Author's response to reviews

Title: An association between seeing incidents of alcohol or drug use in films and young Scottish adults’ own alcohol or drug use: cross sectional study

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Author's response to reviews: see over
Dear Gabriella and Karen Hughes

MS: 1614043084395176.

Thank you very much for forwarding your reviewers’ comments on this paper, and for extending the deadline for receiving this revised manuscript. We are very grateful to all three referees for the careful attention that they have paid to our paper, and we hope that you will agree that our revised manuscript has given equally careful consideration to each of their points. We were very pleased by the very positive overall tone of the reviews, and we were also pleased to have had the opportunity to make amendments which we think improve the paper. We hope that you will now find it acceptable for publication in your journal.

Below we provide a point-by-point response to each of your reviewers’ comments, and in the revised text all changes to the text are highlighted using ‘tracked changes’.

Thank you again for considering this paper. May we take this chance to wish you all good wishes for the coming New Year.

Yours sincerely

Professor Kate Hunt

Responses to Dirk Baier’s reviewer’s report (Referee 1)

Minor Essential Revisions
1. “The title should better be formulated as a question: “Is there an association ...”. This seems necessary because after controlling different background factors (gender etc.) there is no significant association between alcohol/drug consumption seen in films and own alcohol/drug consumption.”

Done.

2. “The subtitle “cross sectional study” should be deleted because the data are longitudinal (data of respondents age 11 and age 15 are used, not only data of age 19).”

We have not done this. The analysis is cross-sectional because the film exposure and substance use data were all collected at age 19, and this is an important influence on the interpretation of our results.
3. “In the method section of the summary should be mentioned how many cases are analyzed (“N < 1002”) and in which years the surveys were conducted (“between 1994 and 2003”)?”

Done.

4.”The Introduction part should additionally deal with the following questions/remarks:
a) Can drug use be affected in the same manner by films like alcohol consumption? Imitating alcohol consumption is much easier because alcohol is better available than illegal drugs. From a theoretical point of view drug use is more deviant than alcohol consumption.
b) Up to now there is nothing said about why some control variables are integrated in the multivariate analyses, others not. For instance leisure time activities or ethnic background are no control variables. Why are social class, parental structure and especially time spend with television controlled? And are there any reasons, why some of the control variables are measured at time 1 (age 11), others at time 2 (age 15) or time 3 (age 19)?
c) Mentioning trends in smoking behavior does not help to introduce the research question. It is said that in different countries there is a decline in smoking prevalence (in Germany, too), but at the same time in many films smoking behavior is still often shown, so the one (decline) could not be attributed to the other (films); the association between contents of films and smoking behavior of adults should be week or even in a reverse direction.”

   a) We already say that there is no evidence that we know of on the influence of film images of drug and we don’t think it is appropriate to expand.
   b) We have included the control variables and the basis of their availability and relevance (on the basis of similar published literature) to the questions addressed in the data set and have taken them from what we consider to be the most appropriate wave of data collection.
   c) The reference to trends in smoking is only made to evidence the point about the inaccuracy of film portrayals of substance abuse; evidence for smoking is the best documented. We hope this is clearer in the revised draft.

5. “Only 1002 out of 2586 persons are at the end analyzed. It is questionable if weighting the data is the best way of dealing with the selective attrition the authors describe; some of the cases in the sample may get very high or very low weights (value above 2 or under 0.5). From a methodological point of view weighting is not necessary because (theoretical plausible) associations are analyzed. Another way of dealing with missing data is (multiple) imputation. Did the authors consider this strategy?”

We point out in the text that weighted and unweighted results were very similar. We agree that there are different ways of dealing with missing data. In another analysis two of the papers’ authors (Young and West, BMC Public Health 2010, 10: 165) have compared weighting and multiple imputation and found little differences on results. We think this is sufficient justification to not use an alternative method of dealing with missing data.

6. “The authors used a rather complex way of constructing indices of film alcohol/drug use. A question is if the different lists of 50 films they handed out to the respondents differed significantly in time the films showed alcohol/drug consumption. Maybe the authors used ten lists with 50 films on each: Did the time of showed alcohol/drug use in the 50 films differed significantly between the ten lists?”

We have followed the method used in much past research in establishing the indices of film alcohol/drug use. Coding of films for such exposures is highly labour intensive so we have utilized the coding already conducted by co-author Sargent’s
trained colleagues. Each of the 1002 participants received a different randomly generated list of 50 films. We have clarified this in the methods.

7. “Using lifetime prevalence for drug consumption as a dependend variable is problematic because of the question of cause and effect. The authors discuss this problem. One argument for including these prevalences in the analyses could be that time of first consumption is perhaps around age 17; so it can be assumed that watching different drug films happened earlier in life then using drugs. Do the authors have data on first drug consumption?”

We have made it more explicit in the text that this is a limitation of our cross-sectional analysis. It would not help to examine data on first drug consumption because we do not have information on when they first saw each film, or how many times they have seen films since first viewing. This is stated in the text.

8. “The significance levels shown in table 2 do not correspond with the significance levels mentioned in the text. The authors write “There were linear trends in the percentage of heavy drinkers (p=.018) and binge drinkers (p=0.12) by film alcohol exposure quartiles”. In table 2 no linear trend can be found (only the quartile 4 differs from the three other quartiles) and the significance levels are p=.054 (heavy drinking) and p=.079 (binge drinking). The same discrepancies can be found for drug use. Which informations are right?”

Please see our response to point 25 raised by Reviewer 2 below where we address this point.

9. “Looking at table 3, an impact of alcohol/drug use in movies can be found only in three of the four dependend variables (heavy drinking, binge drinking, cannabis use; not: hard drug use). …All significant differences disappear after controlling other variables. So all in all the effect of media consumption is completely mediated by some other factors. …”

Please see our response to point 28 raised by Reviewer 2 below where we address this point.

10. “Maybe the association between movies seen and own alcohol/drug consumption could be better analyzed if, in the logistic regression, only two groups (quartile 1 to 3 vs. quartile 4) would be differentiated. Maybe in the last models there would be still significant differences.”

We think it is better to show how all 3 of the higher quartiles of exposure relate to the lowest quartile of exposure.

11. “An important risk-factor for alcohol/drug consumption as well as for media consumption is gender. Did the authors analysed whether there are interaction effects between gender and media consumption on drug use?”

Because we have a primary focus on gender and health behaviours such as alcohol consumption and drug use, we had already tested for interactions between gender and media consumptions. We did not find any significant gender interactions.

12. “The discussion section should in part be rewritten. Up to now a big part of it deals with a study on smoking that is not reported before (and smoking behavior is not analysed before). In the discussion part it seems necessary to deal with two important results of the analyses:
a) Why does only seeing much of alcohol/drug use in films (quartile 4) have an impact on own behavior? Why are quartiles 2 and 3 not affected by the media?
b) In which step of the multivariate analyses the significant effect of quartile 4 disappear? Discussing that helps to find the “real” causes of consume styles (these are gender, personal characteristics, friends behavior). It should also be discussed how media consumption influence these “real” causes.

We have amended the discussion in the light of all of the reviewers’ comments, including some of those raised here. We think that the points raised in the smoking paper about the limitations to this analysis are all relevant to this analysis of the same young people, but in relation to alcohol and drug use rather than smoking. We feel that it would be inappropriate not to refer to our previously published paper that highlights these generic limitations.

Responses to Amanda Atkinson’s reviewer’s report (Referee 2)

We were pleased that this reviewer judged the paper to be interesting and novel, and to provide “much needed research findings from the UK context”.

Discretionary Revisions (which are recommendations for improvement but which the author can choose to ignore)

1) “In the introduction it is suggested that there has been a ‘dramatic rise’ in young people’s drinking. However, there is evidence to suggest a polarisation in drinking patterns with an increasing number of young people abstaining from alcohol use.”

We thank the referee for the additional reference but judged that we had provided sufficient context in the paper on alcohol.

2) “Given that the paper discusses the strong influence of advertising on young people’s alcohol use, literature around ‘product placement’ (i.e. alcohol brands) in films may be worth consideration.”

We have chosen not to incorporate a focus on the literature on ‘product placement’ in relation to alcohol because we wish to keep a relatively equal focus on alcohol and drug use in the paper (and there is obviously no similar brand placement of illicit drugs).

3) “Measuring media ‘effects’ is extremely difficult to establish and it must be considered that the media play a role in young people’s socialisation into alcohol use among multiple factors. Furthermore, causation cannot be established from a cross sectional study, as the authors state it may be that drinkers/drug users seek out film representations of substance use.”

We agree, but we feel that we have already made adequate reference to this point in the first paragraph of the discussion.

4) “The paper states that media portrayals of alcohol and drug use are ‘unrealistic’. This is true in that many forms of media often provide a selective image of substance use. However, the authors should elaborate on the ways in which these portrayals can be defined as ‘unrealistic’.”

We already say that “portrayals of the hazards of drinking were not reflected” in media portrayals on p4. We have added more explicitly at the top of p4 that
portrayals of smoking in films are often glamorized and make smoking appear more prevalent in the community than it is.

5) “Messages around substance use in films occur within the wider media context, in which an array of messages are transmitted over time.. and from a variety of sources.. It is difficult to control for such confounding factors and thus difficult to determine the influence of one media source in isolation.. the paper would benefit from emphasising these points.”

This is a good point. We have added text to the discussion as follows: “Furthermore, it is important to acknowledge that images of substance use in films occur within a wider media context in which a vast array of different images are portrayed over time from a variety of sources (including magazines, TV, newsprint, websites and social messaging sites).”

6) “A consideration of the debate between ‘active’ and ‘passive’ audiences may be beneficial. The authors do touch on this when suggesting that ‘young adults may also have a more sophisticated and critical reading of media effects which makes them more resistant to their effects’.”

We feel that it would disrupt the flow of the discussion to include a more detailed consideration of the literature on ‘active’ and ‘passive’ audiences, or on considerations of the debate about ‘media as powerful’ vs ‘audience as powerful’.

7) “The paper states that the sample of films included additional films that featured stars popular among young people. The paper would benefit from discussing why this is important.”

We included these films to have maximum comparability with previously conducted research.

8) “More detail should be added to explain the concept of ‘normalisation’ introduced in the discussion. There is extensive research literature in this area.”

Again we feel that an extensive discussion of ‘normalisation’ would disrupt the flow of caveats that we are focusing on in our discussion.

9) “By not knowing the age at which drug and alcohol use was initiated, or the dates at which young people had viewed the films, there is no way of determining whether or not alcohol or drug use occurred prior to, or proceeded the viewing of substance use in films.”

We agree. A different longitudinal methodology would be necessary to examine whether the exact dates of viewing films was influential in relation to timing of uptake of health behaviours. This is why we have emphasized the cross-sectional nature of this analysis (in the paper’s title and the discussion) and that we cannot establish direction of causality (in the discussion).

10) “The research does not consider or measure the nature of substance use images in the films. In may be that such images are glamorised or normalised, with the potential to influence initiation. Yet at the same time depictions may be negative with the potential to prevent young people from initiation.”
This more detailed coding of images is very labour-intensive and was not available for the analysis we present here. We now argue in the discussion that future longitudinal studies should be conducted to take account of this.

11) “It is confusing as to why images of empty alcoholic containers (e.g. beer bottles) where excluded when calculating the frequency of alcohol use in the films. If bottles where clearly identifiable as containing alcohol (e.g. by brand logo) then this image is clearly alcohol-related. Moreover, empty bottles may be particularly important as they symbolise that alcohol has actually been drank.”

In the text of the methods we say that “Excluded were occasions when a character had an empty alcoholic beverage container (e.g. empty beer bottle) or when alcoholic beverage containers were displayed but were not implied as being consumed (e.g. bottles shown above a bar).” Hence when it was implied that the alcohol had been drunk then this would have been included.

12) “If it is to be argued that it is the extent to which young people view alcohol-related images in films that influence’s their own use, then the more subtle images of alcohol such as bottles behind a bar or shop counter will also be influential. Background images such as branded bottles are essentially product placement or indirect advertising. Drawing on the evidence of the effects of alcohol advertising on young people’s alcohol use, such images may also be significant.”

As explained in the text, the coding was completed by Sargent’s American team as utilized in other papers for comparability.

Minor Essential Revisions (such as missing labels on figures, or the wrong use of a term, which the author can be trusted to correct)

13) “The paper did not read well in parts and contained numerous typos that should be corrected.”

We have read through the paper carefully and made appropriate amendments.

14) “The paper discusses concern around young people’s alcohol and drug use. However, the references to support this claim relate to alcohol use only. References to support concern around young people’s drug use are required”.

This is now taken out of the first paragraph and introduced in the fourth paragraph of the introduction.

15) “Reference is also made to cannabis use becoming common among young people. The reference used to support this claim is based on Scottish research…. the paper should clarify that this is referring to cannabis use in Scotland.”

Done.

16) “When referring to ‘media’, references should be included that relate to more than one type of media e.g films. (Introduction, paragraph 5, references 16-18).”

We have amended the text to make it clear that we are referring specifically here to images of smoking in films.
17) “The number of top 50 box office hits from the first half of 1999 included in the sample should be specified.”

It is the 50 top grossing box office hits from the first half of 1999 as indicated in the text.

18) “The paper reports that short interviews and physical measurements were conducted by nurses. It is unclear what questions this element of the study addressed and how they relate to the data explored in the paper.”

We included information about the physical measurements for completeness but they do not contribute to this paper so we have deleted reference to them.

19) “Consistency is required in the terminology used throughout the manuscript. In the background section of the abstract, the article states that there has been greater interest in ‘images of certain behaviours in films’… Clarity is needed that these are referring to substance use.”

Done.

20) “Consistency is also required when discussing the studies measures. On a few occasions differing terms are used. e.g. ‘qualifications’ v ‘higher qualifications’. ‘Social class’ v ‘parental social class’. The term ‘family background’ is introduced in the results section, paragraph 6, it is unclear what measures this includes.”

Text has been amended to ensure consistency. The ‘family background variables (which were previously only spelt out in table 3) have now been added to the text in the results section for clarity.

21) “The authors discuss ‘other influences’ that could have had such a strong effect that the impact of exposure was ‘swamped’. Examples of such factors should be provided.”

Done.

22) “When discussing the findings, results should be consistently reported in the text. This is not done for all statements made. e.g in the results section (paragraph 4) P values should be reported alongside statements as in the paragraph above.”

Done.

23) “When discussing the findings of the logistic regression, the text reads- ‘those in the highest quartiles of film alcohol exposure were more likely to be classed as both heavy and binge drinkers’. However, what has actually been measured is ‘film substance use exposure’ (i.e. also includes drug images). This point also relates to discussion around ‘film images of illicit drug use’ (i.e. also include alcohol images).”

The reviewer has misunderstood here. This is such a crucial misunderstanding that we have amended the text on pp8-9 to emphasise that we have separately estimated film alcohol exposure and film drug exposure for each respondent.
Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

24) “Using self reported alcohol use ‘in the last week’ as a proxy for young people’s alcohol use, and as a way of determining ‘binge drinking’ may not have been the best measure of use. Drinking behaviour in the previous week may not be representative of an individual’s usual pattern and frequency of drinking.”

We accept this and have acknowledged this potential limitation in the discussion.

25) “Results section, paragraph 3 is confusing as the P values displayed in the text do not appear to match the table (.018 is .054; 0.12 is .079; .000 is .001 and .033 is .59). The P values in the text are significant and the ones in the table are not.”

We have amended the text to clarify that the p values in the table test for differences between the group, whereas we also tested for linear trends.

26) “In the cross tabulation the association between alcohol and drug use images and own use have been examined separately. It is unclear as to why alcohol and drug use images have been merged in the logistic regression.”

This comment relates to the referee’s earlier misunderstanding about the calculation of exposure to film alcohol use and film drug use which we have clarified in the text as indicated above.

27) “Consistency in the terms used when discussing Chi Square results would be beneficial. The use of terms such as ‘linear trends’ and ‘stepwise increase’ should be clarified.”

See response to point 25 above.

28) “The authors also need to reach an overall conclusion with regards to the strength of the association. The suggestion that there is ‘some association’ is too vague (discussion paragraph 2). In the final element of the LR model, f associations appear not to be significant. This has implications as it suggests findings are not significant when controlling for confounding factors. The effect of viewing film images of alcohol and drugs is diminished when controlling for confounding factors, suggesting impact may be minimal. This should be the conclusion of the manuscript.”

We had felt that our finding of a diminution in the association between film images of substance use and young people’s own substance after adjustment for other factors was sufficiently clear in our previous version, but we see from this referee that it was not. We have emphasized this much more clearly in the discussion to give this the prominence it deserves.

Responses to Bridget Spicer’s reviewer’s report (Referee 3)

We are grateful to referee 3 for her very positive comments on the paper and for reading it with such close attention. She suggested 18 minor points for attention in her comments in addition to suggesting some editing changes to the references. We have followed her suggestions in the amended text for all but 1 of her 18
points, and have also attended to her editing suggestions for the references. The exception is:

Results
5. Table 3: An aesthetic change only- on the bottom line of the table, the brackets have moved onto the following line.

We have ignored this comment as it is not a problem on our file.