Reviewer’s report

**Title:** Acceptability and effectiveness of opportunistic referral of smokers to telephone cessation advice from a nurse: a randomised trial in Australian general practice

**Version:** 2  **Date:** 25 December 2007

**Reviewer:** Scott E Sherman

**Reviewer’s report:**

Overall, this is a clearly written paper outlining a well-done study of an important intervention.

Major compulsory revisions

1. The article desperately needs a power calculation, either a priori or after the fact. In actuality, there was not really ever enough power to find a difference in cessation rates. The proper way to do the calculation is to factor in both the rate of uptake of the intervention and its effectiveness. Therefore, an intervention that increases the quit rate from 8% to 12% and is received by 50% of the population will result in a population quit rate of 10%. Comparing 8% to 10% (a reasonable estimate for this study) would require 265 patients in each group to have an 80% chance of showing a difference. This is why many studies use surrogate markers, such as the quit attempt rate. For example, if the intervention increased the rate of quit attempts from 30% to 50% and was again received by 50%, it would result in a population quit attempt rate of 40%. To have an 80% chance of detecting an increase from 30% to 40% would require only 30 subjects per group.

2. The other major problem I have with the study is that it is unclear whether the providers actually carried out the intervention and to what extent. Having carried out a number of interventions directed at providers, this is a crucial and often unreported factor. It could have had a wide range of effects in this study, and I will illustrate two obvious ones. First, if the providers did not actually offer the telephone counseling intervention or did it in a quick but ineffective way, there will of course be no effect of the intervention. Alternatively, if seeing the patient survey lead providers to do a better than usual job (since they knew they were being measured), there would have been less difference between the groups. So what data are there on how the intervention was actually delivered? The only thing I see, which is quite indirect, is that at 6 months, there was no difference between the two groups in the percent of patients reporting being referred to a smoking cessation clinic or counselor. The fact that there was not a difference in quit attempts despite adequate power makes me suspect the intervention was not carried out as intensely as one would hope.

3. I would suggest changing the GP smoking cessation advice section. As I understand it and Table 5, you are comparing the recall of smoking cessation
advice during the prior 6 months among patients who had had a subsequent visit to their GP. Since providers will not remember which group a patient was in, why would you expect there to be any difference between the two groups? I think the 6-month survey is better used to assess recollection of receiving the intervention. Therefore, include all survey recipients in the denominator and look at whether patients recall being referred to a telephone counselor (if that was one of the questions). If there is a difference, that suggests patients remember receiving the intervention.

4. The authors should describe what (if any) social marketing strategies were used to maintain adequate levels of physician participation in the intervention? For example, were e-mail reminders sent? Audit and feedback? Academic detailing or opinion leader approaches? [For references for these strategies, you can look in: Sherman et al. Effectiveness of an on-call counselor at increasing smoking treatment. J Gen Intern Med., 2007; 22: 1125-31.]

5. I would suggest omitting Table 2 and including it in the text instead. The only significant result was the stage of change. I would also suggest omitting Table 4.

6. Omit all chi squares and dfâ##s from the article and from the tables. They donâ##t really add anything.

7. In Table 3, I would suggest eliminating the â##test statistic and p valueâ## column and moving the 12-month data to the right of the 6 month data. This will give a table with three rows and six columns of data, which I think will be easier to read.

Minor essential revisions

1. Numbers should in general be rounded to the nearest integer for clarity. (For example, in the Patient recruitment section in the results, use â##83%â## instead of â##82.7%â## and â##50%â## instead of â##50.4%â##.

2. Page 7, paragraph 3, sentence 3 â## The clause â##especially if recruitment bias in introducedâ## is making the sentence harder for me to understand. Please clarify.

3. How many, if any, patients received the wrong intervention? In a study of this size, it is likely that at least a few patients in the control group would have received the intervention by mistake.

4. To the extent that you are able to say, were there differences in intervention uptake by provider? Of course, if the 169 intervention patients were split equally among the 30 providers, it would be hard to say anything. However, it is likely that some providers had 10 patients and others far fewer. I would also suspect that there are big differences among provider in uptake. With the small sample, it may merit simply a paragraph in the discussion.

5. Page 11, paragraph 3, sentence 2 â## Omit â##Wilcoxon rank sum testâ##.

6. Page 12, paragraph 1, last sentence â## The authors should not say â##nine timesâ## more likelyâ## since the data are presented as odds ratios. It would be
more appropriate to simply say “more likely” without including the “nine times” or to say that there was a nine-fold increase in the odds. Readers are likely to find the latter description confusing.

7. Page 12, paragraph 4, second sentence “I found this sentence confusing. Please clarify or simplify.”

8. In the figure, the text “Did not complete questionnaires” needs to be moved up in the Follow-up boxes.

Discretionary revisions

1. I would mention in the discussion that patients having to buy their NRT is a barrier to receiving treatment. This will likely result in a lower population quit rate for both groups, thereby making it harder to find a difference.

2. Page 11, paragraph 3 “I would suggest saying “Patient flow through the study is...” rather than “Patient flows through the study are...”.

3. I think the results would be a bit clearer if the 6-month and 12-month outcome data were grouped together. It would also save space. Thus, you could say “There were no significant difference in study outcomes between the two groups at either 6 months or 12 months (Table 3).”

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

Level of interest: An article whose findings are important to those with closely related research interests

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.