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## Authors' contributions

MM and MK contributed equally to formulating the reply.

## Supplementary data

Supplementary data to this article can be found online at <https://doi.org/10.1016/j.jhep.2020.06.023>.

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## Reply to: Correspondence on “High rates of 30-day mortality in patients with cirrhosis and COVID-19”

To the Editor:

We read with interest the letters by Gao *et al.* and Medhat *et al.* on our recently published paper reporting data on the impact of COVID-19 on cirrhotic patients in an Italian multicentre cohort.<sup>1</sup>

Both letters pointed out the limited sample size of our study, which had already been acknowledged as a possible limitation in the discussion section of our paper. So far, no studies have been able to enrol significantly larger cohorts of SARS-CoV-2-infected patients with cirrhosis, although data are warranted to better characterize the natural history of COVID-19 in this setting. However, 2 recent studies seem to confirm our findings. In fact, a single study conducted in North America enrolling 37 patients has confirmed the high mortality rates of patients with cirrhosis and COVID-19, whilst an ongoing international registry has shown a mortality risk similar to ours in 103 patients with cirrhosis.<sup>2,3</sup>

Gao *et al.* raised some concerns regarding the statistical analysis used to predict mortality and suggested that we perform a logistic regression analysis. The logistic regression analysis confirmed that CLIF-OF (odds ratio 1.77; 95% CI 1.24–2.54;  $p = 0.002$ ) and moderate to severe lung failure (odds ratio 1.86; 95% 1.00–3.44;  $p = 0.048$ ) were the only 2 independent predictors of mortality.

Gao and colleagues also suggested that MELD, Child-Pugh and CLIF-OF scores might have been influenced by thromboprophylaxis. However, in our study, these scores were calculated at COVID-19 diagnosis, *i.e.* before thromboprophylaxis was started. As low molecular weight heparin was used in most cases, we do

not foresee any significant impact of this treatment on international normalized ratio values.

Regarding the comments by Medhat *et al.*, we acknowledge that patients with cirrhosis and COVID-19 were significantly older than those with cirrhosis without COVID-19. However, at multivariate analysis including both patients with cirrhosis and COVID-19 and those with cirrhosis only, COVID-19 (hazard ratio 3.594; 95% CI 1.465–8.819;  $p = 0.005$ ) and CLIF-OF (hazard ratio 1.369; 95% CI 1.219–1.539,  $p \leq 0.0001$ ) but neither age nor comorbidities, independently predicted mortality. Moreover, at multivariate analysis, the presence of comorbidities did not independently predict mortality in the 50 patients with cirrhosis and COVID-19.

Finally, we agree that acute-on-chronic liver failure (ACLF) was not the cause of death in most of our patients, as respiratory failure accounted for 71% of deaths. However, we demonstrated that the mortality rates were significantly higher in our cohort of patients with cirrhosis than in a control-group of non-cirrhotic patients with COVID-19. Moreover, ACLF at COVID-19 was diagnosed in 14 (28%) patients and both the CLIF-OF and CLIF-C scores independently predicted mortality in our cohort.

These findings suggest that SARS-CoV-2 infection had a precipitating role in causing deterioration in MELD and Child-Pugh scores. Resulting liver failure has been shown to be (in addition to respiratory failure) associated with mortality in these patients.<sup>1</sup> This is not a surprising, given the well-known effects of infections as drivers of liver disease worsening and death in the setting of cirrhosis.

While the real role of interaction between SARS-CoV-2 and ACE-2 receptors in liver cells is yet to be elucidated, we agree that the long-term effects of SARS-CoV-2 on cirrhosis might deserve attention. As suggested by EASL recommendations on

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the management of patients with cirrhosis during the SARS-CoV-2 pandemic, we still need to implement systems that enable remote management of patients, to reduce their exposure to risk, as we have already suggested for the management of patients with hepatocellular carcinoma.<sup>4,5</sup>

**Conflict of interest**

Massimo Iavarone: Speaking/Teaching, consultant and advisory board for Bayer, Gilead Sciences, BMS, Janssen, Ipsen, MSD, BTG-Boston Scientific, AbbVie, Guerbet, Eisai; Roberta D'Ambrosio: teaching and speaking for AbbVie, Gilead, MSD; Advisory Board for AbbVie, MSD, Research Grant from Gilead; Pietro Lampertico: Advisory Board/Speaker Bureau for BMS, Roche, Gilead, GSK, AbbVie, MSD, Arrowhead, Alnylam, Janssen, Spring Bank, MYR, Eiger.

Please refer to the accompanying [ICMJE disclosure](#) forms for further details.

**Authors' contributions**

Statistical analysis: Massimo Iavarone, Pietro Lampertico; writing of the article: Massimo Iavarone, Roberta D'Ambrosio and Pietro Lampertico.

**Supplementary data**

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**Validation of Baveno VI and expanded Baveno VI criteria to identify high-risk varices in patients with MAFLD-related compensated cirrhosis**

To the Editor:

Metabolic dysfunction-associated fatty liver disease (MAFLD), as a new entity, is a highly prevalent disease affecting more than one-quarter of the world's population, and is the leading cause of chronic liver disease in the United States and Europe.<sup>1,2</sup> We read with interest the recent article by Eslam and Newsome *et al.*,<sup>1</sup> in which they proposed a new definition for MAFLD-related

cirrhosis. MAFLD-related cirrhosis is the result of multiple etiological factors and complex interactions between metabolic dysfunction, poor lifestyle habits and diet, imbalanced microbiota and genetic predisposition.<sup>3–5</sup> MAFLD-related cirrhosis patients with concomitant and disease-modifying etiologies (e.g. hepatitis B or C viral infection and alcohol consumption) are likely to have different natural history and response to management compared to those with a single etiology.<sup>6</sup> Due to limited investigations, protocols for screening gastroesophageal varices among patients with MAFLD-related compensated

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