Reviewer's report

Title: Utilization Patterns of Chinese medicine and Western medicine under the National Health Insurance Program in Taiwan, a population-based study from 1997 to 2003

Version: 1 Date: 29 August 2007

Reviewer: David Legge

Reviewer's report:

General

I think that these data are interesting and that they should be in the public domain but there are expectations that the purpose and logic of the study should be articulated.

Major Compulsory Revisions (that the author must respond to before a decision on publication can be reached)

The research questions are not well articulated. More importantly the policy (or other) significance of this descriptive study is not articulated. Why do the comparative utilisation patterns of CM and WM matter?

The discussion of the context is also thin. There is mention in the body of the report that the density of practitioners of each tradition may be a significant determinant of utilisation. There is no discussion of price effects. Cheng T-M. (Taiwan's New National Health Insurance Program: Genesis And Experience So Far. In Health Affairs, 2003, 22(3): 61-76) point out that copayments are significant and have been somewhat controversial. Clearly levels of copayment could influence the income related variations in utilisation. Should be mentioned.

In the Background section of the Abstract there are two sentences on of which is background, the other describes the purpose of the study.

I am not a statistician but I have some concern about the use of GEE for deriving utilisation rates. In particular it is not clear to me that using GEE to derive increasing utilisation rates from longitudinal data enables correction for increasing population. The per capita visit rate, which clearly does correct for changing denominators does not show the increasing utilisation rate that the GEE regression shows.

See Page 15 (Conclusions): “In this study, we revealed a steadily increasing trend in the utilization of CM.” Should perhaps acknowledge that the rate of increase in the use of WM was greater than the rate of increase in CM (Table 2) and that the increase is not showing up in the number of visits per patient.
Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

In the second para under Background WM is rendered as WN

English language is not everywhere beautiful but is generally clear.

Top of page 11: “WM predominates [(in) the system]”

Page 11 “In this study, we employed the GEE method to determine the demographic factors influencing the utilization patterns of CM and WM.” Need to be careful about imputing causal association.

Also Page 11: “It has been suggested that CAM is highly expected in relieving unfavourable conditions in postpartum conditions, menopause and chronic diseases among women”. Choice of words might be worth reconsideration.

Also Page 11: “Moreover, disorders of menstruation and abnormal bleeding, non-inflammatory disorders of female genital organs, and inflammatory disorders of female pelvic organs have been determined in the most common disease groups for CM ambulatory visits.” Not sure what this means

There are similar English writing anomalies elsewhere in the paper also.

Discretionary Revisions (which the author can choose to ignore)

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I declare that I have no competing interests.