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Letter to the Editor

Regarding: "Liver venous deprivation compared with portal vein embolization to induce hypertrophy of the future liver remnant before major hepatectomy: A single center experience"

To the Editor:

Dear authors, we read with great interest your recently published work[1] from Kobayashi and collaborators. Nevertheless, we were surprised by the low liver regenerative results reported for portal vein embolization (PVE) with *n*-butyl-cyanoacrylate (NBCA) and for liver venous deprivation (LVD). Results reported for PVE were 5.6% for the degree of hypertrophy (DH) and 1.4% per week for the kinetic growth rate (KGR). Most of the previous publications have reported greater hypertrophy for PVE with NBCA, with DH ranging from 9% to 13%, and KGR ranging from 2% to 4.4% per week.[2–11] A prospective trial reported a DH of 16% for healthy livers and a DH of 9% for chronic liver disease after PVE[12] and stated that the expected and published range for DH in PVE should be around 12% ± 5%.[13] Our own group, similarly, has reported greater regenerative results for PVE with NBCA (12.7% for DH and 2.9% per week for KGR)[14] and also for PVE with different embolic materials (Camelo et al. reported 11% for DH and 2.2%/week for KGR).[15] A systematic review[16] reported a medium future liver remnant (FLR) volume increase of 49.8% (149.8%) after PVE which is even greater than the value reported for LVD (135%) from the current study.[1] The relevance of low hypertrophy results are associated with the prediction of increased liver-related mortality after right or extended right hepatectomy for patients with low DH and KGR results (KGR <2% per week) after PVE as published by Shindoh et al.[17] Nonetheless the overall mortality from the presented cohort[1] (0% during the first 90 days after operation; 80% overall survival at 3 years) is comparable to predicate literature,[18,19] highlighting that these apparently "low" KGR and DH from the present study (that would imply a much greater mortality after surgery) may be inaccurate. These remarks highlight the discrepancy between the clinical outcomes from the reported cohort and the volumetric analyses shown. Furthermore, we also were surprised by the results reported for LVD (DH 8.5% per week and KGR 2.9% per week).[1] Few publications approached this issue, and the latest article reported a KGR of 16 ± 7 mL/day after LVD (and 4.8 ± 4 mL/day after PVE).[20] Also, the study from Guiu et al reported a DH of 12.7% 23 days after LVD,[21] which yields a KGR of approximately 4.2% per week, once again well superior to the results from the present study.[1]

These apparently "low" KGR and DH of liver regeneration seem to be balanced between the 2 groups being analyzed (PVE and LVD), highlighting the potential issue on the volumetric analysis itself. Was each reader blinded to the other reader analysis? Was a mean value used? Is the dedicated software used certified for these volumetric analyses? Do the authors believe that the included patients could have a role? It was surprising to see that the mean baseline FLR ratio was 35% (FLR volume 520–550 mL), which is also greater than previous reports.[5,17] Low FLR volumes before PVE are associated with more robust liver regeneration,[2] so should we conclude that greater baseline FLRs naturally limit KGR and DH. Could technical issues play a role? What was the mean volume and dilution of NBCA and iodized oil used in these patients? How did authors size the Amplatzer plug used during LVD, and how many plugs were used per patient? May this lesser hypertrophy be related to the technique adopted for LVD?[21,22] Was there any potential bias from different operators between the 2 groups?

Finally, when looking at this retrospective cohort study,[1] there seems to be a practice change from 2010 to 2016 (PVE group) versus 2016 to 2018 (LVD group). The proportion of patients with colorectal metastases/noncolorectal metastases was 50%/50% in the PVE group, whereas it was 90%/10% in the LVD group with very few patients with cholangiocarcinoma, which might explain the decrease in rate of dropout before resection from 23% (PVE group) to 5% (LVD group). Were there any patients submitted to PVE and LVD excluded during the study time-period besides the ones with disease progression? If so, why and what was the criteria for exclusion?

We want to congratulate the authors for the remarkable work with excellent overall clinical outcomes. Also, the potential role and advantages of LVD versus PVE are highlighted here and are excellent grounds to expand during future prospective comparative trials.

Conflict of interest/Disclosure

We have no conflicts of interest.

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