Reviewer's report

Title: Impacts of Pharmaceutical Promotion On Prescribing Decisions of General Practitioners

Version: Date: 19 February 2007

Reviewer: Barbara Mintzes

Reviewer's report:

General
The article is much improved, with the analysis further fleshed out, and more details provided on methods. This is great to see. However, I still have some recommendations for changes. The most important one is the need to include all GP responses in the analysis. Secondly the issue of reliability of self-reported influence of drug promotion needs to be discussed.

In general, much more work is needed on the analysis and presentation before the article is publishable, but this is a very interesting study that should be published, for 2 main reasons:
1) there is so little published research on drug promotion in developing or middle income countries that it would be a shame to reject this article;
2) the study is interesting in comparison with the existing research on influences of drug promotion on prescribing because of the high levels of reported influence among GPès and the identification of the relationship between very brief time per patient and higher promotional influences.

I have not addressed the changes needed to the language and presentation because English is not the authors’ first language.

Abstract:
If the main aim is to determine the impact of promotion on prescribing decisions, the results should focus more specifically on this. The information on mean time per patient should focus on the relationship to self-reported influence of pharmaceutical promotion and/or amount of contact with pharmaceutical representatives. The reciprocal relation between time per patient and number of patients per day is obvious – not worth mentioning. The abstract could be briefer and needs to focus on key outcomes related to the central study hypothesis and aims.

It is worth signaling, even in the abstract, that the degree of influence self-reported by GP’s is likely to be an underestimate, based on other studies of self-reporting of influences on promotion. This is worth discussing as well in the body of the paper. The introduction refers to Avorn’s study and the recent UK study of GP’s, both of which show that self-report resulted in an underestimate and was inaccurate. As the current study relies on self-reported influence this needs to be brought centrally into the discussion of the results of this paper.

Background
The authors should explain why they believe that # of patients per day, work setting, year of practice, and gender are expected to influence the degree to which promotion affects prescribing behaviour. This could be addressed either in the background or discussion section of the paper – ideally briefly in background.

Methods:
It is useful to have the extra information provided on study design. The description on the questionnaire however (starting “there were 5 questions” + following paragraph), could be dropped or cut down considerably– no need to have this much detail. A box could be included instead listing the questions (if brief enough) or the content areas. A general statement could be made about likert scale responses. Although a 6-point likert scale was used, it was analyzed as a 3-point scale, and then intermediate responses excluded in some cases. I have suggested below dichotomizing instead given that they are carrying out chi-square analyses. This leads to a loss of precision, but given that the data are self-reported opinions this is probably OK.

Likert scale responses can also be analyzed using non-parametric tests, such as the Mann-Whitney test, the Wilcoxon signed rank test or the Kruskal-Wallis test. They may want to carry out some analyses of the full data set using a non-parametric test.
Often mean scores are used and t-test comparisons made for Likert scale data. This is inappropriate as it is non-parametric data and the difference between each data point is not necessarily standard (i.e. difference between frequently and sometimes versus sometimes and rarely). Presentation of mean scores also creates an abstraction that makes results harder for the reader to understand (what does 3.2 mean versus 3.5 in conceptual terms – not much). I am therefore not suggesting this.

If possible, I would recommend that the authors discuss the data analysis with a statistician from their university. If there is no one available, perhaps the journal could recommend someone. The current analysis is very weak and the main barrier to publication (further discussed below).

Results:
Table 2 could be skipped or if it is included, clearly labeled as ‘self-reported’ information sources and influences. Other literature that examines the accuracy of self-reported information sources and influences on prescribing needs to be discussed in the paper. For example, Avorn’s 1982 study found a large gap between self-reported influences and beliefs in commercial myths. Do you have reason to believe that your self-reported results are more reliable? Why or why not?

The decision to exclude responses classified as ‘sometimes’ is unacceptable. These are apparently responses 3 or 4 on a 6-point Likert scale. This scale can be dichotomized for simplicity of presentation (and also to allow for a simple chi square analysis), for example grouping those who said they were sometimes or always affected versus those who said they were not affected. Another option, given that this is a descriptive study, is to make the analysis empirically data driven and compare those whose answers were below and above the median for each. However, it is inappropriate to exclude respondents because of how they answered the survey. The categories appear to total to 93 in this table (61% of GPs) although 152 participated in the study. It is not clear what happened to the extra 14 (152=45+93+14). It is also unclear whether the percentages in Table 3 are of the entire cohort or of those that remain after this subset is excluded. Also Table 3 could be clearer about what the dependent variable is.

The column with p values above or below .05 is not particularly informative. Testing should be adjusted for multiple comparisons and rather than a separate column a star could be used with note below the table to identify relationships that were found to be significant at p less than .05 after adjustment for multiple comparisons (for example a bonferroni adjustment). This study is exploratory, with no predetermined primary outcome measure and any analyses need to clearly be labeled as exploratory.

Discussion
Discussion on time per patient needs to be framed in terms of main study aims – it is interesting in itself but the question addressed here is effects of promotion.

In discussing what materials are used for prescribing, it would be worth highlighting that this is self-report and referring to results both of Avorn’s study and the recent UK GP study (Prosser et al) – both identified a large gap between sources physicians said in general they relied on, and either beliefs in specific information or information used as a basis for specific prescribing decisions (on critical incident analysis), both of which were much more influenced by promotional information.

Omitting some data does not increase the reliance of self reported information. If there are any internal validity checks (correlation between specific answers that are less likely to be affected and those more likely to be affected) it would be worth mentioning. Otherwise, the results need to be understood within the limitations of self-reporting. They remain interesting and relevant.

The discussion of education on prescribing is unclear. If this is about continuing medical education, it should be stated. Otherwise, I assume it refers to physician education as medical students (seems unlikely from the context). This also needs to be clarified where education on prescribing is discussed elsewhere in the paper.

Sample size:
This is discussed as a limitation without any calculation of the sample size needed to identify differences between groups. To do so the authors would need to have a primary hypothesis and to identify a clinically meaningful expected degree of difference between subgroups. This cannot be reconstructed a posteriori. I would suggest instead that the authors clearly state that this study is descriptive and exploratory, within a single province in Turkey, and that they refer to the excellent response rate as a strength. Similarly, there is no need to discuss why a specific statistical analysis was not used (logistic regression).
Conclusion
I would add a reference in the conclusion to the link between higher self-reported reliance on promotion and brief patient visits and also the link between higher self-reported reliance on promotion and attendance of CME funded by the pharmaceutical industry. Many BMC public health readers may not be familiar with the Alma Ata declaration and may not see the direct link from study results. It is worth briefly stating the specific relevant recommendation as well.

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

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

---------------------------------------------------------------------
Discretionary Revisions (which the author can choose to ignore)