



Letter to the editor



Comment on a new treatment strategy for end-stage hepatic alveolar echinococcosis: IVC resection without reconstruction

Dear Editor

We read the recent article “A new treatment strategy for end-stage Hepatic Alveolar Echinococcosis: IVC resection without reconstruction” published by Du and colleagues with great interest [1]. The authors stated that liver resection combined with inferior vena cava (IVC) resection is effective and feasible for patients with hepatic alveolar echinococcosis (HAE) infringing on the IVC. We would like to share our opinion and criticisms about this valuable article.

We would like to share our opinion regarding this article under four topics. These are; (i) the need for retrohepatic IVC reconstruction, (ii) the need and duration of albendazole treatment, (iii) the correct use of hepatectomy terminology, (iv) at the end of this letter we will emphasize certain points that we will require the authors to reply.

As it is known very well, the patency of the retrohepatic IVC is required for venous drainage of the both kidneys, lower extremities and many abdominal organs. Resection and reconstruction of retrohepatic IVC can be performed under special circumstances. However, in certain hematologic diseases which cause chronic thrombus and occlusion of the retrohepatic IVC, wide iatrogenic defect of the IVC during surgical procedures, chronic liver disease causing a difficult dissection plane around the IVC, hepatic malignancies resulting in a massive invasion of IVC that obscures R0 resection, and HAE disease invading or causing dense fibrosis around IVC prevent any reconstructive effort [2].

The necessity of IVC reconstruction following the formation of extensive tissue defects after resection depends totally on the duration of obstruction (whether chronic or not) and presence of effective venous collateral circulation (azygos, hemiazygos, ascending lumbar veins, big portosystemic collaterals) [2,3]. If the venous collaterals are well developed; such as the 13 patients presented in the present study; no pressure gradient will be observed around the stenotic-thrombotic IVC segment. On the other hand, if the venous collaterals are weak or not developed, a significant pressure gradient will be present around the stenotic -thrombotic segment of the IVC. In other words, the reconstruction of IVC is not indicated when the venous collaterals and IVC tributaries such as the azygous circulation are well developed. Because the venous drainage of the kidneys and the lower extremities can continue through these alternative routes. In the preoperative period, multidetector computed tomography (CT) or conventional vena cavography (if indicated) can show the venous collateral circulation, and by using these imaging techniques, the operative strategy can be determined. If the surgeons encounter inevitable damage to IVC, the initial step in the management is intraoperative evaluation of the preoperative CT images to determine any venous collateral circulation. If there is an acute injury to IVC under normal conditions, the reconstruction of IVC is very important for the function of both kidneys.

The authors have stated that they have not reconstructed the IVC

because the use of vascular grafts especially artificial grafts was associated with increased risk of infections, bleeding, stenosis, and thrombosis. Furthermore, they have stated that the results of IVC reconstruction with artificial vascular grafts were not satisfactory. In our opinion, this argument does not reflect the actual situation. We believe, the authors should have based their argument on the well-developed venous collateral circulation because if the venous collateral circulation was not developed, the authors would have been obliged to reconstruct the IVC.

According to the current literature, there is no consensus regarding the choice of the ideal vascular graft material for IVC reconstruction but alternatives include cryopreserved homologous vascular grafts (including the aorta, iliac artery, iliac vein, IVC) or artificial vascular grafts (including Dacron, PTFE) [2,3]. The problems such as thrombosis and infection (which was the argument of the authors, as well) are commonly seen with the use of artificial vascular grafts and therefore, it is suggested that cryopreserved homologous vascular grafts should be preferred for IVC reconstruction; whenever possible. However, this is not an evidence-based suggestion and for the centers in which liver transplantation and hepatobiliary surgery is performed at a high volume, it is not always possible to obtain cryopreserved vascular grafts and artificial vascular grafts are used. We have published a study from our cohort of immunosuppressed liver transplant recipients and have shown that Dacron was more resistant to infectious complications than PTFE grafts [4].

The common complications of IVC reconstruction are stenosis, thrombosis which may lead to pleural effusion, ascites, abnormalities in liver function test, and renal dysfunction. One of the largest series in the literature is from our institute and the symptoms were present in 55% of our patients. In addition, radiologic stenting or balloon dilatation could be possible in 27.5% of the patients. Besides, no long-term drainage problem was observed in any of our patients. In our opinion, initiation of low molecular weight heparin and acetylsalicylic acid therapy to these patients has paramount importance in the management. In conclusion, the management of such problems in experienced centers is relatively easy.

There are many nomenclatures for hepatectomy in literature but there seems to be no consensus regarding it. The best and highly accepted classification ever published is the “The Brisbane 2000 Terminology of Liver Anatomy and Resections” which is published by the “Top Guns” of hepatobiliary surgery [5]. Nevertheless, many researchers including the authors of the present study inherit their hepatectomy classification from the former colleagues which creates a huge amount of confusion. For example, the authors present the right trilobectomy and extended right trilobectomy as separate entities; however, they all define the same procedure (extended right hepatectomy =

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extended right hemihepatectomy = right trisectionectomy).

The authors stated that none of the 13 patients received preoperative albendazole treatment while patients with lung and brain metastasis of alveolar echinococcosis received albendazole treatment for 6–12 months following the operation. It is a common fact that treatment of HAE is radical resection and medical therapy. Any patient undergoing elective surgery will require preoperative albendazole treatment for 2–4 weeks. We have accepted and adapted this strategy for our patients. The medical treatment following radical resection should be performed for postoperative two years and the patients should be followed up for a minimum of 10 years. In any patient who received a radical liver resection for AE but sustains distant organ metastasis should receive cyclic albendazole therapy indefinitely following the operation. Life-long medical therapy is required in inoperable cases [6]. For these reasons, in the present study, it is not appropriate to limit the therapeutic period to 6–12 months as stated by the authors.

Our queries for the authors are as follows: (i) The authors state that they have performed portocaval shunt in 8 patients during the hepatic phase. Since the liver is resected together with the retrohepatic IVC, we would like the authors to explain to which segment of IVC the portocaval shunt was performed. Besides, to obtain a functional portocaval shunt in these patients requires the use of vascular grafts to perform the anastomosis between the supra-hepatic IVC and the portal vein; which is not an applicable procedure. Furthermore, if the patients had a sufficient venous collateral circulation, then there is no need for performing portocaval shunts, (ii) The authors state that a patient died due to operation related gastrointestinal bleeding; however, in Table 5, it is stated that the liver function tests were normal for the postoperative 12-month follow-up period. In our opinion, the authors should clarify the cause of death in this patient (iii) The authors have stated that they have classified the complications according to Clavien-Dindo classification and they reported no Grade V complications; however, there is mortality in one patient which means that it was a grade V complication. This point needs clarification by the authors. Furthermore, if the patients with biliary leaks and intrabdominal bleeding required surgical intervention, then these should be classified as Grade IIIb complications.

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Consent

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Guarantor

Akbulut S and Sahin TT are the guarantors for the present commentary and they take full responsibility for the comments and the auxiliary data presented in the commentary article.

CRedit authorship contribution statement

Akbulut S and Sahin TT: Reviewed the literature and wrote the manuscript. Akbulut S and Sahin TT: Supervised the writing process and revised the manuscript.

Declaration of competing interest

No conflict of interest about this letter to the editor.

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