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: 5 November 2007

Reviewer: Lisa Lix

Reviewer’s report:

General

This is a generally well-written manuscript that represents a potentially important contribution to the literature on utilization of alternative health services. However, I do not believe that the results adequately represent the key findings of this research. I have provided a number of specific comments about revisions that will help to clarify both the methods and results sections of this manuscript.

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

The authors report that only 68.2% of the random sample of NHI beneficiaries was found to have valid and complete enrolment and utilization data. The reasons why almost one third of the sample was excluded from the analysis should be described in the manuscript. This is a potentially significant source of bias in this study of trends in health services utilization.

In the methods section, the authors note that the NHI claims data contains information on the drugs prescribed and filled and dispensing method. Why were data on prescription drugs and dispensations not included in this study of utilization patterns? This seems to be a major gap in the analysis.

The way in which the results from Table 2 are described in text is not correct. For example, the third sentence of the Results section should be written as follows: “The odds of using CM (OR = 1.46) and WM (OR = 1.76) were higher for females than for males (OR = 1.00).” Throughout the discussion of the regression results, the authors must be careful to refer to the odds of utilization, not the frequency of utilization. Also, I do not recommend that the authors repeat the upper and lower bounds of the 95% confidence intervals in text; they have already provided this information in Table 2. Finally, there is a lot of repetition of the Table 2 results in the body of the manuscript; I would suggest that this presentation of material be shortened.

Was interaction effects tested in the generalized linear models for Chinese medicine and Western medicine, as reported in Table 2? If not, why not? The change in utilization over time may not be constant for all age, sex, or
socioeconomic groups.

The regression coefficients for year (Table 2) show that there was an increase in the odds for each year relative to the reference year. Why did the authors not include year as a continuous variable in the model, or at the very least estimate and test the linear trend across the levels of the categorical year variable? Either of these analyses will help to support the author(s) conclusions that there is an increasing (and possibly linear) trend over time in the use of Chinese medicine and Western medicine. Including year as a continuous variable in the model would facilitate the testing of interaction effects between time and other model covariates such as age, sex, and socioeconomic status.

The form of the working correlation matrix for the generalized estimating equations should be specified. For example, was it an autoregressive structure or a compound symmetric structure? A justification for the choice of the working correlation matrix must also be provided.

The results, as described in the conclusions and abstract, are potentially misleading. According to the regression analyses, the odds of using both Chinese and Western medicine increased over time in the NHI sample. However, given the way that the year effect was included in the model, it is not clear whether the relative rate or increase in the odds was greater for Chinese or for Western medicine. This point can be clarified using the methods I described previously.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

The authors indicate that the data are from a random sample of NHI beneficiaries. Is this a simple random sample or a stratified random sample? What proportion of the total NHI beneficiaries is represented in the sample?

Provide a reference for the Illness and Injury Severity Score described on page 6.

The authors should note that there were repeated measurements on both the year and season variables.

The acronym NT, found on page 6 of the manuscript, should be defined.

What types of occupations might be represented in the “other” category, which was used to characterize individuals without a well-defined monthly wage?

A reference should be provided for version 9.13 of SAS.

A reference(s) should be cited for the method of analysis (i.e., generalized linear model with generalized estimating equations).

In Table 3, the standard deviation should be provided along with the mean, to assist the reader in understanding the variability in the average number of visits.
Also, the authors should consider modeling the number of visits per patient using a generalized linear model, to test whether the relative rate of utilization is changing after controlling for important patient characteristics such as age, sex, etc.

In Table 5, it should be clear that the percentages reported in each column sum to 100. Also, the title of this table is misleading because the numbers reported are percentages, not frequencies.

Discretionary Revisions (which the author can choose to ignore)

The term “gender” should be replaced with “sex” throughout the methods and results sections.

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: 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.