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## Letter: the efficacy of interferon-free regimens in HCV-related Child C cirrhosis needs careful interpretation—Authors' reply

SIRS,

We are very much indebted to Drs Rezaee-Zavareh and Alavian for the attention they paid to our manuscript.<sup>1,2</sup> We share their concern about the need to provide comprehensive evidences supporting the value of the administration of the new direct-acting antivirals (DAAs) in HCV-infected patients with liver cirrhosis staged in Child C class. Unfortunately, no single study has been so far devoted to this very problematic setting of patients, and what we did was to retrieve information on these patients enrolled in previous studies. Although we scrutinised 10 studies, information could be retrieved for a total of 228 Child C patients only.

Our Iranian colleagues argue that no differential SVR rates were given for patients categorised according to the different HCV genotypes. However, in Table 1, we showed that the majority of patients with cirrhosis were infected by HCV genotype 1, and consequently the reported SVR rates refer entirely to this subgroup of patients. We are not pretending that our claim should be referred also to patients with HCV genotype 2 and 3. In addition, three-quarters of them received a combination of sofosbuvir and ledispavir, and consequently we did several subanalyses exploring the influence of treatment duration and the inclusion of ribavirin only on patients treated with this regimen.

Certainly our colleagues would agree that a final trial evaluating the effect of the new antivirals in patients with Child C cirrhosis is needed: with the appropriate large sample size needed for such a trial, all relevant issues pertaining to the therapeutic regimens, the length of treatment and the need to include ribavirin in the schedule

would be addressed. We are well aware that national and international health authorities<sup>3</sup> do not recommend treatment for these patients: in the light of available information, our work only suggests that such a preclusion towards treating these patients would need a reconsideration.

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## Letter: determining the dominant role of environmental factors in UC development

SIRS,

We read with interest the article by T. Hammer et al.<sup>1</sup> presenting an evidence that environmental factors played a dominant role in ulcerative colitis (UC) development. The study showed that there was a higher prevalence of UC (Men: SIR 1.44, 95% CI=1.10-1.87; Women: SIR 1.36, 95% CI=1.09-1.68) in the first-generation immigrants from Faroe Islands to Denmark than the second-generation (Men: SIR 1.04, 95% CI=0.71-1.40; Women: SIR 1.29, 95% CI=0.97-1.69) and

third-generation (Men: SIR 0.87, 95% CI=0.39-1.66; Women: SIR 1.18, 95% CI=0.62-2.02).

In this article, the authors took into account many factors affecting the interpretation of the study results, such as ethnic, genetic dilution, diet and so on. However, the influence of age between the generations had not been taken into account in the data analysis, which was confirmed to be an important factor in the UC development.<sup>2</sup> In the authors' previous study on the trend of IBD prevalence