Reply to Boyd and Lacombe

To the Editor—We agree with Boyd and Lacombe that there is a paucity of prospective longitudinal data on changes in liver fibrosis measured by transient elastography (TE) in human immunodeficiency/ hepatitis B viruses (HIV/HBV)–coinfected patients who start on tenofovir, particularly among patients in sub-Saharan Africa [1].

TE has been validated as a noninvasive measure of liver fibrosis with good diagnostic accuracy, including evaluation in HBV infection using histologically determined fibrosis scores as the gold standard [2]. Most data arise from Western or Asian regions in the setting of HBV mono-infection, and there remains a need for validation studies in African populations and in HIV/HBV coinfection [3, 4].

There is mounting evidence of the independent prognostic value of TE in predicting subsequent liver-related mortality, decompensation, and development of hepatocellular carcinoma [5–7]. It may be argued that TE has challenged liver biopsy for position as the gold standard in assessing hepatic fibrosis and prognosis. A larger area of liver is examined using TE. In addition, problems of inter- and intra-observer variability and sampling error due to inhomogeneous fibrosis that is associated with liver biopsy are less likely to affect TE [8]. Furthermore, TE permits frequent reassessment and can be used as a measure of responses to therapy, so long as the limitations of the technology are considered, including variation with meals and false elevation with cholestasis, steatosis, and acute transaminitis [8–11].

The data presented by Boyd and Lacombe are interesting and demonstrate highly variable TE responses among French HIV/HBV–coinfected patients treated with tenofovir, with most of the improvement in TE scores occurring within the first year of treatment [1]. In the data presented, a number of patients had increasing TE measurements while on treatment, and it would be interesting to understand the underlying mechanisms and the contribution, for example, of poor treatment adherence, or hepatitis C or delta superinfection. It would be important to correlate their finding with results of HBV DNA suppression; while more than 80% of the total cohort under follow-up at 6 years (n = 172) achieved HBV DNA suppression, as noted in their accompanying article, results for this TE subgroup (n = 28) are not presented [12]. The data overall suggest that there may be limited improvement beyond the first year of tenofovir treatment. This is in contrast with evidence from larger studies of HBV mono-infected patients that showed marked ongoing improvements after the first year and for up to 5 years of treatment [13]. If such a finding can be confirmed in a large sample with long-term HBV suppression, the unique HBV immune pathogenesis associated with HIV coinfection may be found to be responsible.

Even if no further improvement is shown to continue beyond the first year with TE, it is reasonable to propose that by effectively limiting HBV replication among patients with a high rate of lamivudine resistance and virological breakthrough progression of fibrosis and liver-related morbidity can be prevented. Our findings of high rates of HBV DNA suppression and early regression of TE-determined fibrosis with tenofovir in HIV/HBV–coinfected patients extensively exposed to lamivudine remain highly encouraging [14]. We agree, however, that long-term data are required and have planned to undertake further follow-up of the participants of hepatitis B infection in Kumasi, including further assessment with TE. We thank Boyd and Lacombe for highlighting the need for further research.

Note

Potential conflict of interest. Both authors: No potential conflicts of interest. Both authors have submitted the ICMJE Form for Disclosure of Potential Conflicts of Interest. Conflicts that the editors consider relevant to the content of the manuscript have been disclosed.

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