PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Socioeconomic inequalities in the delivery of Brief Interventions for smoking and excessive drinking: Findings from a cross-sectional household survey in England
AUTHORS	Angus, Colin; Brown, Jamie; Beard, Emma; Gillespie, Duncan; Buykx, Penelope; Kaner, Eileen; Michie, Susan; Meier, Petra

VERSION 1 – REVIEW

REVIEWER	Andrea Gallassi University of Brasília, Federal District, Brazil
REVIEW RETURNED	03-May-2018

GENERAL COMMENTS	5. There is no mention in the text about Ethical Committee approval; 11. The Discussion section should present some Brief Intervention studies conducted in poor country to try to make comparison with the health inequalities (Dalo & Martins. Association between the risk of alcohol use and unprotected sex in adolescents in a city in the southern region of Brazil, Ciência & Saúde Coletiva, 23(1):303-314, 2018); 14. The same comment presents at the point 5.

REVIEWER	Fabienne El-Khoury INSERM, Sorbonne Université, Institut Pierre Louis d'Epidémiologie et de Santé Publique (IPLESP), Department of social epidemiology, Paris, France
REVIEW RETURNED	30-May-2018

GENERAL COMMENTS	This is a well written article about an important Public health subject. The WHO recommends the evaluation of the effect of all public health policies on social inequality, and this analysis tries to do that. However, I would've liked to see the analysis also looking at the Brief interventions' effect (smoking cessation or reduction in alcohol consumption) according to SES. And not just the reception of BI. Studies have shown that people with low SES are more likely to fail their smoking cessation attempts compared to people with higher SES. So despite a "positive" social gradient in the reception of the intervention, it does not mean that it will reduce social inequalities regarding smoking or alcohol. Also, the reasons behind the visit to the GP and the number of cigarettes smoked are not stated. It could be that people with low SES smoke more and are more likely to visit for smoking or alcohol related problems.

Minor comments :
-How was the CI of the prevalence calculated?

-Why didn't you adjust for numbers of cigarettes smoked?

-Did you compare the model fit for the different models with different SES indicators?

-Did you try to create a score for SES?

-Table 1 : + please put missing value percentages, because most of the number s don't add up (for example for age n for past year smokers =6488 and not 6450, n for risky drinkers =3787 and not 3974,…)
+ I don't think "Arab" is an ethnicity. Middle Eastern you mean? For example Iranians are Middle Eastern but they don't speak Arabic…

-Table 2 : would be a good idea to add the unweighted n.

-In the text (throughout the manuscript): use "to" to separate the lower and upper CI instead of "-"

-In the discussion, would be nice to discuss the idea that patients expectations of what the GP could recommend could also be different depending on whether they smoke or drink heavily. In fact, many people don't perceive alcohol drinking as problematic, but they think that smoking is. Patients expectations from the GP could also affect recall; with people thinking that drinking is "not dangerous" probably less likely to recall that their GP talked about their drinking.

REVIEWER	Mark Robinson
	NHS Health Scotland, Public Health Sciences, Scotland
REVIEW RETURNED	14-Sep-2018

GENERAL COMMENTS	**General**
	I was pleased to be given the opportunity to review this paper because, in agreement with the authors' assertion, the extent to which brief interventions may contribute to health inequalities is under-researched. In this paper, the alcohol and smoking toolkit, a data source that is being used increasingly for informing and evaluating policy, has been exploited to examine patterns of BI receipt by socioeconomic status among smokers and risky drinkers at the population level. The paper is well written, the methods are robust and well described, and the results well presented. The authors are also considered in the interpretation of their findings. As such, I am confident that this paper achieves its aim of advancing of our understanding of how delivery of brief interventions for smoking and drinking in primary care might impact on population health inequalities. Overall, the authors should be congratulated on a clear and instructive piece of work.

I have listed my specific comments below. I hope that these will help to improve the paper further, but they should be considered as minor amendments.

Specific comments

Page	Line	Comment
2	8	The use of the terms socioeconomic 'status' is c... that 'position' should be used. See here: http://melb4886.blogspot.co.uk/2014/03/the-trou... status.html?m=1 and here https://www.kcl.ac.uk/ioppn/depts/psychology/re... gs/healthpsych/seminars/DB-Feb14.pdf
2	12	Change risk to risky
2	12	Ad 'years' after 16+
2	20	I wondered whether this definition of smoking wo... to pick up on those who would categorise thems... asked by a health professional.
2	24	'Social class' should be 'social grade'
2	24	Should this be negative socioeconomic gradient... position improves (i.e. positive), BI delivery decr...
2	27	The uncertainty in the best estimate of the assoc... of comment somewhere in the article – probably... Discussion. Also the observation that the relatio... was stronger doesn't seem based on any test, m... judgement. On that basis 'appeared stronger' m... wording.
2	43	The final bullet in the article summary makes a v... glad to see it given such prominence – well done...
3	10	It would be good to see a bit more of the nuance... just a brief mention that there are more non-drin... groups and among those who do drink, especial... guidelines, they tend to drink at higher levels.
3	27	We tried to look at this using our Triple I model (... http://www.scotpho.org.uk/comparative-health/he... tools/informing-investment-to-reduce-health-ineq... showed that ABIs had the potential to reduce he... population level, especially if targeted in more de... include any differential effect, or reach/uptake, b... deprivation (because of lack of evidence!) – ineq... by patterning of outcomes.
3	47	A bit more detail on the smoker definition would... include someone who's smoked a single cigaret...
4	20	It is great that you have examined different meas... position – well done.
4	37	'Organisation' should be 'Organization'
4	40	I'm not suggesting the analysis is changed in an... understanding is that BIs are not recommended... alcohol dependence. This should probably be co... somewhere.
4	44-52	The information on missing data is useful but ca... summarised with the detail being put into the ap...
4	53	Well done for pre-registering your analysis proto...
5	4	35-44. – change full stop to comma
5	6	The order of analysis is interesting. I would norm... entered first, given it is the key explanatory facto... models that attempt to assess how much of the... away' by the demographic and behavioural facto...

	5	16	The inclusion of a linear trend to account for underlying trends in delivery is great.
	5	29	Write out STROBE in full.
	5		As you interpret the results using significance values, you should more detail of this in the methods, particularly given the risks inhe with multiple testing given the number of explanatory variables a models.
	6	49	The word 'unadjusted' could be confused here with weighting. Th authors should seek to ensure there is no ambiguity.
	6	56	It's quite hard to see the patterning for BI delivery among risky dr Figure 1. Consider focusing only on BI delivery for the figure so t can be truncated to show the difference more clearly.
	8	3	Specific that this results subsection is for the adjusted models for smoking
	11	5	I like the way the different models are described – far more instru than Model A etc.
	11	12-23	I wonder how important it is to describe these results in such det the primary research question. There is scope to make the result more focused.
	11	22	The fact that level of alcohol use was a good predictor of BI recei reassuring!
	14	4	The stronger gradient observed for drinking was not tested statis don't think) and has a lot of uncertainty around it so I'm not sure i be a headline observation.
	14	11	Akin to my earlier comment, I don't think these results
	14	28	Limitations para is thoughtful and well articulated.
	14	43	It would be worth expanding this paragraph to explore why the re other studies may be different to those observed here.
	15	20-25	I'm not sure this paragraph adds much to what has already been covered.
	15	-	I'm aware that the Cochrane review, Angus review and the PHE all conclude that ABIs are effective and cost-effective, but I'm als that there those who express skepticism (I think Nick Heather ha a couple of papers on it)
	22	24	Is it appropriate that those referred to specialist services are inclu BIs are not effective for those with possible dependence.
	26		Using asterisks may be the convention of the journal, but present actual p value is better. Also: put all results to 2dps, align the CIs and include OR in the k
	Might be worth mentioning some of the more skeptical views of the effectiveness of BI		

VERSION 1 – AUTHOR RESPONSE

We would like to thank all three reviewers for their clear and insightful comments. We have made numerous revisions to the manuscript in light of their suggestions and we believe the result is a significantly clearer paper.

Responses to specific comments below:

Reviewer 1:

- There is no mention in the text about Ethical Committee approval

As a reanalysis of secondary data, specific ethical approval was not required for this study, however we have added details of the ethics committee approval for the broader Smoking and Alcohol Toolkit Studies

- The Discussion section should present some Brief Intervention studies conducted in poor country to try to make comparison with the health inequalities (Dalo & Martins. Association between the risk of alcohol use and unprotected sex in adolescents in a city in the southern region of Brazil, Ciência & Saúde Coletiva, 23(1):303-314, 2018)

We agree that Low and Middle Income Countries are an important area for future research, particularly in light of the recent review from Joseph and Bassu, which showed that SBIs are likely to be effective in LMIC contexts. However, given wide differences in primary care systems we are cautious about the generalisability of our findings to other countries beyond England and we have added text to that effect to the Discussion section

Reviewer 2:

- I would've liked to see the analysis also looking at the Brief interventions' effect (smoking cessation or reduction in alcohol consumption) according to SEP. And not just the reception of BI. Studies have shown that people with low SEP are more likely to fail their smoking cessation attempts compared to people with higher SEP. So despite a "positive" social gradient in the reception of the intervention, it does not mean that it will reduce social inequalities regarding smoking or alcohol

We agree that understanding how SEP gradients in BI receipt may interact with gradients in health and in effectiveness is an important priority for future research and we have highlighted this in the paper. We hope that the present study will act as a jumping off point for such research in the future, however the toolkit data we have used in the present study does not include questions on BI effects.

- Also, the reasons behind the visit to the GP and the number of cigarettes smoked are not stated. It could be that people with low SEP smoke more and are more likely to visit for smoking or alcohol related problems.

Unfortunately data on the reason for visiting the GP is not recorded in the Toolkit studies, although any visit, irrespective of the underlying cause, is a potential opportunity for intervention. We have added a discussion point noting that if low SEP smokers or drinkers were more likely to visit a GP specifically for a related illness may be the cause of greater delivery in these groups, and should be subject to further research. Table 2 does not suggest that there is a clear SEP gradient in terms of primary care attendance for either smokers or risky drinkers. We have, however, amended the text to make it clearer that the logistic regression models are fitted on smokers and risky drinkers who reported visiting their GP in the past year only.

- How was the CI of the prevalence calculated?

The Toolkit surveys record data on the demographic characteristics of all respondents, including those who do not smoke or drink at risky levels – these CIs are therefore calculated directly (by Stata) using a conventional SEM approach, accounting for survey weights.

- Why didn't you adjust for numbers of cigarettes smoked?

While the epidemiological evidence for alcohol suggests that risk increases monotonically with alcohol consumption, the evidence for smoking primarily focuses on the difference in risk between smokers and non-smokers. Further, while level of smoking is associated with likelihood of a quit attempt being successful, it is largely unrelated to whether smokers attempt to quit in the first place.

- Did you compare the model fit for the different models with different SES indicators?

The fitted models are based on multiple imputation and additionally incorporate survey weights, meaning that the model results presented are in fact derived from multiple MI models, aggregated using Rubin's Rules. It is therefore difficult to conceptualise a meaningful measure of goodness of fit which can be presented alongside the aggregated model coefficients. However, the rationale for using

multiple measures of SEP is that different measures have previously been observed to have different relationships with alcohol consumption. We are therefore seeking to explore how our findings might vary if we use different SEP measures, rather than find the single measure which provides the best model fit.

- Did you try to create a score for SES?

A previous study (Beard et al. DOI 10.1371/journal.pone.0160666) has created a composite SEP measure using data from the toolkit data, however, as outlined above, the focus here was on examining whether the results varied when using different SES measures.

- Table 1 : + please put missing value percentages, because most of the number s don't add up (for example for age n for past year smokers =6488 and not 6450, n for risky drinkers =3787 and not 3974,…)

Missing value rows added to Table 1

- I don't think "Arab" is an ethnicity. Middle Eastern you mean? For example Iranians are Middle Eastern but they don't speak Arabic…

The Alcohol and Toolkit studies are contained within a larger survey administered by the polling company Ipsos MORI. The broader demographic questions, including ethnicity, come from questions within the broader survey and follows the classification system recommended by the Office for National Statistics in the UK (https://www.ons.gov.uk/methodology/classificationsandstandards/measuringequality/ethnicgroupnationalidentityandreligion). The question on ethnicity include 18 alternative options for respondents to select (plus don't know) of which 'Arab' is one.

- Table 2 : would be a good idea to add the unweighted n

We have added this data in an additional table (S1) in the supplementary material on order to avoid Table 2 become too cluttered.

- In the text (throughout the manuscript): use "to" to separate the lower and upper CI instead of "-"

All CIs amended as suggested

- In the discussion, would be nice to discuss the idea that patients expectations of what the GP could recommend could also be different depending on whether they smoke or drink heavily. In fact, many people don't perceive alcohol drinking as problematic, but they think that smoking is. Patients expectations from the GP could also affect recall; with people thinking that drinking is "not dangerous" probably less likely to recall that their GP talked about their drinking

We agree that understanding the factors which may affect delivery rates, including the attitudes of both patients and providers, is crucial in order to make any inroads into addressing the disparity between delivery of BIs for alcohol and tobacco. We have added a sentence drawing attention to some of these possible explanations and highlighting some of the key references for interested readers to follow up.

Reviewer 3:

- The use of the terms socioeconomic 'status' is contentiuous. Some argue that 'position' should be used

Amended

- Change risk to risky

Amended

- Add 'years' after 16+

Amended

- I wondered whether this definition of smoking would be sensitive enough to pick up on those who would categorise themselves as smokers if asked by a health professional

The definition of smokers is deliberately broad in order to ensure that we can capture those who may have attempted to give up in the past year as a result of receiving a BI. We have added precise details of the definition to the Supplementary material, but this includes respondents who answered the question 'Do you smoke or have you ever smoked' with 'I smoke cigarettes (including hand-rolled) every day', 'I smoke cigarettes (including hand-rolled), but not every day', 'I do not smoke cigarettes at all, but I do smoke tobacco of some kind (e.g. pipe, cigar or shisha)' or 'I have stopped smoking completely in the last year'.

- 'Social class' should be 'social grade'

Amended

- Should this be negative socioeconomic gradient? As socioeconomic position improves (i.e. positive), BI delivery decreases (i.e. negative).

Text amended to remove reference to 'negative' gradient, since this is somewhat ambiguous

- The uncertainty in the best estimate of the association is striking – worthy of comment somewhere in the article – probably under limitations in the Discussion. Also the observation that the relationship with risky drinking was stronger doesn't seem based on any test, more of a qualitative judgement. On that basis 'appeared stronger' may be more appropriate wording.

Text amended to reflect uncertainty in headline findings and temper the comparison between the gradients for alcohol and tobacco.

- It would be good to see a bit more of the nuance explained here – maybe just a brief mention that there are more non-drinkers in more deprived groups and among those who do drink, especially those who exceed the guidelines, they tend to drink at higher levels

Text expanded to clarify the conflicting socioeconomic gradients at play in alcohol consumption

- A bit more detail on the smoker definition would be useful – would this include someone who's smoked a single cigarette in the past year?

See response above – full details now added to the Supplementary Material

- 'Organisation' should be 'Organization'

Corrected

- I'm not suggesting the analysis is changed in any way, but my understanding is that BIs are not recommended for those with possible alcohol dependence. This should probably be commented on somewhere

The definition of BI used in the study (as given in the supplementary material) is very broad and includes referral to treatment for those with potential dependence. Main text amended to make this clear.

- The information on missing data is useful but can probably be summarised with the detail being put into the appendix

Text removed as this information has now been added to Table 1

- 35-44. – change full stop to comma

Amended

- Write out STROBE in full.

Amended

- As you interpret the results using significance values, you should provide more detail of this in the methods, particularly given the risks inherent with multiple testing given the number of explanatory variables and models

We do not believe multiple testing is a major concern for this analysis for three reasons:

1) Our analysis protocol was pre-registered
2) While we are fitting multiple models, many of the parameters are shared between models and therefore the total number of associations being tested is much smaller than the sum of the number of covariates across all models
3) Where parameters are shared between models, the associations we find are consistent, providing support for these being genuine associations rather than statistical 'noise'

- The word 'unadjusted' could be confused here with weighting. The authors should seek to ensure there is no ambiguity

'Unadjusted' replaced with 'observed'

- It's quite hard to see the patterning for BI delivery among risky drinkers in Figure 1. Consider focusing only on BI delivery for the figure so the axis can be truncated to show the difference more clearly.

Figure 1 revised to make it easier to see the gradients in BI delivery

- I wonder how important it is to describe these results in such detail given the primary research question. There is scope to make the results section more focused.

As the analysis protocol was pre-registered, we are keen to ensure that the structure of the results section follows the structure set out in the protocol.

- The stronger gradient observed for drinking was not tested statistically (I don't think) and has a lot of uncertainty around it so I'm not sure it should be a headline observation

Whilst there is considerable uncertainty around the precise gradients, across all 4 measures of SEP, the gradient for alcohol is steeper than for smoking and we therefore think that this warrants comment when summarising the results. We have reworded the language to refer to how the association 'appeared' stronger.

- It would be worth expanding this paragraph to explore why the results in other studies may be different to those observed here.

Text amended to suggest that observed gradients may not be consistent across different contexts.

- I'm aware that the Cochrane review, Angus review and the PHE review all conclude that ABIs are effective and cost-effective, but I'm also aware that there those who express skepticism (I think Nick Heather has written a couple of papers on it)

As the aim of this paper is to examine BI delivery, rather than focus on effectiveness, or outcomes, we believe that a deeper discussion of the effectiveness evidence is perhaps beyond the scope of this paper, although it would be extremely relevant for further studies which seek to model the impacts of BI programmes on health and health inequalities.

- Is it appropriate that those referred to specialist services are included as BIs are not effective for those with possible dependence.

As discussed above, we have now amended the main text to make it clear that the definition of BI includes referral to specialist services.

- Using asterisks may be the convention of the journal, but presenting the actual p value is better.

We are happy to be guided by the editor's advice, although we are conscious that including p-values in the tables will make what are already rather busy tables even busier.

VERSION 2 – REVIEW

REVIEWER	Andrea Gallassi University of Brasilia (UnB), Federal District, Brazil
REVIEW RETURNED	22-Oct-2018

GENERAL COMMENTS	The authors have done the revision which was pointed out and there is no additional adjustment.

REVIEWER	Fabienne El Khoury Lesueur Department of Social Epidemiology, INSERM UMR_S 1136, Pierre Louis Institute of Epidemiology and Public Health, Paris, France
REVIEW RETURNED	28-Oct-2018

GENERAL COMMENTS	The revised manuscript take into account most of the comments, however I think that the study design and the available data do not allow to reply to my main comments mainly about looking at the Brief interventions' effect (smoking cessation or reduction in alcohol consumption) according to SES and not just the reception of BI. Also, looking at the reasons behind the visit to the GP and the number of cigarettes smoked also don't seem possible. However, the authors do state these limitations in the revised version of the discussion, therefore readers' interpretation would be more balanced now.

REVIEWER	Mark Robinson NHS Health Scotland, Scotland
REVIEW RETURNED	26-Oct-2018

GENERAL COMMENTS	Thank you to the authors for their detailed and authoritative responses to my comments and to those of the other reviewers. I have detailed below some further minor comments that they may wish to take into account, but I am happy to recommend that the paper is accepted. Congratulations on an important and well executed piece of work. Title: In light of the various comments about the lack of evidence on differential effectiveness of BIs, I wonder if the title should be amended - the study can't really answer the question of whether BIs are affecting health inequalities; rather, it can shed light on the differential reach/receipt of BIs by SEP. Intro, page 3, para 1: Suggest changing "As a result" to "This, in part, has meant...." or words to that effect.

VERSION 2 – AUTHOR RESPONSE

We thank you and the authors for your further comments. We have revised the title of the paper in line with the reviewer's suggestions and your request. We have also made a small change to the wording in the introduction as suggested by Reviewer 3.