LETTER TO THE EDITOR

Mar 17, 2013
Dear Editors:

Regarding the Feb 2013 case report "Regression of Mucosa-Associated Lymphoid Tissue (MALT) Lymphoma of the Cecum after Eradication of *Helicobacter pylori*", by Manish Prasad Shrestha, Tat-Kin Tsang (Practical Gastro Feb 2013 pp. 42-45), the cecal MALToma was removed endoscopically. There was no proof that any further MALT existed, so to claim that treating the h pylori had any effect is spurious. If the authors after removing the MALToma had repeated the colonoscopy immediately and found on further biopsies that the MALT still existed (before H pylori rx), then they could have proven their case.

Marvin Chinitz, MD, FACG, AGAF, Associate Clinical Professor, Albert Einstein School of Medicine, Senior Gastroenterologist, Mt Kisco Medical Group, Mt Kisco, NY

AUTHOR RESPONSE

Apr 19, 2013
Dear Ms. Mahl:

First of all, I want to thank Dr. Marvin Chinitz for his review of our article. I would like to respond in the following way.

At the time of performing colonoscopy, we did not know that the patient had cecal MALToma. It was an incidental finding. Unfortunately, We did not repeat colonoscopy immediately. Therefore, we could not definitely say that there was no residual MALToma after polypectomy. There is a possibility that the patient could have had residual MALToma. Patient was also seen by an oncologist, who after reviewing pathology and clinical history, recommended patient to be treated with Rituximab. However, patient did not want any chemotherapy. Patient got treated for *H. pylori* infection and that was the only intervention patient had, besides polypectomy. We believe that the *H. pylori* eradication may have played an important role in the regression of MALToma besides polypectomy. As suggested in the discussion of our article, *H. pylori* in the stomach may have provided an antigenic stimulation to the colonic mucosa and the removal of this antigenic stimulation could have contributed to the regression of MALToma.

The purpose of our article was "hypothesis generation", so that further studies could be conducted for definite answer. It was not meant to prove any cause-effect relation. The situation suggested by Dr. Chinitz would have been an ideal case, but this was not our case.

Manish Prasad Shrestha, MD, Assistant Professor of Medicine, Department of Medicine, University of Arizona Medical Center, Tucson, AZ

AUTHOR ADDENDUM/CORRECTION

We regret the following oversight in our article,"Prognosis in Acute Pancreatitis", by Dr. Alexander Brun, Dr. Neelam Gidwaney and Dr. C.S. Pitchumoni that appeared in our March 2012 issue (Volume XXXVI, No. 3, pp. 16-41). Specifically, in Figure 1 on page 18, "pulse > 60 beats per minute" was listed as one of the SIRS criteria. This should have read, "pulse > 90 beats per minute." ***The Editor***s