approach. Because we are also trying to preserve as much lung tissue as possible, we prefer not to perform wedge removal of the cysts.

Second, we think that a giant hydatid cyst (>10 cm) is a relative contraindication, as was stated in our original report [3]. There are some reasons for this argument. First, because the diameter is big, it makes it harder to capitonage. Second, there may be a risk for recurrence after the removal of a giant hydatid cyst because it is harder to control the spillage through videothoracoscopy. We prefer a minithoracotomy for a safe operation.

Third, we agree that hilar localized hydatid cysts are difficult to manage by videothoracoscopy.

In conclusion, video-assisted thoracoscopy is a safe and reliable tool in the treatment of hydatid cyst disease. It is superior to thoracotomy in selected patients, with less pain, less morbidity, shorter hospital stay, and better aesthetic consequences. In these days in which extreme (extended) lung resections are performed by videothoracoscopy, a parasitic disease—a benign disease—should be considered or even forced to be a candidate for a videothoracoscopic operation.

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Risk Scoring System Without the Effect of Transesophageal Echocardiography Is Not Enough for Dysphagia

To the Editor:

We eagerly read the article “A novel risk score to predict dysphagia after cardiac surgery procedures” by Grimm and colleagues [1]. We appreciate the authors for their study. However, we have identified some contradictory points in the article that should be highlighted.

As is widely known, transesophageal echocardiography (TEE) is used extensively in cardiac operations. Unfortunately, it has some disadvantages like bleeding, esophageal injury, and dysphagia after the operation. The authors stated the importance of TEE use as a risk factor for postoperative dysphagia at the beginning of their article and continued in the comments to document studies about an association between dysphagia and TEE use. The overall results of the studies compromise over the association between TEE and dysphagia. Although the authors defined the importance TEE, they did not include TEE use in the scoring system because of the retrospective nature of their study, inasmuch as they did not have information about TEE duration. Furthermore, they concluded that TEE use could no longer be an independent risk factor. In our opinion, forming a new scoring system without information about TEE after so much knowledge about TEE has been gained is not logical. In 2011, Chin and colleagues [2] described intraoperative TEE use as an independent risk factor in multivariate regression analysis in a prospective study. They suggested a modification of the TEE protocol and shortened the time to reduce postoperative dysphagia. In our opinion, in the current state, lack of information about TEE use is a missing value in the risk of dysphagia in cardiac surgery (RODICS) scoring system. Setting a prospective study that also includes information use of TEE, with duration times and dysphagia, may strengthen the RODICS scoring system. Despite the authors’ description of the predictive strength of the model as excellent, with the area under the receiver operating characteristics curve 0.75, adding a TEE value may improve the predictive strength.

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