

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Alcohol and drug use among adolescents - and the co-occurrence of mental health problems. Ung@hordaland, a population-based study.
AUTHORS	Skogen, Jens Christoffer; Sivertsen, Børge; Lundervold, Astri; Stormark, Kjell Morten; Jakobsen, Reidar; Hysing, Mari

VERSION 1 - REVIEW

REVIEWER	Arve Strandheim Department of Public Health and General Practice, the Faculty of Medicine, Norwegian University of Science and Technology (NTNU), Trondheim, Norway. Department of Child and Adolescent Psychiatry, Levanger Hospital, Nord-Trøndelag Health Trust, Levanger, Norway.
REVIEW RETURNED	30-Apr-2014

GENERAL COMMENTS	<p>4/12: The missing data information and discussion could be more nuanced. The total N of the age group in "Ung i Hordaland" is not given in the method chapter, so we lack the overall attendance rate. Of the respondents, about 10% were excluded because of missing data. Totally, my guess is, that at least 20% of the cohort is missing. By deleting the missing students from the data set, you analyse "completers only", with a risk of both underestimating (most likely) or exaggerating the relationships. This problem is not very alarming given the size of the population, but the authors can give us the total attendance rates, and more openly discuss possible effect on the results.</p> <p>7/12 chapter Statistics: The statistical methods are mainly logistic regression, which seems adequate given the data material. It is not clarified if the mental health variables are entered in the same model or one by one. The inter-correlation of the mental health-variables are not addressed. What happens when you put all variables in the same model? Is single analysis appropriate??</p> <p>10: Figure 1 is confusing and have an error in the title, alcohol bars are the proportion that never have tried alcohol, while drug bars are the proportion that ever have tried illig.drugs, Either split in 2 figures, with appropriate headings, or turn one of the figures, so they can be compared.</p> <p>Interesting and well written paper. Huge data material and modern, well evaluated mental health variables are used. A discussion of inter-correlation of these variable would strengthen the paper. Some revision needed</p> <p>A little more attention and information in the heading of the figures</p>
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	and tables would make your research ore easily accessible (some of us readers go straight to the tables).
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REVIEWER	Robyn Stargatt So Hlth
REVIEW RETURNED	17-Jun-2014

GENERAL COMMENTS	<p>This is a good study investigating an important area of psychiatric health in young people. Particularly as there is increasing recognition among mental health providers, and the governments that fund them, across the world regarding the interactions between drug and alcohol use and abuse and mental well being. The study is well described and presented and I find very little to criticise. I perhaps would have preferred table 3 to include effect sizes rather than significance with a cohort this large. I have attached a word document of minor errors that perhaps are due to translation.</p> <p>Otherwise I think this should be published and I think it will attract a lot of attention.</p> <p>Pg 3 - L45. "If the relation" should be " Whether the relationship:"</p> <p>Pg 4- L51. "adolescent" should be "adolescents"</p> <p>Pg 5 – L44 "a summed variable including the following gender-specific distribution" should be "a summed variable based on gender-specific distributions"</p> <p>Pg 7 – L40 "was CRAFFT-cases" should be "were CRAFFT cases"</p> <p>Pg 10 – L51 " is highly frequent" should be "is frequent"</p> <p>Pg 11 – L43 "relation" should be "relationship"</p> <p>P 12 – L2 "adolescent versus in an adult population" should be "adolescents versus adults"</p> <p>P12 – L25 "adolescent" should be "adolescents"</p> <p>Figure 1. The "ever" in the heading is inconsistent with the "never" in the descriptor of the vertical axis in the alcohol section of this figure.</p> <p>Table 2. Most journals require a definition of the abbreviations used in the table to be defined underneath.</p>
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REVIEWER	Dr Sukanta Saha Queensland Centre for Mental Health Research, The Park Centre for Mental Health
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REVIEW RETURNED	04-Jul-2014
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GENERAL COMMENTS	<p>The paper assessed association between alcohol or drug use, and common symptoms of depression, anxiety, inattention and hyperactivity. The authors report that both debut of alcohol and drug use and drug-related problems were related to higher symptom levels of depression, anxiety, inattention and hyperactivity.</p> <ol style="list-style-type: none"> 1. In the abstract, no methods were mentioned 2. The results of the paper is not new, several previous papers have found similar results among adults as well as among adolescents. However, it is the debut users among adolescents that make the paper a bit different. 3. P. 4, last para (“the majority of participants responded....”) what is the actual percentage? 4. Both outcome and explanatory variables are not based on any formal diagnosis, also based on self-report. This is a major limitation of the paper. Usually adolescents/young adults make erroneous statements about their behavior towards illicit alcohol or drug use. Still the results carry some weight towards the relationship mentioned. 5. Age and sex are the main variables to control for, are there any socio-economic factors? 6. There are spell mistakes and errors in many places. For example in Fig 1, the Y axis caption is probably wrong (“Percentage NEVER consumed alcohol”). A thorough spell and grammar checking is required. 7. Table 1. This is the first time we see ‘absenteeism’ as a result in the table. No need to put % with all numbers, can go in the heading somewhere. 8. Table 2. All abbreviations should be spelt out (tables should be self-explanatory), significant results can be marked with either bold letters or asterisk (*). 9. Table 3 is not necessary 10. Table 4. dose response: a few rows can be collapsed to make the table concise
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REVIEWER	Erin Winstanley University of Cincinnati, USA
REVIEW RETURNED	04-Jul-2014

GENERAL COMMENTS	<p>The authors state that little is known about adolescent drug use and mental health and that statement is not accurate. There have been several large epidemiological studies, many in the U.S., that describe rates of co-occurring substance misuse/abuse/dependence and mental illness among adolescents. These articles should be cited in order to help the reader better understand how rates of adolescent behavioral health problems in Norway are similar/dissimilar to other national studies. Furthermore, the authors did not discuss important gender differences in the epidemiology of adolescent mental health whereby girls are more likely to experience internalizing disorders and boys are more likely to experience externalizing disorders.</p>
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The term 'exposures' is not appropriate. The outcomes are mental health problems, while research conducted by Kessler and colleagues suggests that the onset of mental health problems occurs, on average, prior to the onset of substance use disorders. It seems more appropriately to frame this as the epidemiology of co-occurring substance use and mental health problems and to describe the association between the two.

Page 3, line 57: "but only girls reported frequent intoxications reported more symptoms of anxiety and depression" – this is not clear and needs to be restated.

It's unclear why 1,017 subjects were deleted from the study, particularly given that there was data missing for the primary outcome variables. Furthermore, it is not clear whether this introduced any response bias.

Alcohol consumption was recoded based on percentiles and it would have been better to categorized based on a standardized/established thresholds of risky and binge drinking.

Binge drinking is usually defined as a number of drinks consumed in one setting, rather than drinking to intoxication more than 10 times. Due to tolerance, some subjects may in fact meet the criteria for binge drinking without self-reporting experiencing the intoxicating effects of alcohol.

The 90th percentile is used frequently and because the raw data is not reported, it's not known what level of symptoms or item endorsement this is associated with. It would be helpful to anchor these artificial cut-points and help the reader understand the potential clinical implications.

Given that the legal drinking age is 18, it would have been helpful to report the percentage of adolescents' aged 16-17 that reported any alcohol use versus the number of adolescents aged 18.

The term "CRAFFT-caseness" is not appropriate and should be restatement as a scoring positive on the instruments, as well as providing an explanation of what a positive score means.

Since the study included adolescents from only one county in Norway, it would be helpful to know the extent to which this county may or may not represent other adolescents in Norway.

In some countries, school-based surveys often miss the most severe behavioral health disorders because these adolescents have either been placed in institutional settings (e.g., mental health facilities, juvenile detection centers) or have dropped out or have been expelled from school. The article states that the survey was mailed to homes for students not in school, but it would be helpful to know more about the extent to which this study reaches adolescents that are not enrolled in school.

The authors appear to have tested for all age and gender interactions and this process did not appear to be guided by a conceptual model nor developmental changes in behavioral health disorders in adolescents.

	<p>The results should be more clearly presented and should include rates of alcohol use, illicit drug use and co-occurring alcohol, drug, depression and anxiety. It's not clear what the magnitude of co-occurring alcohol/illicit drugs and mental health problems is in this sample. Similarly, it would have been helpful to know what percentage of adolescents that report having used alcohol or illicit drugs scored positive on the CRAFFT.</p> <p>The association between adolescent substance use, depression and ADHD has been established and this literature should be appropriately cited.</p> <p>There is no discussion of why these results differ from other studies in the same region – such as where the same assessment tools used and is the age range of adolescents the same. In adolescent behavioral health epidemiological studies, frequently differences in rates are attributable to 1) differential measurement of symptoms, 2) important age differences, and 3) the time period of the prevalence (e.g., past month, past year, or lifetime).</p> <p>In the limitations section, the study name is listed as the “Bergen Child Study” and that is confusing. Are you referring to the current study or another study?</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer # 1

Comment # 1: 4/12: The missing data information and discussion could be more nuanced. The total N of the age group in "Ung i Hordaland" is not given in the method chapter, so we lack the overall attendance rate. Of the respondents, about 10% were excluded because of missing data. Totally, my guess is, that at least 20% of the cohort is missing. By deleting the missing students from the data set, you analyse "completers only", with a risk of both underestimating (most likely) or exaggerating the relationships. This problem is not very alarming given the size of the population, but the authors can give us the total attendance rates, and more openly discuss possible effect on the results.

Response to comment # 1: We share the reviewer's comments regarding the handling of missing data, and also the conclusion that this most likely should cause little concern due to the large sample size. As suggested, we have now included information about the participation rate on page 4 under «study population». Moreover, we have now expanded the discussion on the issue of attrition as a study limitation, which now reads:

“The attrition from the current study could affect generalizability, and result in adolescents attending school being overrepresented. The problem with non-participation in survey research seems unfortunately to be on the rise.[1] Official data show that in 2012, 92% of all adolescents in Norway aged 16-18 attended high school,[2] compared to 98% in the current study. Based on previous research from the former waves of the Bergen Child Study (the same population as the current study), non-participants have also been shown to have more psychological problems than participants.[3] It is therefore likely that the prevalence of mental health problems may be underestimated in the current study. This could suggest that the non-responders also have a higher alcohol and drug consumption, which could bias our sample further. This non-participation bias is, however, not likely to reduce the associations of interest substantially.[4]”

Comment # 2: 7/12 chapter Statistics: The statistical methods are mainly logistic regression, which seems adequate given the data material. It is not clarified if the mental health variables are entered in the same model or one by one. The inter-correlation of the mental health-variables are not addressed. What happens when you put all variables in the same model? Is single analysis appropriate??

Response to comment # 2: We have now tried to clarify this in the revised manuscript under "Statistical analysis", page 7:

"Second, odds ratios were computed using crude logistic regression models for the associations of interest, as well as logistic regression models adjusted for age and gender, and then adjusted for age, gender and SES, with mental health variables both analysed separately, and collectively (i.e. entered into the same logistic regression model)."

We also agree that the overlap between mental health variables justifies an additional analysis including all of the mental health variables. Thus we have now analysed the associations of interest in both separate analyses for the mental health variables, as well as analysing them collectively. The latter results are presented in a table (table 3) and is included in the results and discussion.

Comment # 4: 10: Figure 1 is confusing and have an error in the title, alcohol bars are the proportion that never have tried alcohol, while drug bars are the proportion that ever have tried illig.drugs, Either split in 2 figures, with appropriate headings, or turn one of the figures, so they can be compared.

Response to comment # 3: Thank you for noticing the error in the title. This has now been fixed. We agree with your suggestions on presentation of the results and have now split the figure into two separate figures, one for alcohol and one for illicit drugs in the revised manuscript (Figure 1 and Figure 2).

Interesting and well written paper. Huge data material and modern, well evaluated mental health variables are used. A discussion of inter-correlation of these variable would strengthen the paper. Some revision needed

A little more attention and information in the heading of the figures and tables would make your research ore easily accessible (some of us readers go straight to the tables).

Response to comment # 4: We agree with the reviewer, and have now tried to revise the tables aiming to make them more clear and independent of the main text.

Reviewer # 2

This is a good study investigating an important area of psychiatric health in young people. Particularly as there is increasing recognition among mental health providers, and the governments that fund them, across the world regarding the interactions between drug and alcohol use and abuse and mental well being. The study is well described and presented and I find very little to criticise.

I perhaps would have preferred table 3 to include effect sizes rather than significance with a cohort this large.

Response to comment #1: We agree with the reviewer, but in response to comments by the other reviewers, we have now replaced table 3.

I have attached a word document of minor errors that perhaps are due to translation.

Otherwise I think this should be published and I think it will attract a lot of attention.

Pg 3 - L45. "If the relation" should be "Whether the relationship:"

Pg 4- L51. "adolescent" should be "adolescents"

Pg 5 – L44 "a summed variable including the following gender-specific distribution" should be "a summed variable based on gender-specific distributions"

Pg 7 – L40 "was CRAFFT-cases" should be "were CRAFFT cases"

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Pg 11 – L43 "relation" should be "relationship"

P 12 – L2 "adolescent versus in an adult population" should be "adolescents versus adults"

P12 – L25 "adolescent" should be "adolescents"

Responses to comment #2-9: We appreciate pointing out these errors, and we have now changed these in the revised manuscript according to your comments.

Figure 1. The "ever" in the heading is inconsistent with the "never" in the descriptor of the vertical axis in the alcohol section of this figure.

Response to comment # 10: This has now been fixed, and we have made two independent figures, as suggested by reviewer 1.

Table 2. Most journals require a definition of the abbreviations used in the table to be defined underneath.

Response to comment # 11: We agree that abbreviations should be spelled out in the footnotes of the tables/figures, and we have now included this information in all the tables and figures throughout the paper.

Reviewer # 3

The paper assessed association between alcohol or drug use, and common symptoms of depression, anxiety, inattention and hyperactivity. The authors report that both debut of alcohol and drug use and drug-related problems were related to higher symptom levels of depression, anxiety, inattention and hyperactivity.

Comments

In the abstract, no methods were mentioned

Response to comment # 1: We have now added the following sentence to the abstract (under outcomes):

"Statistical analyses included logistic regression models."

The results of the paper is not new, several previous papers have found similar results among adults as well as among adolescents. However, it is the debut users among adolescents that make the paper a bit different.

Response to comment # 2: We agree with the reviewer on the novelty of this paper. After presenting some of the previous literature on the general associations in adults and adolescents, we have now added the following sentence to the introduction regarding the lack of studies on debut and mental health among adolescents (page 4):

“Furthermore, most of the previous studies have focused on drug and alcohol use, whereas less is known about the relationship between mental health and debut in adolescence.”

P. 4, last para (“the majority of participants responded....”) what is the actual percentage?

Response to comment # 3: We have now specified the participation rate in the methods section (page 5).

Both outcome and explanatory variables are not based on any formal diagnosis, also based on self-report. This is a major limitation of the paper. Usually adolescents/young adults make erroneous statements about their behavior towards illicit alcohol or drug use. Still the results carry some weight towards the relationship mentioned.

Response to comment # 4: We agree with the reviewer that it would be advantageous to obtain information from multiple sources, not relying only on self-reports, and the following is included in the revised manuscript under «Strengths and limitations», page 11:

“Lastly, the study is based on self-reported questionnaires assessing symptoms of both alcohol and drug problems and mental health problems, and multiple information would have been have been preferable to reduced informant bias and common method bias. Also, self-reported questionnaires do not provide information regarding the existence of a clinical diagnosis, and the lack of a clinical interview in confirming a formal diagnosis is a limitation of the present study»

Age and sex are the main variables to control for, are there any socio-economic factors?

Response to comment # 5: We have now included perceived family affluence as an indicator of SES. The inclusion of this covariate only marginally changed the estimated associations. (see the revised table 2 and result section).

There are spell mistakes and errors in many places. For example in Fig 1, the Y axis caption is probably wrong (“Percentage NEVER consumed alcohol”). A thorough spell and grammar checking is required.

Response to comment # 6: We have now carefully checked the manuscript and done our best to remove all spelling mistakes and grammatical errors.

Table 1. This is the first time we see ‘absenteeism’ as a result in the table. No need to put % with all numbers, can go in the heading somewhere.

Response to comment # 7: We have now revised this table and moved the «%» to the heading. We have also revised all tables aiming to make them more clear and independent of the main text. The term absenteeism is removed from the label and the heading now reads:

“Table 1: Patterns of drinking and drug use, and problematic alcohol and drug use (CRAFFT positive) in adolescents born from 1993-1995, stratified for gender. N=9,203 (girls n=4,995, boys n=4,208).”

Table 2. All abbreviations should be spelt out (tables should be self-explanatory), significant results can be marked with either bold letters or asterisk (*).

Response to comment # 8: We have now expanded the titles of all figures and tables to make them more self-explanatory and independent of the main text.

Table 3 is not necessary

Response to comment # 9: We agree, and table 3 have now been replaced by a new table indicating the association between alcohol and drug use with all mental health problems entered into the regression model collectively, as suggested by reviewer 1.

Table 4. dose response: a few rows can be collapsed to make the table concise

Response to comment # 10: We agree with the reviewer, and have now collapsed a few rows for the alcohol consumption categories.

Reviewer # 4

The authors state that little is known about adolescent drug use and mental health and that statement is not accurate. There have been several large epidemiological studies, many in the U.S., that describe rates of co-occurring substance misuse/abuse/dependence and mental illness among adolescents. These articles should be cited in order to help the reader better understand how rates of adolescent behavioral health problems in Norway are similar/dissimilar to other national studies.

Response to comment # 1: We agree with the reviewer, and now cite some key publications, based on data from the National Comorbidity Survey Replication - Adolescent Supplement, [NSC-A; 5] as well as a European and an Australian epidemiological study on co-occurrence of mental disorders [6, 7]:

“Previous studies of adolescents have found that a considerable proportion with one mental disorder also meets the criteria for at least one other mental disorder, including substance-use disorder.[5-7]”

Furthermore, the authors did not discuss important gender differences in the epidemiology of adolescent mental health whereby girls are more likely to experience internalizing disorders and boys are more likely to experience externalizing disorders.

Response to comment # 2: We agree that this important information was missing in the original manuscript, and we have therefore added the following sentence to the introduction (page 4):

“In general the mental health problems show a consistent gender pattern in adolescence, with girls having a higher rate of depression and internalizing problems, while boys have a higher rate of externalising disorders.[5]”

The term ‘exposures’ is not appropriate. The outcomes are mental health problems, while research conducted by Kessler and colleagues suggests that the onset of mental health problems occurs, on average, prior to the onset of substance use disorders. It seems more appropriately to frame this as

the epidemiology of co-occurring substance use and mental health problems and to describe the association between the two.

Response to comment # 3: We agree with the reviewer that the use of «exposure» is a misnomer, and we have changed the wording throughout the revised manuscript.

Page 3, line 57: “but only girls reported frequent intoxications reported more symptoms of anxiety and depression” – this is not clear and needs to be restated.

Response to comment # 4: We have now tried to clarify this sentence, and it has been changed to:

«For instance, a recent study investigating the association between alcohol intoxications and mental health problems among 13-19-year-olds found that high level alcohol consumption was associated with conduct and attention problems for both genders, but only the girls who reported frequent intoxication reported more symptoms of anxiety and depression.»

It's unclear why 1,017 subjects were deleted from the study, particularly given that there was data missing for the primary outcome variables. Furthermore, it is not clear whether this introduced any response bias.

Response to comment # 5: We agree with the reviewer that this was not described clearly, and have tried to clarify the issue in the revised manuscript. Particularly, we have specified why there are fewer participants in the analyses including «frequency of intoxication» by including the following sentence under «statistical analysis» (page 8):

«Due to missing information on the frequency of intoxication question, the analyses involving this variable only included n=9,056 participants. Frequency of intoxication was included as an independent variable in order to retain as much information as possible.»

Alcohol consumption was recoded based on percentiles and it would have been better to categorized based on a standardized/established thresholds of risky and binge drinking.

Response to comment # 6: We agree that the use of existing category thresholds may provide additional and useful information in general. When deciding on the use of percentiles, this was based on the lack of validated thresholds for Norwegian adolescent. The existing thresholds are prescribed to an adult population. Also, self-reported alcohol use is subject to underreporting (see for instance [8-12], and there are indications that the amount of underreporting varies across different populations [10, 11]. It has also been suggested that the guideline thresholds for risky drinking are «artificially low» [10]. Furthermore, most studies investigating underreporting as an issue have focused on adult populations, less is known about how underreporting of alcohol consumption affect population-based studies of adolescents. We tried to resolve the issue of underreporting by defining the alcohol use groups by their gender-specific rank order. This is an approach we have used previously in several studies [13-17]. We think this at least mitigate the issue of underreporting in analytic epidemiology as the percentile-categories are broad enough to at least retain the rank order of alcohol consumption.

Based on these considerations we have chosen to keep the categories previously employed in the revised manuscript, but we are open for further suggestions from the reviewer.

Binge drinking is usually defined as a number of drinks consumed in one setting, rather than drinking to intoxication more than 10 times. Due to tolerance, some subjects may in fact meet the criteria for binge drinking without self-reporting experiencing the intoxicating effects of alcohol.

Response to comment # 7: We agree with the reviewer, and have changed the term «frequency of binge drinking» to «frequency of intoxication» and the term «frequent binge drinking» to «frequent intoxication» in the revised manuscript.

The 90th percentile is used frequently and because the raw data is not reported, it's not known what level of symptoms or item endorsement this is associated with. It would be helpful to anchor these artificial cut-points and help the reader understand the potential clinical implications.

Response to comment # 8: We agree with the reviewer and have included information about the range and mean of the raw score for each of the mental health scales in the methods section (page 7).

Given that the legal drinking age is 18, it would have been helpful to report the percentage of adolescents' aged 16-17 that reported any alcohol use versus the number of adolescents aged 18.

Response to comment # 9: We agree with the reviewer and have included the following in the results section (page 8):

«Among the participants aged 17, 35.3% had not tried alcohol, compared to 16.0% aged 18 or more ($p < 0.001$)»

The term “CRAFFT-caseness” is not appropriate and should be restatement as a scoring positive on the instruments, as well as providing an explanation of what a positive score means.

Response to comment # 10: We agree with the reviewer and have changed the term «caseness» accordingly in the revised manuscript.

Since the study included adolescents from only one county in Norway, it would be helpful to know the extent to which this county may or may not represent other adolescents in Norway.

Response to comment # 11: We see that this information was missing from the original paper, and we have now added the following paragraph to the Methods section regarding the representativeness of Hordaland County (page 5):

“Hordaland County is generally regarded representative of Norway as a whole, comprising both the second largest city of Norway (Bergen), as well as large rural areas. Official statistics of the general population (not just adolescents) show that Hordaland County does not deviate in any substantial degree from the national average on key parameters, including both socio-demographic indicators and several health indicators.”

This information is only available in Norwegian:

<http://khp.fhi.no/PDFVindu.aspx?Nr=12&sp=1&PDFaar=2012>

In some countries, school-based surveys often miss the most severe behavioral health disorders because these adolescents have either been placed in institutional settings (e.g., mental health facilities, juvenile detention centers) or have dropped out or have been expelled from school. The article states that the survey was mailed to homes for students not in school, but it would be helpful to know more about the extent to which this study reaches adolescents that are not enrolled in school.

Response to comment # 12: We agree that including a representative sample in epidemiological

studies of adolescents is challenging. In addition to the procedure described in the Methods section through email, SMS and mail, we also contacted institutions you mentioned such as juvenile detention centers, mental health facilities, and other institutions caring for adolescents. The adolescents at these facilities all had the possibility to participate. We do not have information on the actual attendance rate from the specific institutions. We do know that most of the adolescents participating were pupils attending school, and former waves of the Bergen Child Study indeed have found a higher level of psychological problems in non-attenders. This information is now included in the revised manuscript under “Strengths and limitations”, page 11:

“Fourthly, due to missing information, almost ten percent of the total eligible study population was not included in our analyses. The attrition from the current study could affect generalizability, and result in adolescents attending school being overrepresented. The problem with non-participation in survey research seems unfortunately to be on the rise [1]. Official data show that in 2012, 92% of all adolescents in Norway aged 16-18 attended high school [2], compared to 98% in the current study. Based on previous research from the former waves of the Bergen Child Study (nested within the current study), non-participants have also been shown to have more psychological problems than participants [3]. It is therefore likely that the prevalence of mental health problems is underestimated in the current study. This could suggest that the non-responders also have a higher alcohol and drug consumption, which could bias our sample further. This non-participation bias is, however, not likely to reduce the associations of interest substantially.[4]”

The authors appear to have tested for all age and gender interactions and this process did not appear to be guided by a conceptual model nor developmental changes in behavioral health disorders in adolescents.

Response to comment # 13: We agree with the reviewer that the rationale behind the interaction analyses are not clearly stated in the original manuscript. Specifically we have added the following section under «statistical analysis», page 7:

«In addition, as previous studies have shown differential age- and gender effects in the development of both mood disorder, anxiety disorder, behaviour disorder and substance use disorders among adolescents, [40] we investigated the potential two-way interaction with age and gender for the alcohol- and drug-related variables in relation to mental health problems.»

The results should be more clearly presented and should include rates of alcohol use, illicit drug use and co-occurring alcohol, drug, depression and anxiety. It's not clear what the magnitude of co-occurring alcohol/illicit drugs and mental health problems is in this sample.

Response to comment # 14: We have tried to clarify this in the results section and the discussion section in the revised manuscript. We have not included a full description of the rates for co-occurring alcohol and drug use and mental health problems, as the odds ratios given in table 2 and 3 gives the estimates the co-occurrence between the independent and dependent variables in this study. This is due to the already considerable length of the manuscript, as well as the consideration that providing rates would not provide substantial additional information than the information already included in the manuscript. Also, since mental health problems (and high-level alcohol consumption) are all defined as ≥ 90 th percentile of the summed scores on the different questionnaire, we are uncertain whether providing the rates would be of value for the reader.

We are however open for further revisions if the reviewer still finds it necessary.

Similarly, it would have been helpful to know what percentage of adolescents that report having used alcohol or illicit drugs scored positive on the CRAFFT.

Response to comment # 15: We agree with the reviewer, and have added the following in the methods section, page 6:

“In relation to the ung@hordaland sample, a linear relationship between CRAFFT-score and excessive alcohol consumption, frequency of intoxication and debut of illicit drug use has previously been reported. [13] In the current study, 28.4% of those having tried alcohol were CRAFFT-positive, while 61.1% of those having tried illicit drugs were CRAFFT-positive.”

The association between adolescent substance use, depression and ADHD has been established and this literature should be appropriately cited.

Response to comment # 16: We agree, and have included more information about relevant studies in the revised manuscript.

There is no discussion of why these results differ from other studies in the same region – such as where the same assessment tools used and is the age range of adolescents the same. In adolescent behavioral health epidemiological studies, frequently differences in rates are attributable to 1) differential measurement of symptoms, 2) important age differences, and 3) the time period of the prevalence (e.g., past month, past year, or lifetime).

Response to comment # 17: We did not find gender or age confounding factors for the association between alcohol and drug use and mental health in the present study, which was in contrast to some other studies. We agree that we did not sufficiently discuss the possible reasons for this discrepancy in the original manuscript, and have therefore revised some of the discussion and added the following sentence on page 13:

Overall, there may be multiple reasons for discrepancies between our findings and previous studies. The recency of our data collection in comparison to a decade or so older comparison studies might have impacted the results, especially with regards to the pattern of drug and alcohol use. Also, the assessment of mental health differs across studies, with most other studies using brief general mental health measures.

In the limitations section, the study name is listed as the “Bergen Child Study” and that is confusing. Are you referring to the current study or another study?

Response to comment # 18: The fourth wave of the longitudinal population-based study the «Bergen Child Study» is nested within the ung@hordland-study. We agree that this was not described with sufficient clarity, and have now included more information about this on page 4 under «Study population». We hope this clarifies the mentioned sentence in the limitations section.

References

1. Morton, S.M.B., et al., In the 21st Century, what is an acceptable response rate? Australian and New Zealand Journal of Public Health, 2012. 36(2): p. 106-108.
2. The Directorate of Integration and Diversity (IMDi), [Education participation in high schools]. 2012, <http://www.imdi.no/no/Fakta-og-statistikk/Fakta-og-statistikk/Utdanning/?tab=chr>.
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VERSION 2 – REVIEW

REVIEWER	Erin Winstanley University of Cincinnati College of Pharmacy
REVIEW RETURNED	17-Aug-2014

GENERAL COMMENTS	The authors did a nice job responding to the reviewers comments and the manuscript is significantly improved.
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