Author's response to reviews

Title: Posttransplant Lymphoproliferative Disorder Involving the Ovary as an Initial Manifestation: a case report

Authors:

Takamitsu Inoue (takamitu@doc.med.akita-u.ac.jp)
Shigeru Satoh (shigerus@doc.med.akita-u.ac.jp)
Mitsuru Saito (mitsaito@med.akita-u.ac.jp)
Yohei Horikawa (horikawayo@gmail.com)
Norihiro Tsuchiya (tsuchiya@med.akita-u.ac.jp)
Tomonori Habuchi (thabuchi@doc.med.akita-u.ac.jp)

Version: 2 Date: 28 December 2008

Author's response to reviews: see over
December 26th, 2008

Professor Michael Kidd
Editor-in-Chief
JOURNAL OF MEDICAL CASE REPORTS

Dear Professor Michael Kidd,

Thank you very much for giving us a chance to revise the manuscript entitled “Posttransplant Lymphoproliferative Disorder Involving the Ovary as an Initial Manifestation: a case report” (manuscript ID: 1783268781231399). I am herein sending the revised manuscript modified according to the reviewers’ comments.

For Reviewer Dr. Tomokazu Shimizu,
Minor revision

1) Comment (1): To prove EBV-associated PTLD, you should show the presence of EBV in the cells by in-situ hybridization for EB-ER (Epstein Barr early RNA).

Re: We think the reviewer’s comment stands to reason. I guess it is not enough to probe that the PTLD in this patient is according to the EBV infection by using only LMP-1 immunohistochemistry. However, actually, the PTLD in this patient occurred in 2002 and the formalin-fixed paraffin-embedded tissue was already discarded, unfortunately. Moreover, now the first author is in another country and it is impossible to perform the in-situ hybridization for EB-ER and show the result.

2) Comment (2): In this report, the assessment of clonality is lacking, so you should demonstrate molecular genetic analysis. DNA extracted from formalin-fixed and paraffin-embedded tissue of the patient should be analyzed for immunoglobulin heavy chain (IgH) gene rearrangements using polymerase chain reaction (PCR)-based methods and you should exhibit this result. I think the monoclonal B-cell is important in this report, because the reduction of immunosuppressant for treatment of PTLD alone leaded to remission of the liver involvement, so you should discuss association between the clonality and efficacy of reduction of immunosuppressant.
Re: I agree it is sometimes important to investigate the clonality of the B-cell in PTLD. A distinction of clonality between polyclonal and monoclonal proliferations has been assumed to be helpful for the clinicians making critical decisions. However, we think this issue is controversial. Nalensik et al (Curr Probl Surg 25, 367-472, 1988) showed that the cases that are polyclonal tend to regress with reduced immunosuppression. This evidence is opposite to the reviewer’s opinion. On the other hand, Pinkerton et al (Br J Hematol 118, 456-461, 2002) reported that neither clonality nor any specific pathological subtype predicted response to a reduction in immunosuppression or clinical outcome. Moreover, Dunphy et al (Am J Clin Pathol 117, 24-28, 2002) illustrated that 50% cases of PTLD had discrepancies in the analysis of clonality by genotyping and flow cytometric immunophenotyping.

In the same reason as comment 1, it is impossible to perform the PCR method to determine the clonality in this case, unfortunately. The most important thing we want to show in this case report is that PTLD can be presented in the ovary as an initial manifestation rather than that this case was treated only by the reduced immunosuppression.

3) Comment (3): Dose the sentence 'acute cellar rejection' mean 'acute T-cell-mediated rejection'?

Re: According to the reviewer’s recommendation, we replaced the sentence 'acute cellar rejection' as 'acute T-cell-mediated rejection' in Page 4, line 10.

4) Comment (4): I think the sentence 'serum tacrolimus concentration' should change to 'serum tacrolimus trough level'

Re: According to the reviewer’s recommendation, we replaced the sentence 'serum tacrolimus concentration' as 'serum tacrolimus trough level' in Page 4, line 14.

We would like to thank you for your helpful comments and hope that we have now produced a better account for our work. We hope that the modified manuscript is now suitable for publication in JOURNAL OF MEDICAL CASE REPORTS.

Sincerely yours,
Takamitsu Inoue, M.D.
Department of Urology Akita University School of Medicine
1-1-1 Hondo, Akita 010-8543, Japan
Phone: 81-18-884-6156
Fax: 81-18-836-2619
e-mail: takamitu@doc.med.akita-u.ac.jp
takmitz@gmail.com