Author's response to reviews

Title: New estimates of the number of children living with substance misusing parents: results from UK national household surveys

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Author's response to reviews: see over
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RE: Manuscript MS: 1259433353256191 - New estimates of the number of children living with substance misusing parents: results from UK national household surveys

Dear Editor,

Thank you for providing the reviewers’ comments on this manuscript. We are pleased that they both agree with and acknowledge the importance of this work and that the paper was well written. However their views on how the manuscript can be improved are not consistent, with one reviewer talking from a policy point of view and the other from an academic one.

Normally we would seek your advice on how to reconcile these two reports. However we are under pressure to publish this as soon as possible. Action on Addiction, the charity who part funded the study are holding a launch in the House of Lords about families and addiction on July 1st, and are extremely keen to publicise these findings at this event. Ideally any references to the work then appearing in the media (national press) would be directed to the full article. We have therefore carried out these revisions as a top priority. If the amendments are deemed satisfactory and the paper is acceptable for publication, we would be extremely grateful for an indication of this outcome as early as possible. For example an agreement in principle, would enable the charity to promote this work. We will of course be happy to make further revisions before the paper is published, if required.

The general tone of reviewer 1 is that the results could be more bold and more must be made of the implications in terms of the child protection agenda and preventative efforts. In contrast reviewer 2 suggests that there are several limitations with the data which mean that the estimates are not an improvement on existing ones and that it is too premature to talk about prevention and intervention responses and should merely suggest more research is needed. We feel that reviewer 2 makes some valid comments but some that we do not agree with, as outlined below. We have done what we can to reconcile these views while retaining the integrity of the article. We indicate where changes have been made in the paper according to [Page number, Paragraph number, Line number]

Reviewer 1 (Joy Barlow)

(Discretionary Revisions only)

The focus in the article takes account of the wider context of protecting children. The data contained within lend themselves to discussion on the importance in policy and practice terms, on the areas of early identification and intervention. As government policy is concerned with the earlier identification of potential problems, data on binge drinking and recreational drug use should be further highlighted. Such data have important implications for the wider dissemination of these epidemiological factors to universal and generic services. These are the key services in early identification and intervention to prevent the potential of significant harm.

We agree entirely with this and have added more discussion of this [see 11, 3, 2]
It would be useful to note with rather more emphasis the age of the existing data, and its unreliability when one considers the importance laid upon the scale of the problem in current government strategies. You have just one sentence at the end of one paragraph which asserts this.

We have, as Barlow suggest, linked the targets of the current drug strategy (now stated explicitly on [see 4,1,1-13]) to the need for better estimates and have alluded to the age of the data on which the current targets were set [see 4,2,15]. We have not been as bold as to insinuate that existing estimate are inadequate because as Meier points, the HH estimates answer different and arguably more pertinent questions.

In addition I would suggest that this article is of potential significance. Therefore more can be made, by the use of temperate language of why new data are potentially so important. Without an understanding of the significant numbers we are facing, if matters are left until crisis point we shall not be able to assist children and families. If universal, generic services understand their role in identification and intervention, prevention initiatives may finally bear fruit.

We have stressed the importance of having new estimates in the way Barlow suggests – making the point that knowing about substance use in families is important and that we should avoid waiting until crisis point [see 5,1,1]. To accommodate this we have removed the sentence “It is likely that in the few occasions where actual harm does exist that those attributed, for example to heavy drinking, differ significantly from those attributed to heroin injecting” because on reflection, the findings of the study do not shed any light on how the nature of harm varies with substances use.

In terms of the detail of the data, I would suggest that e.g. the doubling of children living in a household where the only adult is a drug user, is of significance in terms of vulnerability. All of this relates to seeing the wider child protection agenda as that of need as well as risk.

We have added some discussion around this finding in the discussion section [see 10,1,7]

I am also a little concerned about the ambivalence of some comments i.e. a ‘small minority of children are likely to experience clear and identifiable harms,’ I would suggest that recent significant research would lead one to the conclusion that harm may accrue to children over a wide range of situations and circumstances. This is not meant as an attack on drug and alcohol misusers as parents, but a statement of fact. The authors may wish to consider noting the importance of assessment and make further comment on the use of interagency protocols which provide a comprehensive, proportionate assessment on need, harm and risk.

We have removed this sentence retaining the point that whilst substance misuse can impair parenting capacity actual harm is not inevitable. We have reiterated the need for assessment of need risk and harm through inter-agency working [see 12,2,7]. We are trying to avoid speculations about the extent of harm given the limited research evidence available.

The whole article could be more bold in its assertions e.g. the finding should be used to inform and design etc of future research, rather than ‘can’. The findings of this research are of considerable significance, and the article should reflect this in tone and style.

We have changed this sentence as suggested [see 11,2,1] and throughout the revised manuscript have tried to emphasise the significance and application of these findings. This is an emotive subject and we have tried to be as conservative in our interpretation of the data as possible – we feel that policy considerations and assertions should not muddy the basic presentation of these data nor the debate we hope they will initiate.

In the conclusion the point is made that children will be at the greatest risk when living in households unsupported by mainstream services. Such services, using the data advised here, should see their role in explicitly supporting such vulnerable families. In the now well known Scottish phrase ‘It’s everybody’s job to make sure I’m alright’ My advise is – be more bold – we need it!
This is a very good point. We have added a sentence in the conclusion to reflect this [see 13,1,2] and have added a reference to a government document making the same point [see 12,2,13]

Reviewer 2

Major compulsory revisions

1. The central argument by the authors is that current estimates need updating because treatment seekers may not be representative of all drinkers and drug users. Whilst the current estimates include an inflator for problematic users not in treatment, the authors argue that this is still an underestimate of the number of children affected by drinking and drug use. A counter-argument would be that treated drug and alcohol users may be representative of those who drink and use drugs to an extent that might be expected to interfere with childcare. For example, is it plausible that binge drinking as defined in GHS/HSE would be necessarily (or even likely) associated with problematic parenting? Particularly where parents are not sole caregivers, or where children are not very young? What evidence there is about levels of drinking and child outcome should be critically reviewed, and the authors might then propose credible mechanisms by which infrequent drug use, or binge drinking, could pose a risk to children. There is quite a bit of acknowledgement of these issues in the discussion, but since the whole article rests on the assumption that what the authors provide is a better estimate than what we currently have, they might need to make a better case early on for why they think this is so.

The central argument is not that drug users in treatment are not representative of all drug users but that there is a potentially much larger number of adults misusing substances (i.e those who would not be captured in these previous estimates) where there are children in the household and a potential risk of harm. Meier makes a valid point that although the original Hidden Harm estimates are restricted to problem drug users – these are the parents most likely to place their children at risk of harm. However it is acknowledged in the paper by Meier et al (2004) that the study (on drug users in treatment) is “limited by the cross-sectional nature of data available from national treatment monitoring and by the fact that the dataset consists of problem drug users who have presented for treatment, rather than necessarily representing the drug-using population as a whole”. We have dealt with this by softening our criticism of the existing estimates in the introduction, highlighting that they are dated, not based on local data and need to be broader. We have also removed the description “potentially unreliable”. To acknowledge this we have changed the tone of the manuscript slightly so that it suggests these new estimates complement rather than substitute the existing estimates, making the point that these add to what we know but still fall far short of what we need to know [see 9,2,18].

In response to her point about binge drinking and harm, the issue is we simply do not know what detrimental effects parental heavy drinking episodes or recreational illicit drug use has on children. We have added a few sentences to the introduction explaining how non-dependent recreational use of drugs or binge drinking could impair parenting capacity – i.e their effect on judgement and emotional control that are described in ‘Working Together to Safeguard Children’ (HM Government, 2006) [see 3,2,6]. Because of its sensitive nature and parents fears of social services involvement, it is almost impossible to conduct research to answer these questions or even to capture this level information through mainstream services. Retrospective cohort studies have attempted to answer some of these questions and suggest that alcohol abuse is associated with adversity which can result in harm to the child including alcohol problems and psychiatric disorders later in life. We have added a paragraph in the introduction to this effect and have included references to the limited studies that have been conducted on drinking patterns and potential harm (Anda et al., 2002; Dube et al., 2002; Zlotnick et al., 2004; Kestila et al, 2008) [see 3,3,1-12]. There is no scope for critiquing these studies here, as this paper is merely about the prevalence of substance misuse in households were children under 16 reside. It is not about the nature and context in which harm can occur. Nonetheless it is important to recognise that harm to children could occur when parental heavy drinking episodes take place in household. Also, we categorised parents as binge drinkers as a minimum threshold, many more would have problematic drinkers or drinking at a level that might impair parenting capacity. To make this clearer in the manuscript we have added a sentence to the discussion. [see 9,2,7].

2. Drinking definitions
a) This appears a somewhat uncritical adoption of government cut-offs. Why do these make sense in terms of parental responsibilities? What does existing evidence say about levels of drinking and drug use that are associated with “harm”?

The government cut-offs are the most widely used and adopted cut-offs to define drinking patterns. The precise definition of binge drinking in terms of units consumed in a single session has been heavily debated in recent years (see McAlaney & McMahon, (2006). Engineer et al.’s (2003) Home Office study on binge drinking in young adults for example states that “One definition that has frequently been used in the UK is men drinking at least eight units or woman drinking at least six units, on at least one day in the past week”. This definition has been used in several nationally representative, government funded surveys. However the debate has halted somewhat since the government proposed this classification system of exceeding 2-3 units for women and 3-4 for men daily. The AHRSE document states “The best available proxy is the numbers who drank above double the recommended daily guidelines on at least one occasion in the last week. Using this as a measure of ‘binge’ drinking we estimate that around 5.9m adults drink above this level”, so if the upper limits for women are 2.5 (2-3) and for men are 7 (6-8) then consuming more than 5 units (i.e. 6+ units for a women) and more than 7 (i.e. 8+ units for men) constitutes binge drinking. We have added a sentence in relation to the adopted government definition of binge drinking stating that they were used as an accepted UK convention – this is not to imply that there is parental risk for all drinkers meeting these alcohol criteria, nor, indeed that there is no substance-related parenting risk in those who do not reach these thresholds. As discussed earlier we have also added some of the limited evidence on drinking patterns (abuse) and associated harms.

b) There is a slight mismatch with official cut-offs: government & the ONS survey analysts use 50+/35+, not 51+/36+ as in this paper. The authors could use the official cut-offs or remove the reference.

There is not a mismatch and we have used official cut-offs. The classification system for ‘low to moderate’ drinking, ‘moderate to heavy’ drinking and ‘very heavy drinking’ (reported and presented as a figure in AHRSE) is 0-14, 14-35, 35+ for women and 0-21, 21-50, 50+. Since parents in these current databases could only appear in one drinking category, and the cut-offs overlap (e.g. is a female drinking exactly 35 units a week a ‘moderate-heavy’ or ‘very heavy drinker’? we used 15 and 22 to indicate ‘moderate-heavy’ drinking (meaning a woman could drink up to 14 units and a man up to 21 units and be considered a low-to-moderate drinker). Any more than this would be ‘moderate-to-heavy drinking’. We then used 36 and 51 to describe ‘heavy drinkers’. If anything, this more stringent criteria would underestimate the severity of the problem and it is likely that many children live with problem drinkers than the data suggest. We have added a footnote to the method section to briefly describe this [see page 6].

c) Binge drinking is defined by government as regularly exceeding the 6/8 limit, therefore the maximum unit variable is often combined with a frequency variable in the surveys, especially when referring to chronic rather than acute alcohol related harms. Arguably, problems arising from parental binging would arise from either a frequent display of the behavior or a substantial exceeding of these limits. How was this handled – was the “maximum daily” variable manipulated in any way?

We simply used the variable ‘maximum amount of units consumed in the past week on a single occasion’ for binge drinking. We consider weekly behaviour to be a regular behaviour. As previously described in the paper, because of the way in which the data were captured (response options being ‘Did you drink more on some days than others / one of the days’ or ‘did you drink about the same amount on each of these days’) it was only possible to multiply unit variable with the frequency variable for those who reported drinking the same amount each day. The reviewer is correct in her assertion that harm could arise from either frequent display of the behaviour or a substantial exceeding of these limits but these data are not available in these surveys. We would like to emphasise that we are simply making use of data that is already collected (by others) and capturing sizeable populations. We had no influence in the types of questions that were asked in relation to alcohol consumption patterns and thus are limited by the data available.
3. “However, there are also implications from these findings for agencies that aim to encourage the uptake of substance use treatment and for universal educational initiatives aiming to raise awareness of the harms parental substance use can pose to their children.” The authors freely admit that we do not currently understand the association between irregular drug use and non-dependent drinking and harm. So to talk about new education initiatives to educate parents about harm appears pre-mature, and I would probably not go further than to urge that research is needed.

While we accept the limitations of our findings and of the research evidence base around lower level substance use and risky parenting, this does not preclude raising this issue as a long-term implication of this area of work. We disagree with the above comment and – as reviewer one agrees we believe these estimates have implications for mainstream agencies – in terms of early identification and to avoid waiting until crisis-point and indeed when (if ever) research has identified the harms associated with irregular drug use and non-dependent drinking. Two references have been added (Falkov, 1996) which found that parental mental health featured in one-third of child death cases and Forrester et al (2006) found that parental substance use was a concern in 56% of families placed on the child protection register [see 10,1,17]. Therefore early detection of parental problems could well protect children from harm. We know that binge drinking and drug use can easily escalate into more problematic forms and so they key here is prevention. Educational initiatives, encouraging parents to think about their responsibilities and the possible direct and indirect harms that exist when drinking heavily or using drugs recreational and encouraging them to seek treatment for their substance use, can, in our eyes only benefit society. The overarching aim of the paper is to encourage responsible parenting and to raise awareness. We feel that with the additions made in response to reviewer 1 are sufficient to address this comment.

4. Clarify how the data sets are extrapolated to the UK population (use a weighting variable?). The authors should acknowledge problems with this approach, i.e. that understanding of sampling biases gets more important, as they are magnified through weighting. There is also evidence that under-reporting of substance use is not the same across different population groups in household surveys, and the authors need to consider implications.

The data was extrapolated to the UK population by applying the prevalence (%) of children, living with parental substance misusers indentified in the surveys to that of the under 16 year old population of the UK, in the respective year. There were no weighting variables used. We have added sentence to the limitations section on the problems around accentuating sampling or response bias that can be encountered when extrapolating to the UK population. However the consistency in prevalence rates across the various databases suggests that this is not an issue with the current data [see 10,2,5].

5. “This is due to the fact that hazardous and dangerous drinking could only be calculated from weekly units consumed for a sub-sample (those reporting they drink the same amount every day) participating in the national surveys. Thus those exceeding the recommended weekly units by drinking different amounts on different days e.g. 1-2 units 3 days a week and 10 units each day at the weekend would have been excluded.” I am unsure how this works: Would this lead to under- or over-estimation and what about introduction of bias? The key here is for the reader to understand how the authors use this regular-user subsample to extrapolate from this to the whole population. This should be explained in detail in the methods section, not just in the discussion.

Being able to calculate weekly consumption only for those who reported drinking the same amount each day would result in an underestimation of hazardous or dangerous drinkers as we state in the paper. What we have been able to do is make it clearer that this is a huge underestimation [see 6,2,19] in the methods section. We feel this is already covered sufficiently in the discussion which we have since edited slightly to avoid repetition.

6. “It is important to recognize that these new estimates are likely to be conservative estimates and subjected to measurement and reporting bias. The narrow window in which the binge drinking data were captured i.e. restricting to consumption in the week prior to interview will undoubtedly underestimate the
true number of parental binge drinkers, particularly among episodic heavy drinkers.” The argument is somewhat unclear to me. Surveys will not capture some who did not binge drink that particular week, but they will equally capture others who binge drink this week only but not at other times, e.g. because they had a birthday. Would this not even out at a population level?

We agree with the reviewer and so have deleted this sentence

Minor essential revisions

1. “Since women are less likely to access treatment” – this is a controversial issue. My reading of the evidence is that similar amounts of evidence points to an overrepresentation and an underrepresentation of women in treatment, and no definitive study has yet been undertaken. My understanding of the cited Stewart article is that this relates to residential treatment only.

In our experience women are and continue to be underrepresented in substance use treatment. All of our papers published on samples recruited from our addiction services report a ratio of around 3:1 (males to females). We have weakened this assertion and replaced the Stewart reference with that of a review paper by Greenfield et al (2007) [see 4, 2, 8] that concludes “a convergence of evidence suggests that women with substance use disorders are less likely, over the lifetime, to enter treatment compared to their male counterparts”

2. Age of child: If at all possible, the age of children should be considered. It is a qualitatively different situation if the parent of a 15 year old has a binge whilst out with friends than when a lone parent of a 4-month old does (as a minimum I would expect an acknowledgement of this in the discussion). The authors should probably say why age 16 was chosen as a cut-off point.

We agree that it would have been useful to have the ages of the children considered in the analysis and that the likely harms associated with different ages will differ considerably with a risk of neglect and failure to protect at an early age and a risk of modelling behaviours, family conflict etc in later life. However this is not something the current data can answer,. This is something that can be attempted in future research in this area using these datasets. We acknowledge this as a limitation in the discussion section [see 11, 1, 4]. Age sixteen was chosen as a cut-off point because most child protection services are concerned with the welfare of children between the ages of 0-16 and remains the most common UK definition of ‘children’.

3. “Whilst not a measure of problematic use, these estimates exceed the Hidden Harm estimate of 250-350,000”. Bt of an odd sentence: As the Hidden Harm estimate is specifically based on problem users, we would naturally expect an estimate that includes non-problem users to be higher. It is interesting to note that the estimates for children living with hazardous or dangerous drinkers are actually lower than current estimates (this might be an important discussion point?).

The reviewer is correct in her observations and we have replaced this sentence with one stating that it was not possible to make a comparison to the Hidden Harm (2003) estimate because the surveys sampled different populations use [see 9, 3, 2]. We have not highlighted the finding that the number of children living with those in drug treatment is actually lower than the Hidden Harm (2003) estimates because we feel this would confuse the reader. We have discussed in the manuscript that this estimate is likely to be highly conservative because of an underreporting bias of problematic drug use in a household survey context relative to that of drug treatment data.

4. I am missing more discussion of some of the most plausible, but indirect harm mechanisms: health effects on parents, including premature mortality, financial harms through unemployment.

We have a sentence to this effect in the discussion [see 11, 3, 3]
5. “Assessing the number of children who may be exposed to parental substance misuse is a challenging task”. I suggest replacing assessing with “estimating”

We agree, however we felt that this section repeated the points made earlier and so we have deleted this section.

**Discretionary revisions**

1. In addition to the reference to the ACMD report Hidden Harm, the authors might want to consider reference to our more detailed scientific journal article underlying this work (Meier et al, Addiction, 99, 955–961) because this paper includes details on multiple risks that may provide relevant background to the current manuscript. Generally, the literature review of prior work on levels of drinking/drug use and child harm is not as strong as could be.

We have amended the literature review and have included discussion of the above paper [see 12,2,2].

2. **HSE/HSe – use of consistent acronym**
   All acronyms are now consistent throughout the paper

3. **“Alarming” sounds more like media headlines than academic paper.**
   We have changed the wording in this sentence to “a major concern “ [see 10,1,24]

4. **The second paragraph of page 10 is starting to feel somewhat “long-winded”.**

   We agree and this has been edited

We believe that the amendments detailed above have substantially strengthened the manuscript and see its potential to become a widely used reference source for policy and practice and to support ongoing work in this important area of research. Since there is nothing fundamental challenged in the methodology or findings of the paper and the issues around interpretation have now been addressed, we hope we will be able to reconsider publishing the paper in Journal of Public health.

Yours Sincerely

Dr Victoria Manning and on behalf of all authors
Research Fellow
References not featured in the manuscript
