Author response to Letter to the Editor

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LETTER TO THE EDITOR

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Author response to Letter to the Editor: 'AB0, von Willebrand factor/Factor VIII and portal vein thrombosis in decompensated cirrhosis: Too late to unmask the culprit?'

We thank Bitto et al[1] for the interest in our study in which we tested the hypothesis whether ABO blood type would be a risk factor for portal vein thrombosis (PVT) in patients with advanced chronic liver disease (ACLD).[2] In line with our findings, Bitto et al provide data demonstrating that von Willebrand factor levels significantly differ between patients with O and non-O blood types in early stage ACLD, but not when they have more advanced disease.[1] These data thus confirm that the impact of liver disease severity on von Willebrand factor (VWF) levels overrules the effect of blood type.

The authors comment that the factor VIII/protein C (FVIII/PC) ratio better mirrors the procoagulant state of cirrhosis than VWF or FVIII levels, which is why the PVT risk in patients with ACLD is independent of blood type. In other words, the authors are suggesting that hypercoagulability rather than endothelial dysfunction-related VWF-release followed by VWF-mediated platelet thrombus formation is the main driver of PVT in ACLD. This is an interesting thought which is in line with the use of anticoagulants (rather than antiplatelet agents) in the management of PVT. However, we wish to provide additional comments.

Firstly, it remains unclear whether a hyperactive hemostatic system is causally related to PVT in ACLD. An alternative theory is that reduction of portal flow is the main driver of PVT development and TIPS implantation which redirects blood flow by decreasing resistance has been reported to be similarly effective as anticoagulation.[3] Even though markers of hypercoagulability may be associated with risk of PVT, this association is not necessarily causal. Notably, some of the markers used to signal hypercoagulability are strongly associated with severity of liver disease, and it may very well be that liver disease severity on its own (unrelated to hemostatic changes) drives the increase in PVT risk.

Secondly, although ratios have become popular in the studies of coagulation in liver disease, and have been associated with clinical features, there are important caveats to the use of these ratios. The hypercoagulable state of a patient with cirrhosis is directly identified using thrombomodulin (TM)-modified thrombin generation assays. As we have argued previously, we feel it is mandatory to report endogenous thrombin potential in the presence of TM (ETP + TM) values instead of derivatives such as ETP ratios, as they may be misleading and do not always confer hypercoagulability.[4] A similar argument applies to FVIII/PC ratios which might be useful predictors, but do not necessarily indicate hypercoagulability.

Finally, the call for studies on the effect of blood type on PVT in early stage ACLD is interesting, but as outlined in our manuscript, such studies will be difficult to conduct and of modest clinical impact given the rarity of PVT in these patients.

CONFLICT OF INTEREST

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