Reviewer's report

Title: Patient risk profiles and practice variation in serious nonadherence to antidepressants, antihypertensives and oral antidiabetics

Version: 1 Date: 24 January 2007

Reviewer: Manel Pladevall

Reviewer's report:

General

This is an interesting and well-written paper. The topic is relevant and the linking of practice and prescription databases is appropriate to respond the research questions. The importance of practitioner factors in explaining adherence levels is warranted and relevant. However, I have some reservations before recommending publication (see below).

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

The authors use adherence as a binary variable with a cutoff at the 80% level for all drugs. They argue that when adherence is < 80%, nonadherence is serious. What does serious mean and where is the evidence that < 80% is serious nonadherence? It is true that, when dichotomizing adherence, several authors have used the 80% threshold but this is arbitrary. The relationship between adherence and outcomes is drug and disease specific and the definition of nonadherence should be based on a threshold that is relevant for outcomes. Also, I don't understand why the authors didn't use a continuous measure of adherence such as CMA/CMG or medication possession ratio. If they had done so, their statistical analysis would have been more powerful. (1) Moreover, with a continuous adherence variable, they could have estimated the intraclass correlation coefficient for adherence levels by practice.

One year of observation might be a too short period of observation for measuring adherence in chronic diseases. How did the authors deal with the issue of the terminal gap? How did they differentiate a gap due to nonadherence from a gap because of a medical indication (the drug was stopped by the doctor because of side effects or lack of response)?

The authors should have at least differentiated between the biguanides and sulfonylureas classes among the antidiabetic drugs. Did they include patients who were taking insulin?

Adherence to diuretics might have been low because of the dose used. What was the typical dose used for diuretics? Also the literature is not consistent regarding the low levels of adherence for diuretics and the authors should have addressed this in the discussion (2,3)

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

4th line in the methods section: a coma or a parenthesis is missing between dropout and i.e.

Table 1, third column. 2.59 is a significant estimate but it lacks the asterisk. The same for 0.97. The footnote should also include an explanation on how to interpret the results for refill adherence. The same applies to table 2.

Discretionary Revisions (which the author can choose to ignore)

Reference List

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: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests