Reviewer's report

Title: Increasing smoking cessation care across a network of hospitals: an implementation study

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Reviewer: Emma Beard

Reviewer's report:

This study assessed the impact of an intervention on the provision of smoking cessation care to nicotine dependent smokers across a network of hospitals. The study is interesting, but I have a significant number of concerns which are outlined below. In particular, given the time-series nature and clustering of the data (i.e. in hospitals) the use of simple regression is not appropriate. The authors may wish to consider multilevel modelling and assess for possible seasonality trends and autocorrelation. These can be adjusted for using appropriate parameters. The authors should also consider guidelines which have been published on the reporting of interventions and non-randomised trials. In particular, the description of the active ingredients of the intervention is poor. The primary outcome is not clearly specified and a flow diagram of recruitment would be helpful. In addition, it is unclear as to the methods used to randomly select a subgroup of participants, and as to why only 14% of the total possible sample were used. The authors also only focus on dependent smokers, using arbitrary criteria. Surely all smokers are dependent but to varying degrees.

Introduction

• Guidelines around the world, including the UK and US, do not just suggest brief cessation advice but more extensive behavioural support for smokers wishing to quit. In addition, NRT is only one of three first-line therapies (also have Varenicline, Bupropion) and there are many second-line treatments. This first paragraph therefore needs to modified.

• The second paragraph needs rewording. The first sentence would make more sense if the authors said that despite the guidelines the provision of such care is suboptimal. They may then wish to refer to research on the application of evidence based practice and then go on to say that evidence exists to suggest that the provision of smoking cessation care can be improved. This would then lead on to the second sentence regarding the Joint Commission (which should be explained).

• The authors need to set up from the start that they are only interested in the application of smoking cessation guidelines in hospitals. There is a vast literature on the application of guidelines generally which may be applicable but is not referenced.

• The authors focus on the Joint Commission and so should explain and describe what this is in more detail for the international audience. Do recommendations
differ relative to other countries?

- The last paragraph states that the study is being conducted in Australia, but the introduction focuses on America. The authors should, as already stated, give a general overview of smoking cessation treatment around the world and then set the picture for Australia.

Method

Significant amendments are required for the methods section.

- The authors should consider the use of guidelines for the reporting of interventions (e.g. there are CONSORT guidelines for RCTs).
- How were the hospitals randomly selected? i.e. the method of randomisation. It is unclear why 5 hospitals were group c and only 1 group A if randomisation occurred, unless it failed. Would a representative sample have included equal numbers of each?
- Please provide details of how smoking status and demographic characteristics were assessed.
- Please state the method used to randomly select patients, not just the program. Why did you select only 14%?
- Are these measures of nicotine dependence valid? I know time to first cigarette of the day is, but is 10 cigarettes a common cut off for nicotine dependent smokers? Are not all smokers dependent but to different degrees?
- The description of the intervention needs to be improved. What behavioural components did it consist of e.g. goal setting, provision of social support etc. A group of researchers at UCL lead by Professor Susan Michie have developed a taxonomy of behaviour change techniques to help in the reporting of behaviour change interventions.
- What was the primary outcome of the seven reported? All guidelines recommend one primary and then secondary outcomes. The study is powered for the prior.

Analysis

- The authors have time-series data which is multilevel in nature. Their statistical analysis is not appropriate for such types of data. Clustering needs to be accounted for using multilevel modelling, with hospital as a random effect. Given the longitudinal nature of the intervention, it is possible that there are seasonality effects, autocorrelation etc which should be accounted for.
- The authors have attempted a sort of post-hoc power analysis, given the sample size they anticipated to be collected. What sample size would they have required to detect an appropriate effect size. Is a 9% difference likely?
- Why were there significantly more smokers during the intervention period? Were health-care professionals encouraged more to record during this period? Could this have affected the results
- There are clear differences in the profiles of smokers across the three periods. Could this have affected results? This needs to be addressed more thoroughly in
the discussion.

Discussion

• Implications and limitations of the results should be addressed in more detail
• Regression analyses show associations not causation and so care needs to be taken with regards to interpretation.

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

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, and I have assessed the statistics in my report.

Declaration of competing interests:

I declare that I have no competing interests