Reviewer's report

Title: Utilization Pattern of Traditional Chinese Medicine for Liver Cancer Patients in Taiwan

Version: 1 Date: 20 October 2011

Reviewer: Laura Weeks

Reviewer's report:

The goal of the research is clearly stated: to analyze the utilization pattern of TCM by patients with liver cancer from 1996-2007, using a population-based random sample of 1 million insured patients. The authors have accessed a comprehensive and population-based dataset with reliable data.

The literature review is a bit sparse and a few references out of date (e.g., there is an update to the 1998 Eisenberg paper published in 2007 by Barnes et al), but the authors acknowledge a few key reports directly related to their work and to which their work adds further knowledge.

The methods are appropriate, although I would like to see a bit more depth to the analysis, for example regarding TCM use only, WM use only and combined TCM and WM, as outlined below.

The writing is acceptable. There are a few grammatical corrections needed.

Overall, the paper will be of interest to a limited set of professionals working in the field of liver cancer, and TCM and liver cancer specifically.

Some suggested revisions include:

Discretionary Revisions

• Coexisting diseases (page 7): The text is restating what is in Table 3 but with less detail and no reference to the table. Recommend redrafting this section and using the text only to highlight similarities and differences between groups in Table 3, and adding a reference to the Table.

• In Figure 1, the authors separate out use of TCM alone, WM alone, and TCM and WM combined. This is an interesting analysis as it highlights an important issue re: combining different approaches to care. It would add depth to the paper if the authors could pursue this issue further, and further analyze the pattern of TCM use by separating out how many use TCM alone and how many combine TCM with WM.

• In the conclusion the authors state that an increasing trend in TCM use was not found in the study period. This should be changed to “no trend” neither increasing nor decreasing, as no rationale is provided to suggest why we might expect TCM use to be increasing.
• I disagree that 65.63% is “much” higher than 54.99% in regards to the proportion of TCM vs. WM outpatient services provided by private clinics (page 7: Medical institutes). Do the authors mean “significantly” higher? A p-value would help readers to understand the difference.

• Could significance values be added to help interpret the differences between WM and TCM expenditures? (page 8: expenditures)

Minor Essential Revisions

• It would be helpful if the authors explained why more recent data than 2007 is not available.

• It needs to be clarified whether the 6,358 visits to TCM practitioners for liver cancer is the total number of VISITS or the total number of PATIENTS, as it is likely that one patient would visit a practitioner more than once and thus #visits should be greater than #patients. If visits, it would be helpful to know how many unique patients are within the total. If it is not possible to make this distinction from the LHID2005 dataset, this limitation must be stated.

• Table 3, WM for liver cancer, under DM number of visits there is an extra comma “,”

• Table 3, TCM for liver cancer, under malignant neoplasm of the liver and hepatic bile duct, the “,” is in the wrong place “1,9847” and not “19,847”

• Add to the list of abbreviations to correspond to all those used in the text, e.g., LC, WM, CAM, NHI, BNHI, LHID2005, LC, etc.

Major Compulsory Revisions

None.

Level of interest: An article of limited interest

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