



## Letter to the Editor

## Thoracic versus abdominal approach to correct diaphragmatic eventration in children



To the Editor,

We read with interest the article ‘Thoracic versus abdominal approach to correct diaphragmatic eventration in children’ by Alisha Gupta et al. [1]. We congratulate the authors for publication of their study.

In their study, the authors included all cases of diaphragmatic eventration (DE) less than 16 years of age, but didn't mention if any exclusion criteria were applied. Patients with DE, when symptomatic, have complaints of respiratory difficulties (wheeze, exercise intolerance, respiratory distress) and recurrent infections. DE is often incidentally detected. Asymptomatic cases might not require any intervention [2]. Wu et al. published one of the largest series of DE cases, in which 91 out of 177 cases did not require any intervention [3]. The authors could have mentioned the indications for surgery in their study population. In that manner, the postoperative clinical improvement with respect to symptoms could have been better understood.

Technically the repair of DE involves plication of diaphragm by thoracoscopy/laparoscopy (minimally invasive surgery; MIS) or thoracotomy/laparotomy (open method). But the impact it has on the patients is different for each of type of procedure. Thoracotomy, for example, has intraoperative ventilation problems (owing to single lung ventilation), hypercarbia, pneumothorax (and the chest tubes), more postoperative pain, and increased time to recovery during the postoperative period, and cause chest deformity owing to fibrosis in the long term. These are established facts. In comparison, thoracoscopy is carried out via small incisions for ports, minimizes the postoperative pain and sequelae of a thoracotomy incision. The authors have acknowledged such contrasting differences between open and MIS methods, yet grouped these four different methods under the umbrella terms ‘thoracic’ and ‘abdominal’ approaches, and compared the outcomes, which we think can cause statistical bias. Out of 102 patients, thoracotomy was done in 86, thoracoscopy in 3, laparotomy in 4, and laparoscopy in 9 patients. Uneven distribution of numbers among these groups would make it impossible to arrive at any statistical conclusion, unless they're made into two groups. By not clearly defining their methods, this approach only appears to be data dredging.

The authors failed to mention several key details: a) specialty of operating surgeons (Cardiothoracic or Pediatric Surgeons), b) how many of them operated on these cases, and c) the reason why thoracotomy was preferred over the other three methods.

To repair a right sided DE by abdominal approach requires mobilization of the liver, which is technically difficult. It would be of interest to know the reason why six cases were repaired by the abdominal approach (again, laparotomy or laparoscopy?).

Sixteen cases had recurrence in this study. All of them were found in the thoracic group. The distribution of recurrences among thoracotomy

and thoracoscopy could have been mentioned. For the recurrence cases, reoperation was done by the thoracic ( $n = 13$ ) and abdominal ( $n = 3$ ) approaches. It is a known fact that after cardiac or mediastinal surgeries, there can be dense adhesions in the thoracic cavity. Despite this, 13/16 cases were reoperated by the thoracic approach again. The reason for preference to thoracic approach and whether reoperations were by MIS or open method were not mentioned. Since recurrence rate was the primary outcome of this study, further emphasis should have been done on this aspect. It should also be noted that these are only short-term recurrences, and it's not uncommon to find recurrences in the long term [4].

Considering the other complications ( $n = 17$ ), out of which most of them (15/17) occurred in the thoracic group, overall complications are much higher in the thoracic group (31 out of 34 complications), which could be significant. The authors did not elaborate on this finding.

Comparing the highest point of the diaphragm before and after surgery to measure the success of the operation was similarly done by other studies [5,6]. The clinical impact of differences in levels of diaphragm with respect to intercostal spaces is not validated. Wu et al. assessed the postoperative outcomes by assessing respiratory symptoms or pulmonary function tests (for older children), which is more sensible [3].

In their conclusion, the authors claim that this is the largest published pediatric series on DE, which is misleading. Wu et al. published a cases series of congenital DE, which had 177 cases, out of which 86 were managed by surgery [3]. When the number of cases treated by MIS is considered, Hu et al. compared 27 cases of DE treated by thoracoscopy and laparoscopy [6] and Miyano et al. compared the same approaches in 20 cases [7].

To conclude, we believe that this paper has not guided the readers significantly in the management of DE. In their attempt to make a statistical analysis, the authors made skewed observations. Instead, the authors could have presented an observational study of four different approaches towards repair of DE, describing the symptomatology of their population, indications for surgery, the reason for surgical approach, postoperative outcomes in terms of symptomatic improvement, and complications that occurred with each approach. This could have been more relevant for the readers.

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