PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Can use of health care services in 15–16-year-olds predict an increased level of high school dropout? A longitudinal community study.
AUTHORS	Homlong, Lisbeth; Haavet, Ole; Rosvold, Elin

VERSION 1 - REVIEW

REVIEWER	Theresa Fleming University of Auckland
REVIEW RETURNED	25-May-2013

THE STUDY	Standard of written English: This article would benefit from a professional edit. At times the ideas are not expressed clearly andsentences are hard to follow. There are grammatical errors and at times a different word from the one chosen would be more appropriate. The article would benefit from a careful review of the discussion. There are good points here, but it could be tightened up. Please give full terms for acronyms used. The references need tidying up. It is unusual to refer to 15 and 16 year olds as 15 year olds. Methods: Please give the total number of adolescents approached to participate in the survey. Please give a brief data re reasons for non participation if possible. Please provide a little more brief description about the survey. E.g is this of adolescents present on a particular school day only? Was the survey carried out in all 6 counties each year? is it a pen and paper survey? etc.
RESULTS & CONCLUSIONS	Please check the data in table 1 carefully. The totals and percentages given appear to contain numerous errors. Please check consider if the data in table 2 can be presented more clearly. I am not sure what the numbers in brackets in table 2 refer to. Please consider adding a column to this table for no health problem and or for total sample. Please check your rounding in the text compared to the numbers in the tables. There are examples in table 3 where the number in the text and the number in the table are slightly different.

	Please review the titles and labelling for the tables. For example the title for Table 4 is long and provides information not normally included in a title. You have reported that you have adjusted for socio-economic variables but you have not said what these variables are/how they are measured. How have you dealt with missing data? There are big differences in findings for adjusted compared to unadjusted odds ratios. Has this data been closely checked? It would be useful to comment on this in the text and to consider the reasons. Please consider whether adjusted or unadjusted odds ratios are your focus and why, and then focus on one of these in the discussion. I haven't reviewed other data closely as these major errors need addressing before the remaining data can be fully considered.
REPORTING & ETHICS	Ethical approval is not detailed.
GENERAL COMMENTS	This is a worthwhile research question and a good use of data. It is clinically useful research. Important points have been raised in the article. Relevant research has been considered. Although I have noted many issues which need to be addressed I do think that if there is careful checking of the tables and a professional edit this work should be published.

REVIEWER	Henrik Daae Zachrisson, PhD, The Norwegian Center for Child Behavioral Development, University of Oslo I am involved in a study using the same data on a bordering topic. Otherwise no conflicts of interest.
REVIEW RETURNED	30-May-2013

GENERAL COMMENTS	This study investigates whether contact with health-services among mainly 15-year olds in Norway predicts high-school dropout during the following 5 years in a large, high-quality dataset. Overall, evidence is found that contacts with school health services, youth health clinics, and child and adolescent mental health services predict higher rates of dropout, while contact with general practitioner predicts lower dropout for boys. The topic for this study is timely and interesting. The data is in all respects outstanding for studying this topic. The paper is in general well structured and written. I have, however, a number of concerns about the paper in its current form which makes me suggest a major revision before acceptance. At a general level, I struggle with understanding the scope of the research question and the design. This may be due to the introduction, which is quite brief. First of all, I am uncertain whether the authors address contact with health-services as a potential causal agent for drop out, whether they address this contact as a proxy for drop-out, or whether they seek a potential venue for prevention of drop out. The authors make the first suggestion explicit in the limitation section, where they state that no causality can be established. Furthermore, the design, which conditions on health-related and demographic characteristics, suggests that they want to

	isolate the unique effect of contact with health services. If this is the case, the authors should provide a stronger argument for why this might be the case. It seems not intuitive to me that contact with health services should cause adolescents to dropout from high-school. It may be a reasonable hypothesis, please explain. If the authors are mainly interested in contact with health services as a proxy for other risk factors, I wonder why they do not study the effect of poor health and demographic characteristics, and use contact with health services as moderator (is this a marker of greater risk among adolescents with, e.g., poor health?) or a mediator (is contact with health services the mechanism that makes people with poor health drop out? – related to the previous point). Or they could compare adolescents with similar health profiles seeking- and not seeking help, to determine whether services influences on drop out for certain groups. If they are interested in health services as a venue for establishing contact with adolescents at risk for drop out (as suggested in the discussion), then it might be relevant to investigate for whom this should be considered. Given the high number of adolescents in contact with health services, it seems unrealistic to establish preventive actions for all. Under all circumstances, I struggle to see why conditioning on health is the way to address any of these questions. It makes an odd counterfactual to examine contact with health services regardless of health and demographics– as these are the main causes for contact with health services. At least I would like to see an explanation for the choice of design more closely related to the research question. In sum, an extended introduction which more clearly explains the rationale for the study and the design would improve the paper. Another point related to the introduction is that it seems in one way to have a very local scope –as neither the contexts of the health-care system nor the high-school system are considered. These contexts are not familiar to non-norwegian readers. The authors do, however, some of this in the discussion. I assume that there will be context specificity to such findings, and the research question may become clearer if this is laid out from the beginning. I also have some more detailed comments regarding design and statistics. Demographics are not described in the method section. If, as I believe is the case, the authors have access to information about ethnicity, this should at the very least be included as covariate, but I would also consider it a potential moderator. The authors may also think of considering low income families beyond poverty, as there may be a dose-response relationship both with health-service use and dropout. Furthermore, dummies for counties and actual student age should be considered as covariates. I also think the authors should use a sandwich-estimator or multilevel design to account for nesting in schools, if this is available. A multi-level approach may be considered given that access to youth mental health services may way by region or school, and as students at some schools may have higher dropout rates than others. Also, the authors may find logistic regression inappropriate for making their interpretations given the prevalent outcome (see e.g., McNutt et al, 2003), as this may lead to overstating the interpretation of OR as relative risk (which I believe they in fact do). Finally, it was not immediately easy to understand what the parentheses in Table 2 are, please add explanation.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1: Theresa Fleming
University of Auckland

Standard of written English:
This article would benefit from a professional edit. At times the ideas are not expressed clearly and sentences are hard to follow. There are grammatical errors and at times a different word from the one chosen would be more appropriate.

Response: To improve the standard of the English writing, we decided to do a language edit before submitting a revised version of the manuscript. All changes concerning words and expressions are highlighted in the text. We have tried to correct all grammatical errors, but of course, we can have missed some.

The article would benefit from a careful review of the discussion. There are good points here, but it could be tightened up.

Response: We have worked on the discussion, deleted one paragraph, which made the discussion shorter and more 'to the point' and focusing on the results from the adjusted model.

Please give full terms for acronyms used.

Response: The acronyms HSCL-10 and SCL-25 have been given full terms in the 'methods' section were they are first described in the manuscript.

The references need tidying up.

Response: We have carefully checked the reference list and found some errors, which have been corrected.

It is unusual to refer to 15 and 16 year olds as 15 year olds.
Response: We have decided to change '15-year-olds' to '15–16-year-olds' throughout the manuscript.

Methods:
Please give the total number of adolescents approached to participate in the survey.

Response: The total number of adolescents invited to the baseline survey was 18 425. This number comprised all 10th grade secondary school pupils in the six regions included. The number of invited pupils has been added to the text in the 'methods' section.

Please give a brief data reasons for non-participation if possible.

Response: A few of those invited was not reached because they had moved, while some pupils of foreign origin who figurated on the class lists at time of the invitation, were not present and had probably emigrated when the survey took place. Some of the pupils not present at the day of the survey did not respond, neither on the questionnaires left for them at school, nor on the questionnaires sent by mail to their home address. We don't have any further information on these few non-responders.

Please provide a little more brief description about the survey. E.g is this of adolescents present on a particular school day only? Was the survey carried out in all 6 counties each year? is it a pen and paper survey? etc.

Response: We agree that the baseline survey was quite briefly accounted for in the manuscript and have decided to give an extended description, which we have added to the 'methods' section. We have also supplemented the reference list with an article that describes how the survey was planned and organised in a more detailed way (Soegaard et al), in addition to the already included net site reference from the Norwegian Institute of Public Health.

Please check the data in table 1 carefully. The totals and percentages given appear to contain numerous errors.

Response: As you correctly pointed out, there were errors in table 1. It turned out that some numbers had been filled in wrongly. The total number of boys is 6425, not 5425. In addition, we found some decimal errors. In the revised version of the manuscript, we have supplemented the descriptives of sociodemographic variables with missing values and included ethnicity as a covariate, as suggested by the second reviewer. We also made minor changes of the categories under 'family economy' and 'parents educational level', more thoroughly described in the 'methods' section. Thus, the numbers and percentages in table 1 are slightly different in the new version of table 1, compared to the first submitted version.

Please check consider if the data in table 2 can be presented more clearly. I am not sure what the numbers in brackets in table 2 refer to.
Response: We agree that table 2 is unclear and not immediately easy to understand. This was also commented by the second reviewer. We have decided to make major changes to improve the quality of this table.

Please consider adding a column to this table for no health problem and/or for total sample.

Response: In table 2 the row with the heading 'Overall numbers', show the numbers of pupils/percentages of reported health problems in the total study population, statified by gender. We have added a new heading in the start if this row to make this clearer. In addition, we have added N and % at the top of each column, which refers to the numbers outside and inside the brackets in the columns. You can also find supplementary information in Table 3, both on health indicators and percentages of pupils reporting different levels of health care seeking in the total population, also here stratified by gender.

Please check your rounding in the text compared to the numbers in the tables. There are examples in table 3 where the number in the text and the number in the table are slightly different.

Response: As you correctly pointed out, we found some inaccuracies in the roundings in the text compared to the numbers in the tables. We have thoroughly checked all the numbers and corrected the errors. As mentioned above we have added ethnicity as a covariate and changed the categories on the background variables 'familiy economy' and 'parents educational level'. . Thus, table 3 has been changed moderately, compared to the original version.

Please review the titles and labelling for the tables. For example the title for Table 4 is long and provides information not normally included in a title.

Response: We agree that the title of table 4 is quite long and have have decided to do major changes, as highlighted in the revised version.

You have reported that you have adjusted for socio-economic variables but you have not said what these variables are/how they are measured.

Response: The manuscript lack a thorough description of the sociodemographic background variables, as you correctly pointed out. This was also commented by the second reviewer. We have decided to add a paragraph in the ‘methods’ section, where the sociodemographic variables and how they were measured now are described in detail.

How have you dealt with missing data?

Response: Those pupils with missing values in some of the baseline independent variables, are not included in the final analyses. Since this is a community study with few missing values, we consider this appropriate and in line with common practice in similar studies.

There are big differences in findings for adjusted compared to unadjusted odds ratios. Has this data been closely checked?

Response: The differences between the unadjusted and adjusted odds have been thoroughly checked and are correct. In the unadjusted analyses the association between each health service and high-school dropout has been tested separately. In the adjusted model the health services have been adjusted for each other, the health indicators and for sociodemographics. Hence, we are not surprised that there are differences, since both sociodemographics and health have a strong influence on dropout. The isolated ‘effect’ of health service usage on dropout, is thus smaller in the adjusted model.

It would be useful to comment on this in the text and to consider the reasons. Please consider whether adjusted or unadjusted odds ratios are your focus and why, and then focus on one of these in the discussion.

Response: We consider both unadjusted and adjusted odds to be interesting. Our hypothesis is that those who use the health services at 15–16 are vulnerable, for a lot of reasons. We know that health problems lead to help seeking and have an influence on dropout. Sociodemographic background factors have a major influence on dropout. The unadjusted odds reflect these factors. In the adjusted odds, we have corrected for the effect health and sociodemographics have on dropout and found the crude ‘effect’ of health service use. In other words, can health service use still be a proxy for dropout after correcting for common reasons for help-seeking? In the revised version of the manuscript, we have chosen to focus mainly on the adjusted odds in the discussion, to emphazise this point.

Ethical approval is not detailed.

Response: We have supplemented with additional information in the ‘methods’ section concerning ethics.

Reviewer 2: Henrik Daae Zachrisson, PhD, The Norwegian Center for Child Behavioral Development, University of Oslo

At a general level, I struggle with understanding the scope of the research question and the design. This may be due to the introduction, which is quite brief. First of all, I am uncertain whether the authors address contact with health-services as a potential causal agent for drop out, whether they address this contact as a proxy for drop-out, or whether they seek a potential venue for prevention of drop out. The authors make the first suggestion explicit in the limitation section, where they state that no causality can be established. Furthermore, the design, which conditions on health-related and demographic characteristics, suggests that they want to isolate the unique effect of contact with

health services. If this is the case, the authors should provide a stronger argument for why this might be the case. It seems not intuitive to me that contact with health services should cause adolescents to dropout from high-school. It may be a reasonable hypothesis, please explain. If the authors are mainly interested in contact with health services as a proxy for other risk factors, I wonder why they do not study the effect of poor health and demographic characteristics, and use contact with health services as moderator (is this a marker of greater risk among adolescents with, e.g., poor health?) or a mediator (is contact with health services the mechanism that makes people with poor health drop out? – related to the previous point). Or they could compare adolescents with similar health profiles seeking- and not seeking help, to determine whether services influences on drop out for certain groups. If they are interested in health services as a venue for establishing contact with adolescents at risk for drop out (as suggested in the discussion), then it might be relevant to investigate for whom this should be considered. Given the high number of adolescents in contact with health services, it seems unrealistic to establish preventive actions for all. Under all circumstances, I struggle to see why conditioning on health is the way to address any of these questions. It makes an odd counterfactual to examine contact with health services regardless of health and demographics– as these are the main causes for contact with health services. At least I would like to see an explanation for the choice of design more closely related to the research question. In sum, an extended introduction which more clearly explains the rationale for the study and the design would improve the paper.

Response: In this study we aimed to examine associations between health service use and high-school dropout. The main hypothesis was that adolescent help seekers in general represent a vulnerable group, as lined out in the first paragraph of the introduction, especially those with multiple encounters. As stated in the discussion, we do not consider help seeking to cause dropout, as you correctly observed, rather that help seeking is a proxy for problems that can constitute risk factors for dropout. Hence, the health services can be an arena for prevention. This is not the main focus for our work, though, but we did address it as an issue in the section 'implications for practice and further research'. We agree in your opinions on prevention. It is unrealistic and not appropriate to approach all help seekers with preventive actions. The high school dropout percent is, however, quite high among those who encounter the youth health clinic and the child and adolescent mental health services. Nearly 50% in girls, while 71% of boys with frequent encounters to the youth health clinic drop out. Thus, more general preventive actions can be relevant for these groups. When conditioning on health and sociodemographic we aimed to neutralize the effects of these factors on dropout to see if health service usage still could be considered as a proxy for dropout, which we found for some of the services. We have chosen to extend the introduction to emphasize these points.

In the baseline questionnaire, the pupils were not asked to give reasons for their contacts with the different health care services. Methodologically, this gave us challenges. We considered to examine the effect of poor health and sociodemographics on dropout, with health service use as a moderator/mediator, as you suggest, but as long as no information is available on the direct reasons for help seeking, the results could be difficult to interpret. To compare adolescents with different health profiles seeking– and not seeking help, would be an alternative, and an interesting approach. Also here the challenge in not knowing the reason for help seeking, would provide problems when interpreting the results. We chose to investigate the 'effect' of health service use per se to focus on health service use mainly as a proxy for other factors that can influence on dropout. Hence, we emphasize marginalisation as a process, i.e can problems in the adolescence years be linked to high-school dropout and marginalisation in young adult life, which is the main focus of the research project in total. In the extended introduction, we have tried to describe the rationale of the study more thoroughly, as you asked for.

Another point related to the introduction is that it seems in one way to have a very local scope –as neither the contexts of the health-care system nor the high-school system are considered. These contexts are not familiar to non-norwegian readers. The authors do, however, some of this in the

discussion. I assume that there will be context specificity to such findings, and the research question may become clearer if this is laid out from the beginning.

Response: In the discussion we have some considerations about the local scope, as you correctly pointed out. The ‘methods’ section includes both a description of the Norwegian school system and the Norwegian health care services. We have added some supplementary information on this in the introduction to make it clearer for the foreign readers.

I also have some more detailed comments regarding design and statistics. Demographics are not described in the method section.

Response: We agree that the manuscript should have included a description of the sociodemographic background variables. We have added a thorough description in the ‘methods’ section, were the sociodemographic variables and how they were measured now are described in detail.

If, as I believe is the case, the authors have access to information about ethnicity, this should at the very least be included as covariate, but I would also consider it a potential moderator. Response: This study includes six counties, were five of them have a quite low percentage of ethnic minorities. Thus, we chose to exclude ethnicity as a covariate. After your comments we have decided to include it after all – this did not, however, change our main results. We will, explore ethnic differences in health service use, dropout and marginalisation in another paper in this project, a study the Oslo-part of the survey exclusively, where the number of ethnic minorities is much higher.

The authors may also think of considering low income families beyond poverty, as there may be a dose-response relationship both with health-service use and dropout.

Response: We are not quite sure what you mean when you use the term ‘dose-response’ in this context. However, since the families’ economical situation in this study is measured by self-report, with no information on actual income, the effect of low income can be difficult to explore in a proper way.

Furthermore, dummies for counties and actual student age should be considered as covariates.

Response: students had the same age when they participated in the survey and the follow-up time is equal for the whole group. Thus, it is not relevant to include actual student-age as a dummy variable. We did try to include county as a dummy, but found that it had no influence on our results, it did not change the odds significantly, a finding which supports our hypothesis. When we explored the study material descriptively, we found surprisingly small differences in health service use when we compared the six counties, including use of the mental health services. The dropout rates do, however, differ between the regions. Considering our research question and hypothesis, though, we do not believe that health service usage as a proxy for high-school dropout is influenced by differences in dropout rates.

I also think the authors should use a sandwich-estimator or multilevel design to account for nesting in schools, if this is available. A multi-level approach may be considered given that access to youth mental health services may way by region or school, and as students at some schools may have higher dropout rates than others.

Response: Information on schools is only available in the Oslo-material. Since we use a material from six different counties, we cannot examine nesting in schools, but this can be a relevant approach when working on the Oslo-material exclusively.

Also, the authors may find logistic regression inappropriate for making their interpretations given the prevalent outcome (see e.g., McNutt et al, 2003), as this may lead to overstating the interpretation of OR as relative risk (which I believe they in fact do).

Response: This is of course correct, and it was not our intention to interpret the odds ratios as relative risks in this work. We can see in the text that some of our expressions can be misunderstood in this direction. Therefore, we have thoroughly reread the text and changed all formulations that can be interpreted in this manner.

Finally, it was not immediately easy to understand what the parentheses in Table 2 are, please add explanation.

Response: We agree that table 2 is unclear and not immediately easy to understand. The first reviewer also made comments on table 2. We have decided to make major changes to improve the quality of the table.
As we have added ethnicity as a covariate and changed the categories of the background variables 'familiy economy' and 'parents educational level', both table 3 and 4 has changed from the original versions. It does not, however, affect our main results.

VERSION 2 – REVIEW

REVIEWER	Theresa Fleming Lecturer Department of Paediatrics: Child and Youth Health & Department of Psychological Medicine The University of Auckland New Zealand No competing interests
REVIEW RETURNED	27-Jul-2013

RESULTS & CONCLUSIONS	I have one point re interpretation of results which I believe should be addressed: -Page 13, The association between attendance at school based clinics and drop out in the unadjusted model is rightly pointed out, however the finding that moderate attendance at school clinics was protective for girls once covariates had been considered (at p smaller than or equal to .05), is not commented on. In contrast, a finding at this same significance level for youth health clinics is commented on. The authors should either set a higher p value or report all of the significant findings at this level. Should they do the latter, this would require the addition of a sentence or two on page 13 and corresponding brief additions to the discussion, abstract and perhaps key points. In my view, the message that these various clinics see young people with elevated risk of drop out and hence could and perhaps should address the issue of school retention among their (frequent) attendees is touched on appropriately in the abstract, but is not made very clearly in the implications section. This is not a critical flaw, but should the authors be asked to make minor corrections, they could perhaps address this.
GENERAL COMMENTS	This article is much improved, the issues raised by the reviewers appear to have been addressed and the article makes a useful contribution to the literature. I believe it is appropriate for it to be

	published. There is one issue I believe should be addressed before the article is finalised and several very minor points may be considered as below: -Page 13, The association between attendance at school based clinics and drop out in the unadjusted model is rightly pointed out, however the finding that moderate attendance at school clinics was protective for girls once covariates had been considered (at p smaller than or equal to .05), is not commented on. In contrast, a finding at this same significance level for youth health clinics is commented on. The authors should either set a higher p value or report all of the significant findings at this level. Should they do the latter, this would require the addition of a sentence or two on page 13 and corresponding brief additions to the discussion, abstract and perhaps key points. I would like to note the following very minor points: -Key messages page 2, line 27-29: all of the groups of adolescents had some risk of becoming drop outs. This message could perhaps be better worded as … 'have slightly increased risk'… or 'have slightly elevated rates'… or such. - There are places where the authors slightly diverge from the focus of the article. I don't think it is essential to change these but if they are asked to make a second set of changes to the paper these may be considered: a) Paragraph two, page 6 adds little - most these points are raised in the previous paragraph b) The second half of paragraph one on page 18 (line 15-22) slightly goes beyond the results - I assume the authors mean 'context' rather than 'content' page 18 line 32 - The use of the word 'dilatation' (page 18 line 38) is unusual in this context in English; 'divergence' would be more common.

VERSION 2 – AUTHOR RESPONSE

Reviewer 1: Theresa Fleming
University of Auckland

This article is much improved, the issues raised by the reviewers appear to have been addressed and the article makes a useful contribution to the literature. I believe it is appropriate for it to be published. There is one issue I believe should be addressed before the article is finalised and several very minor points may be considered as below:

-Page 13, The association between attendance at school based clinics and drop out in the unadjusted model is rightly pointed out, however the finding that moderate attendance at school clinics was protective for girls once covariates had been considered (at p smaller than or equal to .05), is not commented on. In contrast, a finding at this same significance level for youth health clinics is commented on. The authors should either set a higher p value or report all of the significant findings at this level. Should they do the latter, this would require the addition of a sentence or two on page 13 and corresponding brief additions to the discussion, abstract and perhaps key points.

Response: We have decided to keep the original significance level ($p \leq 0.05$) and also consider the significant finding on girls visiting the school health clinics 1-3 times, a result which you correctly asked us to comment on. This has lead to some minor changes in the article summary, the abstract and in the results section. We have also added an extra paragraph in the discussion, commenting on the result. All changes are highlighted in red. In addition, the result is also included in the information box at the end of the manuscript.

I would like to note the following very minor points:
-Key messages page 2, line 27-29: all of the groups of adolescents had some risk of becoming drop outs. This message could perhaps be better worded as … ‘have slightly increased risk’… or ‘have slightly elevated rates’… or such.

Response: We have made minor changes to the ‘key messages’ paragraph, in which we have decided to focus on the results from the adjusted model.

- There are places where the authors slightly diverge from the focus of the article. I don't think it is essential to change these but if they are asked to make a second set of changes to the paper these may be considered:
a) Paragraph two, page 6 adds little - most these points are raised in the previous paragraph

Response: We have chosen to keep paragraph two, page 6, since reviewer 2 in the major revison of the manuscript asked for an extended introduction with a more thorough explanation of the rationale of the study. We believe that the paragraph contributes to this.

b) The second half of paragraph one on page 18 (line 15-22) slightly goes beyond the results
- I assume the authors mean 'context' rather than 'content' page 18 line 32
- The use of the word ‘dilatation’ (page 18 line 38) is unusual in this context in English; ‘divergence’ would be more common.

Response: We have made a minor change in paragraph one on page 18, ‘clearly shows’ is changed to ‘may suggest’, which we consider to be more in line with our results.

We have changed the word ‘content’ to ‘the quality of their treatment’. We have also chosen to change the word ‘dilatation’ to ‘divergence’.