Surgeon has no abacus, but only anatomical benchmarks

Stefano Rausei

Department of Surgery, ASST Valle Olona, Gallarate (VA), Italy
Correspondence to: Stefano Rausei, MD, PhD. Department of Surgery, ASST Valle Olona, Via Pastori 4, 21013 Gallarate (VA), Italy.
Email: stefano.rausei@gmail.com.

In gastric cancer management the extension of the gastric resection, and hence the type of lymphadenectomy, is related to the tumor site and tumor stage, as well as to patients age and comorbidities, possibly influencing surgeons' decision.

Lymphadenectomy has a relevant impact on the gastric cancer therapy, because it responds to two specific key needs: staging and prognosis (1).

At moment there are no reliable criteria to evaluate and measure quality of lymphadenectomy in terms of stage and prognosis definition and the dispute between numerical or anatomical parameters remains unsolved.

Hayashi and colleagues (2) attempted to give a methodologically correct answer to the open discussion about the role of number of retrieved/analyzed nodes after gastrectomy for cancer. By a propensity score matching and apparently reliable methods, they conclude that a number of retrieved lymph nodes <40 is associated with an adverse prognosis of stage III patients who underwent total gastrectomy. Additionally, they suggest to potentially extend to this finding the option of postoperative adjuvant chemoradiation therapy in order to control the locoregional recurrence risk to compensate an inadequate lymphadenectomy.

I have two comments to move to this study. The first one is methodologic, the second one is philosophic.

Firstly, authors declare that the propensity score matching process was estimated using a logistic regression according to several variables potentially affecting the nodes count. I am surprised that this process did not consider N- and T-stage. In fact, it is at least questionable that after matched comparison in the RLN ≥40 group pT4 cases decrease from 68.7% (103/150) to 63.2% (55/87) and pN3 from 54.7% (82/150) to 43.7% (38/87) with an obvious positive impact on the prognosis of its patients.

Secondly, authors stress the (retrospective!) efforts to minimize the other well-known factors influencing the nodes harvesting (such as anatomical inter-individual variability, lymphadenectomy extension, as well as pathological evaluation) (3), but they themselves admit that this variability is unavoidable. Nevertheless, they would encourage surgeons to perform "appropriate" lymphadenectomies, aiming to achieve 40 lymph nodes without destruction. Specifically, Hayashi and colleagues attribute a relevant role to this "nodes destruction": the smaller number of nodes would reflect the destruction of lymph node during surgery and would result in the dissemination of cancer cells into the peritoneal cavity. Actually, I am convinced that authors aimed to focus the necessity to perform "appropriate" lymphadenectomy, aiming to follow anatomical benchmarks in order to reduce the possibility of any nodes destruction. When I conclude my nodal dissection, I am sure to have carefully reproduced the procedure recommended by Japanese Gastric Cancer Association (4), but I am no certain to did not destroy any lymph node. What is more, I am neither able to estimate the number of nodes freshly removed. Could Hayashi and colleagues argue the opposite?

Definitively, the fact that the number of total lymph nodes is linked to the prognosis is well known for gastric cancer patients (5), but no consensus exists on its role in the evaluating the quality of lymphadenectomy and,

particularly, in the therapeutic strategy selection. It is no so convincing to expose a gastric cancer patient with a 39-node count to morbidity risk of adjuvant chemo/radiotherapy on the basis of pure statistical argumentations.

To conclude, the UICC did officially associate the numeric criterion to the concept of lymphadenectomy for gastric cancer in order to simplify and diffuse it among Western surgeons; Japanese surgeons taught us to perform "anatomically appropriate" lymphadenectomies. Once again, I would stress the importance of the anatomical lesson for gastric cancer surgery.

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Footnote

Conflicts of Interest: The author has no conflicts of interest to declare.

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