The study by Choudhuri et al analyses the frequency and clinical features of AIH in Indian patients with acute/chronic liver disease referred to a tertiary care center. Results were largely predictable, and not unexpected in my opinion: a significant lower prevalence of the disease than the figures reported in Western population, a lower proportion of acute presentation, the predominance of type I autoimmune hepatitis and the association with extrahepatic autoimmune disease. Because epidemiologic studies on AIH are scarce and the vast majority of them are coming from Caucasian population of Western Europe and USA, paper of this sort are welcome and could give us an image of the magnitude and epidemiological features of HAI in other parts of the world.

However, several problems need to be addressed:

INTRODUCTION
1) First of all, in Introduction I suggest to the authors to replace the term “autoimmune liver disease” with “autoimmune hepatitis” (because “autoimmune liver disease” comprise also some other distinctive entities like primary sclerosing cholangitis, primary biliary cirrhosis, autoimmune cholangitis and overlap syndrome, which do not correspond to the given definition) and the term of “liver-associated antibodies” with “serum autoantibodies” (because ANA, ASMA and anti-LKM antibodies are not organ-specific).

METHODOLOGY
2) It is not clearly reported which was the frequency of other causes of acute/chronic liver disease in the study group.
3) It is a persistent confusion in terminology. If the 41 patients fulfilled the IAH Group criteria for the diagnosis of AIH, the subgroup of patients with CBP serology or cholestatic features represent an overlapping syndrome. Therefore, I suggest that “cholestatic autoimmune hepatitis” patients should be considered and denominated as “overlap syndrome” based on mixed serology and biochemical cholestasis and I am wonder about the absence of cholestatic lesions in the presence of jaundice and raised bilirubin values. I suggest the authors to introduce in Methodology definition criteria for overlap syndrome.

It would be interestingly to comment distinctly on patients with overlapping syndrome with HCV/HEV concurrent infections or CBP. Which was the frequency of the overlap syndrome? Were these patients “probable AIH” when IAHG score was calculated? Did they respond to immunosuppressive therapy? Did they need UDCA? It was used only UDCA or in association with immunossuppressive therapy?
4) Also, I suggest that the last sentence on page 3 to be part of methodology and patients’ selection.

RESULTS
5) Results were largely predictable based on data published in the literature and does not provide any clue or comparison with previous data: low relative (to other etiologies) prevalence of AIH in a
population with a high carriage rate for hepatitic viruses; female gender and young adults predomination, delayed diagnosis, extrahepatic autoimmune diseases' association, 30% of autoimmune cirrhosis at diagnosis, no distinctive biochemical, histological or serological features. 6) The only data I remark as characteristic for this group of patients are the lower proportion of acute presentations and the lower proportion of treated patients. 8) The results are concise and supported by Tables.

DISCUSSION
9) This is not a true prevalence study. AIH frequency is reported relative to acute and chronic liver disease evaluated in a tertiary care unit. The high proportion of viral hepatitis in Indian patients may be responsible for the low contribution of AIH to acute/chronic liver disease and it would be interesting if the author could be able to give us details about the etiology and comment this in Discussions. The real incidence and prevalence of AIH in general population would be a better approach but it requires a more complicated study design 10) Discuss, also, the difficulties of diagnosis and treatment in patients with autoimmune overlap syndrome between AIH-CBP or AIH-viral hepatitis.

Best regards,
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