Schubert, et al. have presented an interesting case of hepatitis B reactivation in a stem cell transplant recipient over 3 years after allogeneic HSCT in the absence of chronic GVHD. While the case is worth reporting there are some substantive issues that should be addressed in order for this case report to merit publication. In addressing these issues the introduction, discussion, and conclusions sections would likely need significant revision.

MAJOR COMPULSORY REVISIONS
The authors spend time discussing seroreversion (also termed “reverse seroconversion” in HSCT literature) in all patients receiving immunosuppression and report a vast range of incidences. It is worth noting that this is a heterogenous group which includes patients receiving therapy for lymphoma with and without rituximab as well as HSCT recipients. It is not surprising that the risk is highly variable among these different types of patients. Since this case report is about an HSCT recipient this reviewer suggests that the introduction and discussion focus on seroreversion in HSCT recipients only. The risks for seroreversion have been studied previously specifically in this population (see a list of appropriate publications to review below).

However, the most critical issue with the introduction and discussion of this case report is that the literature review is inadequate. The authors rely on several publications (many case reports) that assess HBV reactivation in a small number of HSCT recipients with previously resolved infection (seroreversion/reverse seroconversion) to draw several conclusions (references 1-5, 12, 15, 16, 18--> reference 17 is a duplicate of #3) such as:

- Introduction: “Nevertheless, taking in to consideration. . . the risk of HBV seroreversion may still reach 100%”
- Conclusions: “. . .no well-defined risk factors for HBV seroreversion in patients with previously resolved HBV infections are available in the literature”
- Conclusions: “Interestingly, according to the literature, patients with. . . .then those who are only HBCAb-positive.”

A more complete literature review would reveal that these statements are not true for HSCT recipients with seroreversion. The authors have failed to review four of the five largest studies to assess seroreversion/reverse seroconversion in the HSCT population including:
5. Vigano, et al. (reference 3 and repeated as reference #17--> the only reference of the 5 that the authors have reviewed).

MINOR ESSENTIAL REVISIONS
As noted above, there is a duplicate reference (numbers 3 and 17) that should be corrected

DISCRETIONARY REVISIONS
The observation that the patient had two HBsIE mutations is interesting and could be discussed further. As the authors point out, this has only been reported in two other HSCT recipients (reference 2). One point of interest would be whether the donor was vaccinated against HBV before donation or the recipient was vaccinated for HBV after HSCT--Knoll, et al. suggest that the escape mutant detected in their cohort may have resulted from donor pretransplant vaccination.

The observation that the case patient developed seroreversion late after transplant is also worth emphasizing. This has been observed in other HSCT recipients (Park, et al. and Hammond et al.—where cumulative probability of seroreversion among patients surviving 4 years after transplant was >40%). The point that the authors conclude with about the questions of prophylaxis duration is a keen observation given the timing of reactivation in this patient.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests