Author's response to reviews

Title: The potential to improve ascertainment and intervention to reduce smoking in Primary Care: a cross sectional survey

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Author's response to reviews: see over
Reviewer 1

MAJOR COMPULSORY REVISIONS

The low response rate is the major concern and in my mind a fatal flaw to this study. In general, the minimal acceptable response rate I look for in a patient survey is 60%, with 70% being much preferable. I might at times go as low as 50% with major reservations. The fact that the response rate here is approximately 32% essentially invalidates the study results. Particularly with smokers, one does not know to what extent (if at all) the small fraction of respondents is similar to the large fraction of non-respondents. There are already factors associated with responding, such as being female and older. Who knows what differences there are between respondents and non-respondents? The authors could possibly mitigate this major bias by intensely surveying a small percentage of non-respondents. If they were similar to respondents in all measured characteristics, that would provide some reassurance that the results might generalize to the larger sample. To justify the low response rate, the authors provide one reference that is 26 years old. More recent articles suggest that non-response bias is significant for smoking (Solberg LI et al. Does methodology affect the ability to monitor tobacco control activities? Implications for HEDIS and other performance measures. Prev Med. 2003 Jul;37(1):33-40.)

The authors accept that the response rate to the questionnaire is low, and this is a valid point raised by the reviewer. However, repeating the survey to non-responders is not likely to be an efficient way of increasing a response rate as few recipients are likely to reply with the same approach being used. However, survey data is principally not used to derive prevalence – type data and where survey data is used to estimate the proportion of smokers who might want to talk with a smoking cessation advisor (2nd paragraph, 2nd page of discussion), it is used very conservatively as we assumed that all non-respondents did not want help, reducing the estimated percentage requesting help to 13.8%. This still supports the message of this manuscript that only a fraction of those interested in speaking to a smoking cessation advisor are actually accessing services.

DISCRETIONARY REVISIONS

Page 2, paragraph 1 – “is inconsistent with other measures of smoking status.” Is awkward. It might be better to simply say “is inaccurate.”

We agree that the phrase “is inconsistent with other measures of smoking status” is awkward, and have replaced it with the phrase “is often inaccurate”

Page 2, paragraph 2 – The authors point out that 72% of respondents were interested in quitting, but only 8% had been referred. This does not necessarily mean, as the authors suggest in the next sentence, that only a small proportion of smokers were receiving appropriate interventions. In the United States, even when it is offered, the vast majority of smokers interested in quitting are not interested in referral. This has prompted many health
care organizations and systems to focus on getting the help to the patient rather than insisting that the patient go to the help. Thus, for many of those 72% interested in quitting, offering treatment may have been a more appropriate intervention, since that may well be the most intensive intervention that the patients were willing to accept.

Whilst we agree that not all of those smokers interested in quitting would be interested in referral, we think that there are potentially a large number of smokers who would take the opportunity for referral if it were offered. We have, however, slightly rephrased the wording of the sentence to read “this discrepancy suggests that only a small proportion of smokers may be receiving appropriate interventions in primary care”.

Page 2, paragraph 3 – “trialled” is jargon and should be avoided.
We agree that the word “trialled” is jargon and have removed it from the manuscript.

Table 2 – The results would be clearer if all numbers were rounded to one decimal place.
We agree that the odds ratios were slightly unclear to 3 decimal places. We have rounded the odds ratios to 2 decimal places, a usual level of precision for effects of this size.

Reviewer 2

RESPONSES TO REVIEW REPORT
This is an interesting and innovative study. However, I have a number of serious concerns with it in its present form. My main concern is that I think more of the analyses should have been conducted at the level of practice not patient. In particular, it is important to know if rates of misclassification vary by the proportion of patients with smoking status recorded.

We agree that our data on response rates, recording of smoking status, misclassification of smoking status, and the proportion wanting to speak to an adviser is more appropriately analysed at the practice level, and we have now changed all of these results accordingly.

Many of these outcomes were not symmetrically distributed, and are therefore presented in terms of medians and ranges across practices. The influence of individual characteristics, such as age, sex and socio-economic status, have been analysis at the individual level, allowing for clustering by practice, as before. These changes do not substantially alter the main message of our paper, but where the interpretation has changed, or additional insights have been attained, we have amended the discussion accordingly. Specifically, we have looked at the correlation between the proportion of patients with a smoking status recorded in each practice and the proportion of recorded smokers who were self-reported non smokers and reported the result accordingly: “There was no correlation between the proportion of patients with a smoking status recorded and the proportion of patients who were recorded as smokers but denied tobacco use in the previous 12 months (Spearmans r = -0.14)".
If would also be useful to provide the percentage of practices that achieved effectively complete and sufficiently accurate ascertainment.

We agree that it would be useful to give such figures, but although we can use our data to calculate the proportion of smokers who are inaccurately recorded, and the proportion of those with no-smoking status who are in fact smokers, we cannot ascertain the proportion of recorded non-smokers who are actually smokers, since we did not approach this group. We have therefore presented the inaccuracies in the only way we can given the data available.

Also is there any relationship between the percentage of smokers in a practice (as from records) and the percentage of uncertain cases who turn out to be smokers?

We can do this, but we are not sure what question this would answer. One would expect to find a correlation since both will be related to the prevalence of smoking in the practice, but we are uncertain how much more this will tell us.

If length of time quit is known, (I suspect it isn't) it should be used to review false positives.

We agree that this would be useful, but unfortunately we do not have the information to do this.

DETAILED COMMENTS

1. Abstract: The trial registration should be dropped from here as this study is not reporting on trial outcomes.

   This was an error on our part, the trial registration number has been removed.

2. Background, paragraph 1. Cant you just say that recording is often inaccurate?

   We have replaced the phrase “is inconsistent with other measures of smoking status” with the phrase “is often inaccurate”

3. Paragraph 3. The first sentence is confusing. The fact that at some point (we never get told when) patients/practices (we don’t know which) were part of a RCT is not particularly relevant.

   We agree that the first sentence was confusing, and we have amended it to read “In this study, primary care patients’ smoking status was ascertained and smokers offered evidence-based support to stop”.

4. Methods, last paragraph. The claim referenced to ref 14 is a gross overstatement. The authors might briefly describe what a Townsend score is for non-UK (especially) readers.

   We have slightly rephrased the claim referenced to ref 14 to read “this measure of deprivation has been found to explain variations in health measures and adhere closely to the concept of material disadvantage” as ref 14 does make this statement. Including a brief description of the Townsend score is a good idea, and we have added the following sentence to describe
what the Townsend score is based on: “Townsend scores are based on unemployment, car ownership, overcrowding and tenure”.

5. Results. The proportion of patients within a practice with recorded smoking status is skewed; this means they are not a particularly useful statistic. I would like to know what proportion recorded all or nearly all (say 95% or more).

This is a good point. We have provided the median value and range for recorded smoking status, and have included a cumulative frequency distribution enabling the reader to see the proportion of practices with at least 95% of patients with a smoking status recorded, and enabling then to read off the relevant figures for any chosen percentage.

6. The proportion of smokers among unrecorded cases should be related to the percentage of patients with recorded statues and to estimate of percentage of smokers. The variation across practices is huge. It is similarly so for identified smokers who said they weren’t.

We are unsure what the reviewer is asking in this point.

7. Under Table 1. Did interest in help vary as a function of whether they were newly identified as a smoker or confirmed as a smoker. I would expect the former to have more interest, as they have had no opportunity to be offered it by their GP in the past.

The reviewer raises an interesting point. We have included proportions for smokers originally recorded as such who wanted help compared to those who had no smoking status recorded: “Individuals who were previously recorded as smokers tended to be more likely to want to speak to a cessation adviser than those who previously had no smoking status recorded (42.7% and 33.4% respectively)”.

8. Also did interest in getting help vary by practice. To interpret Table 2 as showing those 41 – 50 were most interested is inappropriate. The age range should at least include the 31 – 40 age group, and I suspect also the 51 – 60 group (if they are sig. different to 61+ and not to 51+).

This is another interesting point. We have presented practice level data for proportions of smokers who wanted help to quit: “This varied between practices from 30.6% and 51.8% (median 39.8%)”. We agree that our previous interpretation of interest in receiving help by age was inappropriate, and have changed the interpretation to read “Those aged between 31 and 50 were most likely to want to speak to an advisor and the oldest and youngest age groups were least likely to desire this (33.4% and 34.1% respectively)”.

9. Discussion. Focus on the 20% of inaccuracies is inappropriate. Tell us the range and try to work out what factors determine how well practices are doing.

We have given the practice range for inaccuracies in recording: “the proportion of responding patients recorded as smokers who denied tobacco use in the previous 12 months varied
between 6.3% and 58.1% across practices (median 20.3%). Unfortunately, however, we do not have the relevant information to judge why some practices record more accurately than others and that is beyond the scope of this paper.

10. Surely the research team know what percentage of returns did not give permission for the research team to see the data. In this regard I find it curious that the team had access to the patient records to be able to ascertain whether smoking status was recorded but were unable to access this other information. Surely if the practices use this information to update their records, the research team could be given the updated figures.

The reviewer raises a valid point. Unfortunately we could not obtain ethical permission to access patients’ medical records, so practices searched these on our behalf and we also had no access to responses of patients who did not give consent. Not all practices involved chose to use the information from the questionnaires to update medical records and so we do not have ‘updated figures’ as suggested above.