Author's response to reviews

Title: Effectiveness of a mobile smoking cessation service in reaching elderly smokers and predictors of quitting

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Author's response to reviews: see over
Dear Dr. Appleford,

Re: MS: 3912636821971293 - Effectiveness of a mobile smoking Cessation service in reaching elderly smokers and predictors of quitting

Many thanks for your e-mail message of July 16, 2008 regarding the above manuscript. We are very delighted to receive comments and suggestions made by the referees. As suggested we have responded to referees’ comments in the attached pages with appropriate changes made in the revised manuscript, with the changes underlined.

Also, we included a section on author’s contributions on page 15. We have also added the following sentence in the acknowledgement section: “However, Pfizer played no role in the results/data analysis/preparation or approval of the manuscript.”

With best wishes,

Yours sincerely,

TH Lam
Point by point response to reviewers’ comments

Reviewer #1  [Lindsay Stead]

Reviewer's comment: report:
- Discretionary Revisions
  The proportion of light (under 10 cigs/day) smokers seems high in this population and since this was a predictor of quitting it would be worth noting how this population compared with the others for which you give comparative quit rates in the discussion.

Response:
Thank you so much. We have added the follow in the Discussion (paragraph 2):

“While our quit rates seems better than the above-mentioned studies [26-28], it should be noted that our subjects included a greater proportion (70%) who were light smokers (smoked less than 10 cigarettes daily). A higher proportion of subjects in the other studies [26-28] were moderate or heavy smokers. The average daily consumption of our subjects was 10 cigarettes per day compared to an average of 25.4 per day among American smokers [27] and a median of 20 (range 1-85) among smokers in New Zealand [26].”

Reviewer #2 (Shu-Hong Zhu)

Reviewer's report:
Comment 1:
This study reports an innovative approach to recruiting older smokers in Hong Kong into cessation services. It found that mobile smoking cessation services can be a feasible way of reaching elderly smokers. The manuscript could bear some re-writing to make it more informative.

Response 1:
Thank you so much.

Essential Revisions (Major/Minor)

Comment 2: It is important that the paper provide some rough estimate of what percentage of smokers in these units were reached. Figure 1 shows that 10% of social service units approached actually accepted the invitation. Given the cited smoking prevalence for this age group, a rough estimate (even if it is
provided in the discussion section) will give readers a much better sense of how effective this approach is. What the paper in its current form has shown is that the response rate is non-zero (and may be quite substantial), but it will help if a magnitude can be indicated.

Response 2:
To provide a rough estimate, we have added the following at the end of paragraph 1 in the Discussion.

“However, our cessation service was not able to reach all the elderly smokers in the 102 participating social service units. Although we trained social workers in each of these units to refer smokers to our programme, we did not conduct any baseline survey of all the residents in the social service units nor record data about what proportion of their smokers actually attended our programme. However, based on our communication with the social workers, we estimated that our program had reached at least 70% of the smoker population in these social service units.”

Comment 3:
A critical element of the program is provision of free NRT. The paper needs to discuss what role it played in attracting smokers to attend the talks and subsequently to enroll in the cessation program. For example, it is conceivable that just giving out free NRT without requiring smokers to attend clinics might be more effective in terms of enrolling smokers. Using the same cost analysis method adopted by the authors, this approach probably would be more cost-effective (although without a control group, it is not clear what the real effect is.)

Response 3:
We agree with the reviewer’s clarification and added the following in the limitations (page 14, paragraph 1, last sentence).

“It was also possible that free counselling service and offer of 4-week’s supply of NRT free of charge had encouraged many smokers to attend our programme.”

Comment 4:
The comparisons of costs (in US dollars) of cessation programs in New Zealand and Mayo Clinics in the U.S. are not very useful unless readers are provided with other comparative data on costs of various medical and social services. It would be more informative to compare with other cessation services
in Hong Kong such as the costs of other community cessation clinics.

4. Given the unique method of recruitment for the study, it is not very useful to compare the quit rate of this self-selected sample with the quit rates of other countries.

**Response 4:**
As suggested, we have only compared the costs with other available services in Hong Kong (i.e. the smoking cessation clinic) and have now deleted the comparison with other studies.

In relation to the quit rates, we have added the following in the Discussion:

“While our quit rates seems better than the above-mentioned studies [26-28], it should be noted that our subjects included a greater proportion (70%) who were light smokers (smoked less than 10 cigarettes daily). A higher proportion of subjects in the other studies [26-28] were moderate or heavy smokers. The average daily consumption of our subjects was 10 cigarettes per day compared to an average of 25.4 per day among American smokers [27] and a median of 20 (range 1-85) among smokers in New Zealand [26].”

**Comment 5:**
The inclusion of those lost to follow up as non-quitters in the regression analysis (of predictors) is problematic. It might be a conservative approach in estimating the quit rate, but it is not appropriate to make that assumption in running regression.

**Response 5:**
We believe that the conservative estimate to include those lost to follow-up as non-quitters will reduce others’ criticism of over-reporting of quit rates. We used the same approach when we did our regression analysis, also to avoid criticisms of inconsistencies. It is very likely that most, if not all who were lost to follow up were non-quitters. If some of them were quitters, our regression analysis on quitting would have under-estimated the odds ratios, which were already quite high (Table 3).

We have added the following as a limitation: “While our intention-to-treat analysis by treating those lost to follow-up as non-quitters might have under-estimated the quit rate, our regression analysis on quitting could also have under-estimated the odds ratio of the predictors, noting that the odds ratios were quite high already (Table 3)”

**Comment 6:**
The predictor analysis probably could be deleted. The fact that longer use of NRT is correlated with quit rate is expected, as those who relapsed would have stopped using NRT. That the number of cigarettes per day predicts quitting is a well known fact that does not bear repeating.

Response 6:
We agree that these are established factors for quitting smoking but such data are lacking in Chinese or Asians, especially for adherence to NRT. We have added the following to the Discussion: “NRT is still not widely available, too expensive and seldom prescribed and used in Hong Kong and Mainland China. Many smoking cessation counsellors and smokers tend to over-emphasize the efficacy of NRT, and neglect the importance of adherence”. Furthermore, we thought it would be interesting to highlight the two most important predictor variables in Table 3 out of the nine factors that were associated with quitting smoking in the bivariate analysis.

Comment 7:
Table 1 has an entry that appears to be a question for the authors themselves (page 21, on length of quitting: “Any breakdown for this category?”

Response 7:
Thank you so much. We have now corrected the typo.