PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Estimating the number of quit attempts it takes to quit smoking successfully in a longitudinal cohort of smokers
AUTHORS	Chaiton, Michael; Diemert, Lori; Cohen, Joanna; Bondy, Susan; SELBY, PETER; Philpneri, Anne; Schwartz, Robert

VERSION 1 - REVIEW

REVIEWER	Hyoung Suk Lee University of Washington Tacoma
REVIEW RETURNED	28-Jan-2016

| GENERAL COMMENTS | Review of the manuscript

Estimating the number of quit attempts it takes to quit smoking successfully

This article compared a few ways to estimate the average number of quit attempts that are needed to achieve the successful quitting. The authors concluded that existing estimates of the number of quit attempts have been underestimated. The following is some concerns about the manuscript.

Introduction
1. (second paragraph of page 5) Authors mentioned that actual data (about the number of quit attempts) are limited. However, I am not sure whether stats from a review article would support authors' argument since a review articles are based on a lot of empirical data.

Methods
2. I have a concern about the response rate and retention rate of the study. Attrition analysis must be done to make sure that there were no differences in psychological factors that can be related to quit attempts and cessation (e.g., motivation, self-efficacy, or demographic variables related to motivation and self-efficacy) between those who were successfully followed up and those who were dropped out from the study.

3. Authors should provide more information about measurements of some key variables. Smokers who made a serious quit attempt were included in the study. Then, how did authors define "the serious quit attempt"? Also, does this mean that those who reported "not serious quit attempts" were excluded from the data analyses? How did authors ask and decide the last cigarette that successful quitters reported? Was any biochemical verification for the successful quitting included in the study (in the reviewer's recent study, approximately 50% of the participants who self-reported successful |
|---|---|

quitting did not pass this biochemical verification)?

4. In the study, number of quit attempts was calculated by asking respondents who tried to quit smoking completely in the past 6 months. I wonder whether authors used any methods (e.g., timeline followback: TLFB) to make sure that the participants accurately self-reported the quit attempts for the last 6 months. It would not be easy to remember the accurate number of events that happened in the past 6 months. If authors did not use any compensatory methods to increase the accuracy of this self-report, how can authors verify that there was no memory bias in this procedure (or self-report)? Was this study really different from the previous studies that used the recalled quit attempts? I believe that the participants in this study were also asked to "recall" the number of quit attempts for the last 6 months.

5. Authors believe that, among methods used to estimate the number of quit attempts, method 3 is the best. However, I am not sure that the life table approach or survival analysis can be appropriately used to estimate the number of quit attempts required to quit. Considering the concept of the life table approach and previous studies that used the approach, the column of the table is a variable with time interval (e.g., years, months, ages). I did not find out any study that used the life table approach with the number of events (number of quit attempts in this study). It would be very helpful to understand method 3 if authors can show that the data in this study did not violate any of important assumptions for the life table approach, providing any references that used the life table approach to analyze the data with the number of events.

Results
6. First, a table or section that describes the participants' characteristics regarding demographic and smoking-related variables would be needed.

7. I am not sure that Table 1 is really needed. No significant results in Table 1 were considered and interpreted in results and discussion. Plus, the two categories of unsuccessful attempts to quit and successful quit attempts should be changed to refer to persons (e.g., smokers who made unsuccessful attempts to quit). Last, if the authors wanted to show the factors that distinguish between those who made unsuccessful attempts to quit and those who made successful attempts to quit, I believe logistic regression is the better way to analyze data.

Discussion
8. My biggest concern about this manuscript is the implication of the study. Although there are several issues to be addressed to verify the validity and reliability of assessing and estimating quit attempts in this study, let's say that the life table approach gives the best estimate of quit attempts needed to quit, approximately 30. What do authors want to say with this number? Should smokers and smoking researchers be pessimistic in quitting smoking? Thus, the last sentence of discussion, "many smokers will die, likely prematurely, without ever having quit successfully," is very annoying as a smoking researcher. Instead, authors should've interpret the results in more developmental ways and could've concluded the study with some suggestions to increase the successful quit rate and decrease the number of quit attempts needed to quit.

REVIEWER	Emily Stockings National Drug and Alcohol Research Centre (NDARC), UNSW Australia.
REVIEW RETURNED	05-Feb-2016

GENERAL COMMENTS	Overall this is a useful analysis of the number of quit attempts required to quit successfully among a longitudinal cohort of smokers. The methods used in this paper improve upon previous estimates in cross-sectional studies based on recall. I feel some improvement in the reporting of the study could be made, particularly in the methods section, where some key details seem to be missing. Further, I feel a bit more context around the sample should accompany the estimated number of quit attempts required, given what is known about differences in smoking rates, quit attempts and successful quitting on the basis of demographic factors and smoking severity. See below for detailed suggestions. Abstract 1. I feel there needs to be a stronger rationale for why this study is being conducted in the objectives section of the abstract, even if just half a sentence, e.g. "previous estimates have been based on lifetime recall in cross-sectional samples, and may be unreliable…" 2. Are there some words missing in the design section, line 26 "…we calculated the estimate of number of quit attempt prior to quitting successful for at least one [year?] under 4 different sets of assumptions.". Introduction 3. The introduction might start in a more informative manner if the authors include statistics on smoking rates, quit rates and the remaining burden attributable to tobacco-related disease before emphasising the difficulty in quitting smoking. This will help place the research within the wider public health context of addressing smoking. 4. Page 6, line 6 the authors refer to a longitudinal study of quit attempts by Borland et al 2012. Given this seems to be the most similar study design to the present study, it might be worth noting what they found, and any limitations to their approach which the authors feel they have improved on/added to in this paper. Methods 5. Page 7, line 3, the authors state "….who self reported a serious quit attempt at follow-up 1.." what is meant by 'serious' quit attempt here? Was a timeframe applied, e.g. quit smoking for at least 24 hours with the intention to stop? 6. Page 7 line 3, it would be useful to put the timing of each follow-up in brackets, e.g "…follow-up 1 (xx months/years), follow-up 2 (xx months/years" as it becomes confusing when the authors go on to say that "Follow-up continued for an additional 1.5 years (3 interviews)". What follow-up points do these 3 extra interviews correspond to? Follow-ups 4, 5, and 6? I suggest rephrasing so that the timing of follow-ups is presented in a consistent format. 7. Page 7, in the measures section, line 39, the opening sentence is a bit confusing "Number of quit attempts was calculated by asking respondents who tried to quit smoking completely in the past 6 months." Does this measure give you the number of quit attempts made by each respondent in the past 6 months? How were quit attempts defined? This seems at odds to the following sentence stating that "A successful quit was defined as a quit that lasted for at

least 1 year…." How can someone report being quit for at least a year when being assessed in the last 6 months? This might just need some wording clarification as it is difficult to follow the logic.
8. Page 7, Statistical analysis section -I suggest starting with a brief overview of the lifetable method used before detailing the 4 assumptions, unless there is a previous paper that used this approach that you are able to cite here.
9. Throughout the data analysis section, the authors refer to the Hughes et al 2004 study, however no background is given about this study and why It is being used to guide the analyses here. I suggest in the opening section of the data analysis section to outline the main analytic approach and any previous studies upon which this approach is based. At the moment the analysis section assumes too much prior knowledge of the reader which is unlikely to be existent except among researchers with very closely aligned interests (or familiarity with the study itself). E.g. "To examine the number of quit attempts required to reach successfully abstinence, we used approach X, which calculates X, Y, Z. We drew on previous studies (cite) for parameters pertaining to success rates..", etc etc. It seems odd that a study would use data from another paper when the present study has that data available?
10. Overall I feel the authors need to provide greater detail of the measures and variables used. There are many outcomes in the tables that have no mention in the methods, e.g. how were "Health Status", "Perceived addition", and "Smoking status" defined? If this is clearly outlined in a previous paper, state this in the methods for the reader's reference.

Results
11. While the main results of this cohort may be published elsewhere, a very short section on demographics would help with interpretation given the known differences in smoking rates and quit success on the basis of demographic factors, e.g. SES, employment status, mental health diagnosis, etc.
12. Similarly, on page 13 the authors make the first reference to level of smoking within the sample (line 35, "daily vs. non daily smoking"), which should also be reported at the start of the results so the readers can get an understanding of the severity of smoking within the sample. Are there any other such indicators that might be useful to report, e.g. daily cigarette consumption, FTND, HSI?

Discussion
13. The authors state that model 3 was the best set of assumptions – why is this?
14. Page 16 line 55 "This study defined quit attempt as a "serous quit attempt" as self perceived by the smoker." This should be stated in the methods, as well as here.
15. Some comment on the clinical utility of this information would add a useful applied component to this paper.

REVIEWER	Timea Partos King's College London United Kingdom
REVIEW RETURNED	05-Feb-2016

GENERAL COMMENTS	SUMMARY
	This research compares four different methods for estimating the

average number of quit attempts smokers make before successfully quitting. These methods are 1) "Constant rate assumption", 2) "Recalled quit attempts among successful quitters", 3) "Variable rate assumption, observed quit success rate", and 4) "Variable success rate assumption with recalled life time quit numbers". It is argued that variable rate assumption using a life table approach (Method 3) provides the most unbiased estimate of the number of quit attempts. Findings indicate that the estimated average number of lifetime quit attempts made by smokers before quitting is much higher than what was previously thought (3-14 attempts vs. 30 or more). This finding has important implications for tailoring quitting assistance and framing the information and advice that is provided to smokers. It is a strength of this study that successful quitting is defined as being quit for at least one year, rather than shorter periods. The comparison of different methodologies is interesting and informative, and the manuscript is generally well written. I think the manuscript would benefit from a clearer description of methodologies and a more detailed outlining of the rationale behind some analytical decisions. Some additional analyses are also suggested, in particular to control for differences between smokers who did and did not use assistance on their quit attempts. Further details are provided below.

ABSTRACT

Requires clarification on some points:

p2, l22: "primarily used" - why "primarily"? did you use other data too? (this is not made clear in the methods section either).

p2, l24: "In 2014" - clarify: did the study end in 2014 (i.e. new samples recruited every 3 years) or did it end in 2008 and data was analysed in 2014? Again this is not made clear in the methods section. If the study ended in 2008, I don't think it is necessary to mention the year of data analysis, as it just creates confusion.

p2, l26: A brief description (or at least the names of) the 4 different sets of assumptions would be useful, as well as the mean number of quit attempts estimated by each.

INTRODUCTION

The analysis contrasts four methods of estimating lifetime quit attempts, yet the introduction does not introduce each method clearly. The authors seem to have decided a-priori that the life table method would provide the most accurate estimates, yet they do not present adequate argument/ evidence for this. I think the intro could be better structured around specifically discussing the pros and cons of the four estimating methods, perhaps with sub-headings and examples (refs) for each. There are sections of the introduction that are vague and not very informative which could be cut to make room for such a discussion e.g.

p5, first paragraph: somewhat repetitive - "keep trying or stay engaged" already mentioned - the final sentence should focus on the alternative, and simply read: "Additionally, knowing the average

number of quit attempts before succeeding could act as a deterrent to trying".

p5, l34: "However, actual data are limited:..." I would cut this sentence, it doesn't seem relevant, especially given that the authors cite a number of academic references in the following paragraph which provide data (albeit potentially underestimating figures) on this topic.

p6, l22: "To do so... ... absence of such a study" Unnecessary, could cut this part. Also the next sentence is quite vague and would benefit from some references.

METHODS

Participants: Despite the fact that sampling and recruitment procedures are documented elsewhere, I would expect more detail here regarding the break-down of participant numbers, and basic characteristics (at least quit vs smoking) at each follow-up interval. It should also be made clear whether the n = 1277 participants comprised a single cohort recruited in 2005, or multiple cohorts recruited and followed-up at various points between 2005 - 2014.

Ethics: It seems unusual to include so much detail in the manuscript itself - usually this is covered in some sort of declaration during the submission process, or in supplementary material. Is this a journal requirement? If not I would cut this section altogether.

Measures:
p7, l39: "... calculated by asking respondents who..." - clarify: asking respondents what?
Clarify: were participants who quit for 1 year followed further to ensure they remained quit. or was it possible to relapse (e.g. quit at follow-up 1, verified at follow-up 4, then smoking again at follow-up 5: were these participants considered successful or not?)

Statistical analysis:
In general, this section needs to be clearer, with the steps taken to derive each of the different estimates (and exactly how they differ from each other) made explicit.

Method 1. Not sure why the 3% success rate from Hughes et al., (2004) was used instead of calculating the rates based on the present sample. The Hughes study focused specifically on self-quitters (i.e. no assistance used), whereas it is not clear that the present study does so, and therefore it is potentially dealing with a qualitatively different sample.
Also, requires some justification of the assumption that success rates are likely to decrease with multiple attempts (perhaps something for the introduction).

Method 3. and 4: The level of detail here is great, however I'm not sure why Method 4 censors the data from successful smokers whereas Method 3 does not appear to do this? Was this unavoidable or a decision made by the researchers? It seems that if Method 4 was identical to Method 3 except with the inclusion of recalled lifetime events, it might be the optimal estimation method... (my apologies - perhaps I am misunderstanding exactly what was done

here).

RESULTS

Would it be possible to include a comparison table of the success rates obtained by each method?

Some more data/ explanation relating to Method 1 would be good - I'm not sure where these number(p10, l4) s are coming from.

It is not clear that the authors have adopted a consistent methodology for assessing the 4 different estimation methods, e.g.: The results presented from Method 2 highlight the fact that significant differences in quit success are related to factors such as education, addiction, and most importantly the use of pharmaceutical and behavioural aids. However these variables do not appear to have been controlled for when calculating estimates using methods 3 and 4. The authors appear to have controlled for gender, age and daily vs non-daily smoking, yet none of these were predictive of success in Table 1. What was the reasoning behind this? It seems that the data is available for the authors to conduct these analyses controlling for assistance used, perceived addiction, etc... and I think the manuscript would be strengthened by these further analyses.

DISCUSSION

I think the discussion would read better if the various methods were referred to by their names/ description rather than "method 1" "method 2" etc...

In discussing the relationship between more quit attempts and less likelihood of success (p18), the authors may wish to consider the possibility that the frequency of attempts may be playing a role - i.e. numerous attempts made in quick succession could lead to a sort of quit attempt burn-out that would reduce success rates. The authors could potentially test for this by deriving a "quit rate per year" or something similar using participants' age and lifetime attempts or observed attempts per year or something similar (this is just a suggestion probably for future research, not necessarily for the present study).

p15, paragraphs 2-3: consider shifting some of this literature review to the introduction to justify the decision that method 3 would provide the best estimates.

p16, l10: "Method 2 also confirms..." Not sure that this conclusion can be stated so strongly given that the authors argue that Method 2 is not optimal and based on flawed recall.

p16, l13: again, I think some of this could be shifted to the intro, and the authors could include some extra discussion about the implications of their findings for smokers/ quit-smoking practitioners.

p16, l33: "Method 4 further suggests the limitations of the assumptions in Method 3" - not sure what is meant by this.

STROBE STATEMENT

Not sure that there is adequate information presented about treatment of missing data/ loss to follow-up / sensitivity analyses Participant numbers completing each stage of follow-up not described

MINOR/ GRAMMATICAL

consistency of terms, sometimes authors use "lifetable" and sometimes "life table" - stick to one
p2, l26: sentence is confusing, suggest replacing "estimate of number" with "estimated number of", and delete "at least one"

p6, l8: should be "only account"

p6, l36: should be "estimates" not "estimating"

p8, l23: "This assumption corresponds to the assumption...." ? This method corresponds to the assumption?

Methods section: Consider including "life table" somewhere in the title of Methods 3 and Methods 4, as this is the terminology that is used throughout the rest of the manuscript.

p9: l48: "analysis analysis"

p15, l56: "that that"

p16, l8: "attributed to the primarily to the"

Figures: The estimation methods used to derive the figures should be made explicit in the figure titles.

REVIEWER	Anton Kunst Department of Public Health AMC University of Amsterdam
REVIEW RETURNED	05-Mar-2016

GENERAL COMMENTS	This introduction of this paper is excellent. The use of life table approach is promising. Yet, I have major concerns about the application of this approach. 1. The authors may need to define more clearly the population of interest: does their objective refer to all smokers (including those who make quit attempts but have not succeeded at the moment of dying) or only to those who did succeed quitting before dying? The title suggests the latter, but a remark made at the end of page 25 suggests the former. 2. The description of the methods is unclear, and does not yet permit a detailed evaluation of their validity. As all four types estimates imply some form of modelling, the authors should carefully

describing the modelling procedure, which includes a precise description of the mathematics, to be followed by detailing (a) the assumptions and (b) the empirical input to the models.

3. The life table approach is highly recommended, but should be explained more clearly along the lines I outlined above. In doing this, the authors could have stuck more closely to terminology used in life table literature. Essentially, the application of life table methodology would imply modelling how the size of a hypothetical cohort of smokers were decrease in relationship to "quit age" (which is 0 for those with 0 past quit attempts, 1 for 1 attempts, etc). The authors could have chosen for a multi-decrement table (exit by successful quitting or by dying) but they seem to opt for single-decrement table (exit by quitting only). In the latter case, the only empirical input needed is a series of chances of exit (i.e. by successful quitting) in relationship to "quit age".

4. It is unclear how the authors have estimated the latter empirical input. The authors have described the data sources and variables, but they have not made clear how they derived from this estimates of quit rates in relationship to "quit age". Without a detailed description of this estimation procedure, it is not possible to assess the validity of the core results of this paper.

5. I think that the life table estimates may be strongly influenced by how they treat the following issues:
- how did the authors take into account the quit rates of those who had already quit after a first few attempts (say, at ages 20-25 years) and who therefore are not included in their survey. Ref 19 and 20 document high quit rates for low 'quit ages'.
- how did the authors take into account the experience of smokers who had died (as smokers not being able to quit) by the time of the survey.
- how did the authors accurately estimate the "quit age" of the respondents to the survey. The authors themselves stress that self-reports on past quit attempts can strongly underestimate the true number of attempts. If so, their data strongly underestimate the 'quit age' of respondents.
6. Extensive information of co-variates of quitting, as given in table 2, are not relevant to main objective of this paper. They distract from issues like identified above.

7. The authors present a well-known function of the life table: the absolute number of 'survivors' per 'quit age'. For their purposes, however, it would be more informative to present another function: the absolute number of 'exits' (i.e. number of successful quitters) per 'quit age'. This directly corresponds to the title of the paper. The authors may probably conclude that this "exit distribution" cannot be simply summarised in terms of one average.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Reviewer Name
Hyoung Suk Lee

Institution and Country
University of Washington Tacoma

Please state any competing interests or state 'None declared':
None declared

Please leave your comments for the authors below Review of the manuscript

Estimating the number of quit attempts it takes to quit smoking successfully

This article compared a few ways to estimate the average number of quit attempts that are needed to achieve the successful quitting. The authors concluded that existing estimates of the number of quit attempts have been underestimated. The following is some concerns about the manuscript.

Introduction
1. (second paragraph of page 5) Authors mentioned that actual data (about the number of quit attempts) are limited. However, I am not sure whether stats from a review article would support authors' argument since a review articles are based on a lot of empirical data.

It is true that many studies have asked successful smokers about their previous quit attempts. We have removed this statement to avoid making a larger claim than we intended.

Methods
2. I have a concern about the response rate and retention rate of the study. Attrition analysis must be done to make sure that there were no differences in psychological factors that can be related to quit attempts and cessation (e.g., motivation, self-efficacy, or demographic variables related to motivation and self-efficacy) between those who were successfully followed up and those who were dropped out from the study.

We have now included more data of characteristics with respect to attrition (see page 8). As we are modelling the chances of success on a quit attempt, only smokers who have made a quit attempt are eligible to be included in the analysis. While there were some demographic differences between those who dropped out of the cohort before they were eligible for this study, there were no significant differences on smoking related characteristics. There was minimal attrition after study eligibility.

3. Authors should provide more information about measurements of some key variables. Smokers who made a serious quit attempt were included in the study. Then, how did authors define "the serious quit attempt"? Also, does this mean that those who reported "not serious quit attempts" were excluded from the data analyses? How did authors ask and decide the last cigarette that successful quitters reported? Was any biochemical verification for the successful quitting included in the study (in the reviewer's recent study, approximately 50% of the participants who self-reported successful quitting did not pass this biochemical verification)?

We have added more detail on the quit attempt definition and last cigarette questions on p. 10. We did not ask about not serious quit attempts. Biochemical verification was not used. Self-reported cessation in surveys has been shown to be a valid and reliable estimate of quitting in situations where there is little incentive to report inaccurately, unlike clinical trials. The Society for Research on Nicotine and Tobacco does not recommend biochemical verification in this type of study. Nevertheless, the lack of biochemical verification has been included as a limitation on page 19.

4. In the study, number of quit attempts was calculated by asking respondents who tried to quit smoking completely in the past 6 months. I wonder whether authors used any methods (e.g., timeline followback: TLFB) to make sure that the participants accurately self-reported the quit attempts for the last 6 months. It would not be easy to remember the accurate number of events that happened in the

past 6 months. If authors did not use any compensatory methods to increase the accuracy of this self-report, how can authors verify that there was no memory bias in this procedure (or self-report)? Was this study really different from the previous studies that used the recalled quit attempts? I believe that the participants in this study were also asked to "recall" the number of quit attempts for the last 6 months.

A timeline follow up would be a further improvement to the methods here. This study is an improvement over most previous research which expected recall over a lifetime. There is a substantial difference between the ability to recall over 6 month period of time and over the course of many years. However, it is certainly possible that quit attempts would have been forgotten, which means the estimates of number of quit attempts presented would be too low. We now discuss this further in the limitations on page 19.

5. Authors believe that, among methods used to estimate the number of quit attempts, method 3 is the best. However, I am not sure that the life table approach or survival analysis can be appropriately used to estimate the number of quit attempts required to quit. Considering the concept of the life table approach and previous studies that used the approach, the column of the table is a variable with time interval (e.g., years, months, ages). I did not find out any study that used the life table approach with the number of events (number of quit attempts in this study). It would be very helpful to understand method 3 if authors can show that the data in this study did not violate any of important assumptions for the life table approach, providing any references that used the life table approach to analyze the data with the number of events.

The analysis is indeed a novel unique application of a life table type design. In the life table analysis, quit attempts are not events but measurement of time, and successful smoking cessation is the event. Counting "time" in this type of way is more common in engineering when using time to event in examining mechanical failure rates. For instance, it is possible to examine machine failure rates per use, rather than per year, per day, or etc. Thanks to comments from reviewer 4 we have standardized the terminology and the description of the methodology to help clarify the assumptions here. The method is outline and referenced now on page 7.

Results
6. First, a table or section that describes the participants' characteristics regarding demographic and smoking-related variables would be needed.
7. I am not sure that Table 1 is really needed. No significant results in Table 1 were considered and interpreted in results and discussion. Plus, the two categories of unsuccessful attempts to quit and successful quit attempts should be changed to refer to persons (e.g., smokers who made unsuccessful attempts to quit). Last, if the authors wanted to show the factors that distinguish between those who made unsuccessful attempts to quit and those who made successful attempts to quit, I believe logistic regression is the better way to analyze data.

As per suggestion 6 and 7, we have replaced table 1 with the characteristics of the analytic sample. Characteristics of the larger population (representative of the Ontario population) from the full study are referred to and cited on page 8.

Discussion
8. My biggest concern about this manuscript is the implication of the study. Although there are several issues to be addressed to verify the validity and reliability of assessing and estimating quit attempts in this study, let's say that the life table approach gives the best estimate of quit attempts needed to quit, approximately 30. What do authors want to say with this number? Should smokers and smoking researchers be pessimistic in quitting smoking? Thus, the last sentence of discussion, "many smokers

will die, likely prematurely, without ever having quit successfully," is very annoying as a smoking researcher. Instead, authors should've interpret the results in more developmental ways and could've concluded the study with some suggestions to increase the successful quit rate and decrease the number of quit attempts needed to quit.

It is a descriptive estimate of a number that is current widely promoted. We discuss further the implications of an increased estimate in the number of attempts to quit. In short, we make three main point: 1)Many people, researchers, and clinicians are underestimating the difficulty in quitting smoking; 2)Increasing the rate of success per quit attempt can help smokers quit with fewer attempts; 3) Further research is needed to ascertain whether providing a more realistic number help smoking continue to try to quit or is it discouraging?

We have also removed the line about "many smokers will die.."

Reviewer: 2
Reviewer Name
Emily Stockings

Institution and Country
National Drug and Alcohol Research Centre (NDARC), UNSW Australia.

Please state any competing interests or state 'None declared':
None declared

Please leave your comments for the authors below Overall this is a useful analysis of the number of quit attempts required to quit successfully among a longitudinal cohort of smokers. The methods used in this paper improve upon previous estimates in cross-sectional studies based on recall. I feel some improvement in the reporting of the study could be made, particularly in the methods section, where some key details seem to be missing. Further, I feel a bit more context around the sample should accompany the estimated number of quit attempts required, given what is known about differences in smoking rates, quit attempts and successful quitting on the basis of demographic factors and smoking severity. See below for detailed suggestions.

Abstract
1. I feel there needs to be a stronger rationale for why this study is being conducted in the objectives section of the abstract, even if just half a sentence, e.g. "previous estimates have been based on lifetime recall in cross-sectional samples, and may be unreliable…"

We have added this to the abstract.

2. Are there some words missing in the design section, line 26 "…we calculated the estimate of number of quit attempt prior to quitting successful for at least one [year?] under 4 different sets of assumptions.".

We have corrected this line.

Introduction
2. The introduction might start in a more informative manner if the authors include statistics on smoking rates, quit rates and the remaining burden attributable to tobacco-related disease before emphasising the difficulty in quitting smoking. This will help place the research within the wider public health context of addressing smoking.
3.

We have added some context of the burden of tobacco to the beginning of the introduction

4. Page 6, line 6 the authors refer to a longitudinal study of quit attempts by Borland et al 2012. Given this seems to be the most similar study design to the present study, it might be worth noting what they found, and any limitations to their approach which the authors feel they have improved on/added to in this paper.

We have added more detail on the Borland paper in the introduction on page 6. The Borland paper examines the rate of quitting which is related to but not the same as directly addressing the question of number of attempts on average before quitting but does provide an annual rate of quit attempts, and provide an estimate for the average number of previous quit attempt for smoker at age 40.

Methods
5. Page 7, line 3, the authors state "....who self reported a serious quit attempt at follow-up 1.." what is meant by 'serious' quit attempt here? Was a timeframe applied, e.g. quit smoking for at least 24 hours with the intention to stop?

The definitions of quit attempts have been clarified and a full description of the questions used to calculate this have been included on page 9. A serious quit attempt was a self-reported serious quit attempt to stop for good.

6. Page 7 line 3, it would be useful to put the timing of each follow-up in brackets, e.g "…follow-up 1 (xx months/years), follow-up 2 (xx months/years" as it becomes confusing when the authors go on to say that "Follow-up continued for an additional 1.5 years (3 interviews)". What follow-up points do these 3 extra interviews correspond to? Follow-ups 4, 5, and 6? I suggest rephrasing so that the timing of follow-ups is presented in a consistent format.

These are indeed follow ups 4, 5, and 6. We have deleted the line to reduce confusions and instead have given the total number of follow ups earlier in the paragraph and increased the description of the measurement of the outcome on page 9.

7. Page 7, in the measures section, line 39, the opening sentence is a bit confusing "Number of quit attempts was calculated by asking respondents who tried to quit smoking completely in the past 6 months." Does this measure give you the number of quit attempts made by each respondent in the past 6 months? How were quit attempts defined? This seems at odds to the following sentence stating that "A successful quit was defined as a quit that lasted for at least 1 year…." How can someone report being quit for at least a year when being assessed in the last 6 months? This might just need some wording clarification as it is difficult to follow the logic.

Quit attempts were based on recalled reports from the past six month. Quit success was determined at subsequent follow ups. This description has been clarified in the text.

8. Page 7, Statistical analysis section -I suggest starting with a brief overview of the lifetable method used before detailing the 4 assumptions, unless there is a previous paper that used this approach that you are able to cite here.

Only methods 3 and 4 use life table analysis. We have now described the general analysis in a titled section to highlight the different approaches. We have also included more information in the introduction that discusses each of the approach, and have created a table with the assumptions for each of the approaches.

9. Throughout the data analysis section, the authors refer to the Hughes et al 2004 study, however no background is given about this study and why It is being used to guide the analyses here. I suggest in the opening section of the data analysis section to outline the main analytic approach and any previous studies upon which this approach is based. At the moment the analysis section assumes too much prior knowledge of the reader which is unlikely to be existent except among researchers with very closely aligned interests (or familiarity with the study itself). E.g. "To examine the number of quit attempts required to reach successfully abstinence, we used approach X, which calculates X, Y, Z. We drew on previous studies (cite) for parameters pertaining to success rates..", etc etc. It seems odd that a study would use data from another paper when the present study has that data available?

For method 1 (now listed as method 2: constant rate assumption), we did not want to rely on a single data set (ours) when an estimate from a meta analysis (ie., Hughes) was available. However, the suggestion to use the estimate from the study itself however is a good one. We have used the estimate from the cohort study so as to be more consistent with the rest of the methods. We have also re-organized the order of the methods to make a more logical transition from one method to another.

10. Overall I feel the authors need to provide greater detail of the measures and variables used. There are many outcomes in the tables that have no mention in the methods, e.g. how were "Health Status", "Perceived addition", and "Smoking status" defined? If this is clearly outlined in a previous paper, state this in the methods for the reader's reference.

We have replace table 1 with characteristics of the sample as per the request of the reviewers. We have also clarified where these variables have been previously used and cited on page 8 as most of the described variables in this table are item responses.

Results
11. While the main results of this cohort may be published elsewhere, a very short section on demographics would help with interpretation given the known differences in smoking rates and quit success on the basis of demographic factors, e.g. SES, employment status, mental health diagnosis, etc.

12. Similarly, on page 13 the authors make the first reference to level of smoking within the sample (line 35, "daily vs. non daily smoking"), which should also be reported at the start of the results so the readers can get an understanding of the severity of smoking within the sample. Are there any other such indicators that might be useful to report, e.g. daily cigarette consumption, FTND, HSI?

Sample characteristics including smoking behaviour are now included in table 1. This is a representative sample of the Ontario population.

Discussion
13. The authors state that model 3 was the best set of assumptions – why is this?

The preferred model is now further discussed in the introduction and in the discussion on page 17. The authors feel that this method has the fewest biases.

14. Page 16 line 55 "This study defined quit attempt as a "serous quit attempt" as self perceived by the smoker." This should be stated in the methods, as well as here.

This is now included.

15. Some comment on the clinical utility of this information would add a useful applied component to this paper.

More discussion on the implications of the results has been included on page 21.

Reviewer: 3
Reviewer Name
Timea Partos

Institution and Country
King's College London
United Kingdom

Please state any competing interests or state 'None declared':
None declared

Please leave your comments for the authors below SUMMARY

This research compares four different methods for estimating the average number of quit attempts smokers make before successfully quitting. These methods are 1) "Constant rate assumption", 2) "Recalled quit attempts among successful quitters", 3) "Variable rate assumption, observed quit success rate", and 4) "Variable success rate assumption with recalled life time quit numbers". It is argued that variable rate assumption using a life table approach (Method 3) provides the most unbiased estimate of the number of quit attempts. Findings indicate that the estimated average number of lifetime quit attempts made by smokers before quitting is much higher than what was previously thought (3-14 attempts vs. 30 or more). This finding has important implications for tailoring quitting assistance and framing the information and advice that is provided to smokers. It is a strength of this study that successful quitting is defined as being quit for at least one year, rather than shorter periods. The comparison of different methodologies is interesting and informative, and the manuscript is generally well written. I think the manuscript would benefit from a clearer description of methodologies and a more detailed outlining of the rationale behind some analytical decisions. Some additional analyses are also suggested, in particular to control for differences between smokers who did and did not use assistance on their quit attempts. Further details are provided below.

ABSTRACT

Requires clarification on some points:

p2, l22: "primarily used" - why "primarily"? did you use other data too? (this is not made clear in the methods section either).

We used a value from a meta analysis to calculate the estimate for Method 1. We now use the estimate from our data as per reviewer suggestions. We have removed the term primarily.

p2, l24: "In 2014" - clarify: did the study end in 2014 (i.e. new samples recruited every 3 years) or did it end in 2008 and data was analysed in 2014? Again this is not made clear in the methods section. If the study ended in 2008, I don't think it is necessary to mention the year of data analysis, as it just creates confusion.

We have removed the date of data analysis for sake of clarity.

p2, l26: A brief description (or at least the names of) the 4 different sets of assumptions would be useful, as well as the mean number of quit attempts estimated by each.

We have created this table with name, estimates, and expected biases.

INTRODUCTION

The analysis contrasts four methods of estimating lifetime quit attempts, yet the introduction does not introduce each method clearly. The authors seem to have decided a-priori that the life table method would provide the most accurate estimates, yet they do not present adequate argument/ evidence for this. I think the intro could be better structured around specifically discussing the pros and cons of the four estimating methods, perhaps with sub-headings and examples (refs) for each. There are sections of the introduction that are vague and not very informative which could be cut to make room for such a discussion e.g.

p5, first paragraph: somewhat repetitive - "keep trying or stay engaged" already mentioned - the final sentence should focus on the alternative, and simply read: "Additionally, knowing the average number of quit attempts before succeeding could act as a deterrent to trying".

p5, l34: "However, actual data are limited:..." I would cut this sentence, it doesn't seem relevant, especially given that the authors cite a number of academic references in the following paragraph which provide data (albeit potentially underestimating figures) on this topic.

p6, l22: "To do so... ... absence of such a study" Unnecessary, could cut this part. Also the next sentence is quite vague and would benefit from some references.

Thank you for the comments. We have made the suggested changes in the text to the introduction.

METHODS

Participants: Despite the fact that sampling and recruitment procedures are documented elsewhere, I would expect more detail here regarding the break-down of participant numbers, and basic characteristics (at least quit vs smoking) at each follow-up interval. It should also be made clear whether the n = 1277 participants comprised a single cohort recruited in 2005, or multiple cohorts recruited and followed-up at various points between 2005 - 2014.

More detail on the sample is provided. The 1277 is a subsample of a larger cohort, recruited in waves between 2005 and 2008, and followed for up to three years from the initial interview.

Ethics: It seems unusual to include so much detail in the manuscript itself - usually this is covered in some sort of declaration during the submission process, or in supplementary material. Is this a journal requirement? If not I would cut this section altogether.

We have left a sentence in to describe the ethics and have moved the data sharing information out of the main text.

Measures:
p7, l39: "... calculated by asking respondents who..." - clarify: asking respondents what?
Clarify: were participants who quit for 1 year followed further to ensure they remained quit. or was it

possible to relapse (e.g. quit at follow-up 1, verified at follow-up 4, then smoking again at follow-up 5: were these participants considered successful or not?)

Participants were followed irrespective of their smoking status, and it was possible for a person to relapse at follow up 5 to be considered "successful". However, this limitation was necessary to avoid censorship by design to allow everyone the opportunity to make a successful attempt. This limitation is now mentioned in the discussion as it is another factor that suggests we have underestimated the effect.

Statistical analysis:
In general, this section needs to be clearer, with the steps taken to derive each of the different estimates (and exactly how they differ from each other) made explicit.

We have added a brief introduction to the analysis section, created a table, and tried to clarify the description and formulas used.

Method 1. Not sure why the 3% success rate from Hughes et al., (2004) was used instead of calculating the rates based on the present sample. The Hughes study focused specifically on self-quitters (i.e. no assistance used), whereas it is not clear that the present study does so, and therefore it is potentially dealing with a qualitatively different sample.
Also, requires some justification of the assumption that success rates are likely to decrease with multiple attempts (perhaps something for the introduction).

It is a good suggestion to use the estimate from our data rather than from the meta analysis. We no longer use the estimate from Hughes. We have explained the assumptions of each of the methods further.

Method 3. and 4: The level of detail here is great, however I'm not sure why Method 4 censors the data from successful smokers whereas Method 3 does not appear to do this? Was this unavoidable or a decision made by the researchers? It seems that if Method 4 was identical to Method 3 except with the inclusion of recalled lifetime events, it might be the optimal estimation method... (my apologies - perhaps I am misunderstanding exactly what was done here).

Method 3 and 4 are the same except for the inclusion of lifetime recall. In method 3, only observed data is included. In method 4 recalled data is included, this recall includes historical time periods in which there were other people who were smokers, but have now quit smoking. In some ways, method 4 is preferable; however, it only, by necessity, includes the data from people who were quitting at that time, and not the data from people who have already quit in the past and are no longer smokers (ie., censored). This is similar in some ways to the issues of using prevalence as an estimate of incidence. We have further discussed the differences between methods 3 and 4 in the introduction and the discussion.

RESULTS

Would it be possible to include a comparison table of the success rates obtained by each method?

A table of the mean number of attempts before success with their assumptions is now included.

Some more data/ explanation relating to Method 1 would be good - I'm not sure where these number(p10, l4) s are coming from.

These numbers come from a meta –analysis, but we are now including data directly from this study.

It is not clear that the authors have adopted a consistent methodology for assessing the 4 different estimation methods, e.g.:
The results presented from Method 2 highlight the fact that significant differences in quit success are related to factors such as education, addiction, and most importantly the use of pharmaceutical and behavioural aids. However these variables do not appear to have been controlled for when calculating estimates using methods 3 and 4. The authors appear to have controlled for gender, age and daily vs non-daily smoking, yet none of these were predictive of success in Table 1. What was the reasoning behind this? It seems that the data is available for the authors to conduct these analyses controlling for assistance used, perceived addiction, etc... and I think the manuscript would be strengthened by these further analyses.

We have standardized the reporting for each method. Reviewer 4 noted that the presentation of this information of the predictors of quit success was distracting. Table 1 was giving the impression that we were controlling for these factors and so was removed and replaced by a table that is more clearly the characteristics of the sample.

Generally, the estimate is meant to reflect the average of a population, rather than any one individual experience or the multiplicity of factors that affect chances of success. This is a descriptive statistic similar to other descriptive statistics like prevalence. While we could potentially standardize these estimates to some theoretical population, but it would make more sense to present the real results as the population of Ontario, Canada is likely not greatly different on important characteristics compared to other Western countries in a later stage of the smoking epidemic. However, we continue to present estimates by occasional vs. daily smokers as smoking status appears to be the strongest effect modifier compared to age,sex, quit aids, etc.

DISCUSSION

I think the discussion would read better if the various methods were referred to by their names/ description rather than "method 1" "method 2" etc...

We use the names more where possible, given the descriptions are long we continue to use both.

In discussing the relationship between more quit attempts and less likelihood of success (p18), the authors may wish to consider the possibility that the frequency of attempts may be playing a role - i.e. numerous attempts made in quick succession could lead to a sort of quit attempt burn-out that would reduce success rates. The authors could potentially test for this by deriving a "quit rate per year" or something similar using participants' age and lifetime attempts or observed attempts per year or something similar (this is just a suggestion probably for future research, not necessarily for the present study).

This is an important point has been added to the limitations. Method 4 adjusts for this in some respect using lifetime quit attempts, but is an important consideration and one that is difficult to account for. It's not clear which direction this effect goes. As your excellent paper in NTR demonstrates having more failed quit attempts is predictive of future poor outcomes. A Bayesian might say that knowledge about previous failed attempts should update our priors about the likelihood of success on the next attempt. Other preliminary research we are doing suggests that success on a subsequent quit attempt can vary by a number of factors. Ie., some people do better on subsequent attempt while some do worse. Figure 1 using recalled data suggests that people get "better" at quitting, before they get

worse.

We can't unfortunately use quit rates per year to calculate the number of quit attempt prior to a quit. We could use the quit rate per year (1.1 in this sample, similar to Borland et al., 2012) multiplied by the estimate provided by the constant quit rate assumption (19.6) to say that the average number of years smoked is 21.5. This is certainly an issue worthy of future investigation along with other modelling strategies.

p15, paragraphs 2-3: consider shifting some of this literature review to the introduction to justify the decision that method 3 would provide the best estimates.

We have shifted some of this to the introduction.

p16, l10: "Method 2 also confirms..." Not sure that this conclusion can be stated so strongly given that the authors argue that Method 2 is not optimal and based on flawed recall.

The conclusion here has been tempered.

p16, l13: again, I think some of this could be shifted to the intro, and the authors could include some extra discussion about the implications of their findings for smokers/ quit-smoking practitioners.

Some of this has been shifted to the introduction and further discussion of the implications has been added.

p16, l33: "Method 4 further suggests the limitations of the assumptions in Method 3" - not sure what is meant by this.

This statement has been clarified. It relates to the differences in the quit success rate at a unique attempt and suggests that the initial attempt success rate is not as large as that used for method 3.

STROBE STATEMENT

Not sure that there is adequate information presented about treatment of missing data/ loss to follow-up / sensitivity analyses Participant numbers completing each stage of follow-up not described

More details on loss to follow up have been included. A table of eligible participant follow ups has been included.

MINOR/ GRAMMATICAL

consistency of terms, sometimes authors use "lifetable" and sometimes "life table" - stick to one p2, l26: sentence is confusing, suggest replacing "estimate of number" with "estimated number of", and delete "at least one"

p6, l8: should be "only account"

p6, l36: should be "estimates" not "estimating"

p8, l23: "This assumption corresponds to the assumption...." ? This method corresponds to the assumption?

Methods section: Consider including "life table" somewhere in the title of Methods 3 and Methods 4,

as this is the terminology that is used throughout the rest of the manuscript.

p9: l48: "analysis analysis"

p15, l56: "that that"

p16, l8: "attributed to the primarily to the"

Figures: The estimation methods used to derive the figures should be made explicit in the figure titles.

Thank you. All these changes have been made in the text.

Reviewer: 4
Reviewer Name
Anton Kunst

Institution and Country
Department of Public Health
AMC
University of Amsterdam

Please state any competing interests or state 'None declared':
None declared

Please leave your comments for the authors below This introduction of this paper is excellent. The use of life table approach is promising. Yet, I have major concerns about the application of this approach.

1. The authors may need to define more clearly the population of interest: does their objective refer to all smokers (including those who make quit attempts but have not succeeded at the moment of dying) or only to those who did succeed quitting before dying? The title suggests the latter, but a remark made at the end of page 25 suggests the former.

The statement on page 25 has been removed as the parameter we are trying to estimate "average number of quit attempt before success" does not depend on death (except as form of censoring).

2. The description of the methods is unclear, and does not yet permit a detailed evaluation of their validity. As all four types estimates imply some form of modelling, the authors should carefully describing the modelling procedure, which includes a precise description of the mathematics, to be followed by detailing (a) the assumptions and (b) the empirical input to the models.

The descriptions have been edited to attempt to clarify the methods. Method 1 and 2 do not use a life table analysis.

3. The life table approach is highly recommended, but should be explained more clearly along the lines I outlined above. In doing this, the authors could have stuck more closely to terminology used in life table literature. Essentially, the application of life table methodology would imply modelling how the size of a hypothetical cohort of smokers were decrease in relationship to "quit age" (which is 0 for those with 0 past quit attempts, 1 for 1 attempts, etc). The authors could have chosen for a multi-decrement table (exit by successful quitting or by dying) but they seem to opt for single-decrement

table (exit by quitting only). In the latter case, the only empirical input needed is a series of chances of exit (i.e. by successful quitting) in relationship to "quit age".

Thank you for the terminology. Indeed, we do opt for a single-decrement table. There are no identified deaths in the cohort, so the only exits (ie., successful quitting) from the cohort are due to loss to follow up.

4. It is unclear how the authors have estimated the latter empirical input. The authors have described the data sources and variables, but they have not made clear how they derived from this estimates of quit rates in relationship to "quit age". Without a detailed description of this estimation procedure, it is not possible to assess the validity of the core results of this paper.

This data is observed in our cohort for each of the methods. For estimates of quit rates in relationship to quit age is "qi" as described in the methodology on page 12. Figure 1 presents this data. We have now described this calculation more explicitly. The suggestion of improvements in terminology should help clarify this.

5. I think that the life table estimates may be strongly influenced by how they treat the following issues:
- how did the authors take into account the quit rates of those who had already quit after a first few attempts (say, at ages 20-25 years) and who therefore are not included in their survey. Ref 19 and 20 document high quit rates for low 'quit ages'.

- how did the authors take into account the experience of smokers who had died (as smokers not being able to quit) by the time of the survey.

Both quitting at an early age and early death would count as left censored data for Method 4. Method 4 is biased because of this, which is explains partially why the estimates are high. This is not relevant for Method 3 which relies only on observed behaviour during a defined period of time, and functions as a "snapshot". Thus, method 3 represents the behaviour of a defined time period including the young and those will have an easier time quitting. The most important distinction is the level of smoking (the difference between older smokers and younger smokers is quite small compared to the differences between those who have more and less difficulty quitting. We present results subdivided by frequency of smoking which is a clear proxy for this difference to demonstrate the differences between these populations. We have discussed the limitations of these models further in the discussion on page 17.

- how did the authors accurately estimate the "quit age" of the respondents to the survey. The authors themselves stress that self-reports on past quit attempts can strongly underestimate the true number of attempts. If so, their data strongly underestimate the 'quit age' of respondents.

This is indeed a limitation. Method 3 attempts to minimize this effect by only including quit attempts during time under observation which includes smaller intervals of time that are more likely to be remembered (as opposed to Method 2). Method 4 does underestimate quit age.

6. Extensive information of co-variates of quitting, as given in table 2, are not relevant to main objective of this paper. They distract from issues like identified above.

We agree. It is possible that effect modification is of interest and now present the effect stratified by daily/occasional smoking status.

7. The authors present a well-known function of the life table: the absolute number of 'survivors' per 'quit age'. For their purposes, however, it would be more informative to present another function: the absolute number of 'exits' (i.e. number of successful quitters) per 'quit age'. This directly corresponds to the title of the paper. The authors may probably conclude that this "exit distribution" cannot be simply summarised in terms of one average.

Figure 1 now presents the probability of exit (successful quit) per quit age for methods 3 and 4.

VERSION 2 – REVIEW

REVIEWER	Hyoung Suk Lee University of Washington Tacoma
REVIEW RETURNED	06-May-2016

GENERAL COMMENTS	All concerns were sincerely and thoroughly addressed. Especially, the addition of the implication of the study (the increased estimate in the number of attempts to quit) is very reasonable and makes this study meaningful. Only, considering my experiences using both the Time Line Followback (TLFB) and a single question to assess the number of quit attempts, a lot of smokers who participated in the study reported more quit attempts to a single question than to TLFB. Since the point here is that the measure of a single question would not be accurate, I believe that authors should consider the possibility that participants over-reported or overestimated the number of quit attempts in this study too (page 19).

REVIEWER	Emily Stockings National Drug and Alcohol Research Centre, University of New South Wales, Australia
REVIEW RETURNED	28-Apr-2016

GENERAL COMMENTS	The authors have comprehensively addressed my comments. I have no further suggestions.

REVIEWER	Timea Partos King's College London, United Kingdom
REVIEW RETURNED	29-Apr-2016

GENERAL COMMENTS	I am satisfied that the authors have done their best to address the reviewer comments. The manuscript is now much clearer and provides a more critical discussion of the findings. I have only a few minor grammatical changes: p2, line 29: change "attempt prior" to "attempts prior" p6, line 34: change "as determine" to "as it determines" p6, line 44: change "of then event" to "of the event" p7, line 44: change "literature estimating of" to "literature estimates of" p 13, line 18: change "have try and" to "have tried and"

	p14, lines 30-35: I would change this sentence to read: "We also do not know what the effect on success rates would be of quit attempts made in relatively short succession after a failed attempt, compared to attempts made further apart."
	p14, line 37: change "that is use to" to "that is used to"