PEER REVIEW HISTORY

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ARTICLE DETAILS

<table>
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<th>TITLE (PROVISIONAL)</th>
<th>A prospective cohort study of the changing mental health needs of adolescents in custody</th>
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<tr>
<td>AUTHORS</td>
<td>Lennox, Charlotte; Bell, Vicky; O'Malley, Kate; Shaw, Jenny; Dolan, Mairead</td>
</tr>
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VERSION 1 - REVIEW

| REVIEWER             | Anna Kissell  
|                     | Research Assistant  
|                     | Institute of Psychological Medicine and Clinical Neurosciences  
|                     | Cardiff University  
|                     | UK  
| REVIEW RETURNED     | 15-Jan-2013 |

THE STUDY

1. It is not absolutely clear from the manuscript to what extent the sample is representative of adolescents in custody across the UK or beyond. Institutions vary in the facilities and environment they provide and their populations may be different. A statement about how the YOI under study was broadly representative of others would provide a rationale for generalising the results.

RESULTS & CONCLUSIONS

1. Abstract: ‘Over time there was general improvement in mental health’. This needs re-writing with reference to how it was measured, or if word count doesn't allow, expressed more precisely.

2. Abstract: ‘Prison does not have a universally detrimental effect on the mental health of young people’. This is too general a statement and needs re-drafting in line with the scope of the study which focuses on acute symptoms and illness related needs. In another study of young prisoners, Brown and Ireland (2006) suggest that while mood states may improve, psychological coping styles may change in a way that harms long-term development.

3. Page7, line 6: ‘We predicted, based on previous research, that many needs would be met following incarceration.’ Is this hypothesis testable? Are the researchers measuring whether needs have been met or simply whether needs are present at each time point? Given that interventions are not being measured and participants are not asked whether they have received help that would constitute ‘met needs’, I am unclear about this. In my opinion this hypothesis needs to be more specific as in the second line regarding ‘caseness’ decreasing over the three time points. Suggest separating hypotheses something like this:

   a. Mental health and other needs, as assessed by the Salford Needs Assessment Schedule for Adolescents (SNASA), will decrease over time.

   b. ‘Caseness’, as assessed by Kiddie Schedule for Affective
Disorders and Schizophrenia (K-SADS), will decrease over time.

4. Page 12, line 17: It looks as though PTSD may have been omitted from the list of diagnoses mentioned at the six month results.

5. Might table 2 be better presented as a bar chart to display the results in a more visual way?

6. Conclusions made on the last line of page 14 and Page 15, line 4-6:

‘This suggests that services are generally meeting the mental health needs of these young people...’ As the authors point out elsewhere, without measuring some aspect of the intervention of services, it is difficult to see how this conclusion can be drawn. It also appears to contradict the tone of the end of discussion (page 15, last line) which states: ‘these findings highlight the scale of the challenges faced by those providing health care to young people in detention and underlines the need to address their health problems.’

It is possible that young people’s needs diminish over time as they become settled in a secure environment, even without access to relevant interventions. This is conceivably more likely in the domains of education, risky behaviour and relationships, the areas which young people’s needs reduced significantly over time. For example substances are much less available in prison and prisoners are away from their usual social context. These needs may well reoccur later in the transition back to the community. There are other factors that may influence recovery in the early stages of imprisonment, as noted above with reference to Brown and Ireland’s work, as well as for example family/professional support, access to visits, employment, prison programs, etc. These and others factors need some consideration and discussion as confounding factors in the reduction of mental health symptoms and needs.

**GENERAL COMMENTS**

This is a well designed longitudinal study which has managed to retain a good number of subjects over the duration of the study given the difficulty of the research setting. The authors have given a strong rationale for the research and used standardised measures which relate to previous research. I have a query about the testability of the first hypothesis and the authors need to be careful of overstating the findings beyond the scope of the study (see the specific points noted).

**REVIEWER**

Tina Maschi, PhD, LCSW, ACSW  
Associate Professor  
Fordham University Graduate School of Social Service  
New York, New York  
United States

**REVIEW RETURNED**

26-Jan-2013

**THE STUDY**

The authors do mention that the majority is mostly white, all male, and there was a high attrition rate. It is recommended that all of these factors should be addressed in the limitations of the design. The authors also note that the study does not account for possible treatment effects while in prison. Therefore, it is also recommended that future directions for research should also examine what treatments might be helpful while in prison. This has important implications for prison rehabilitation efforts.
| GENERAL COMMENTS | I commend the authors for this prospective study on youth's mental health and comorbid issues in prison. Pending page or word limitations, there are a few areas where additional information and/or clarification of information would further bring out important issues raised by the study.  

1) The authors report that the majority is mostly white, all male, and that there was a high attrition rate in the sample. Therefore, in the limitations section, also adding that the inferences drawn may differ by race (and gender).  

2) In the findings section on the longitudinal mental health effects, it is unclear if improvement would continue over time especially for those youth serving longer sentences. In light of the research coming out on the physical and mental health effects of long term confinement. Therefore, simply noting that the temporal effects found in this study may or may not sustain over time. This limitation then suggests the future research recommendations that longitudinal studies should examine longer durations of incarceration.  

3) The issue of attrition, is another limitation as noted by the authors. If any information is available as to the reasons for the attrition. The authors might consider an analysis of significant differences of available baseline data of differences in groups (attrition vs. non-attrition sample) and brief discussion. In some prison studies, participants are dropped out of a study because they are placed in solitary confinement or administrative segregation. Regardless of the reason for attrition, a brief mention that the inclusion of the full sample might have changed the results.  

4) What I found the most interesting about this study is that it provides evidence that some youth show improvement in multiple domains of functioning in a secure care setting. Therefore, the argument for rehabilitation (over punishment) can be briefly discussed, even if it is in the future research recommendations section recommending that future studies should examine what are the therapeutic or treatment effects of a prison setting that foster health and well-being and rehabilitation among youth in prison.  

5) Additional recommendations for future studies that should be mentioned is to examine racial/ethnic difference, gender, age and differing prison settings (minimum to maximum) prison settings, and geographic locations, including different countries.  

6) Another recommendation for future research is to explore the role of access to formal resources (prison programming and staff to inmate ratios) as well as informal resources, such as level of family contact and community (continued contact with schools or spiritual on improved outcomes of youth in care. |

| REVIEWER | Paul Mitchell  
Clinical Lead, Hindley YOI mental health team  
c/o GMW NHS Trust  
Bury New Rd  
Manchester |

| REVIEW RETURNED | 30-Jan-2013 |

| GENERAL COMMENTS | This paper addresses a significant gap in the knowledge base |
regarding the mental health needs of young people in the secure estate and justice system; there have been a number of prevalence studies but very little is known about how young people’s mental health needs and risks change during the first few (and often crucial) weeks in custody. This issue has important implications for screening policy and models of service delivery.

There are two specific points arising from the text;
Page 9 – SNASA is used as one of the tools. Were the scores based only on problem severity rated during the interview? There is an algorithm that also factors in other scores such as subjective rating of problem severity, motivation to change, and carer distress. It would appear that the algorithm was not used, but it should be made clear.
Page 10 – “caseness” for the study is based on scores for depression and psychosis; other areas of mental health need covered by both K-SADS and SNASA are not used, particularly PTSD and ADHD. The rationale for excluding other areas of need should be made clear. Alternatively (and this could greatly strengthen the paper) other mental health needs should also be incorporated if the data are available.

General comments
These relate to the discussion section;
The attrition rate impacts on the power of the study, but the caseness should be described and discussed in more detail (even though the findings may not achieve statistical significance) because it may have implications for service delivery. Specifically, which mental health needs persisted over time and which did not, and what were the needs that increased and achieved caseness over the course of the study.
Also, some discussion of the correlation (or lack of) between SNASA and K-SADS caseness would be helpful, as would discussion on why SNASA caseness is higher.
Finally, the discussion should highlight areas for further study;
• Widening the range to include other mental health needs including PTSD and ADHD
• Increasing the size to take the likely attrition rate into account
• Looking at the impact of specific interventions or service contact on the persistence of caseness

VERSION 1 – AUTHOR RESPONSE

Reviewer: Anna Kissell
1. It is not absolutely clear from the manuscript to what extent the sample is representative of adolescents in custody across the UK or beyond. Institutions vary in the facilities and environment they provide and their populations may be different. A statement about how the YOI under study was broadly representative of others would provide a rationale for generalising the results.

In response to this point and those raised by Tina Maschi we have added more information to the discussion section to help the reader better understand the generalisability of the sample. Recommendations for future research have also been included.

2. Abstract: ‘Over time there was general improvement in mental health’. This needs re-writing with reference to how it was measured, or if word count doesn't allow, expressed more precisely.

We have expanded on this point and included how it was measured.
3. Abstract: ‘Prison does not have a universally detrimental effect on the mental health of young people’. This is too general a statement and needs re-drafting in line with the scope of the study which focuses on acute symptoms and illness related needs. In another study of young prisoners, Brown and Ireland (2006) suggest that while mood states may improve, psychological coping styles may change in a way that harms long-term development.

This statement has been removed and this section of the abstract redrafted to reflect other reviewers' comments.

4. Page 7, line 6: ‘We predicted, based on previous research, that many needs would be met following incarceration.’ Is this hypothesis testable? Are the researchers measuring whether needs have been met or simply whether needs are present at each time point? Given that interventions are not being measured and participants are not asked whether they have received help that would constitute ‘met needs’, I am unclear about this. In my opinion this hypothesis needs to be more specific as in the second line regarding ‘caseness’ decreasing over the three time points. Suggest separating hypotheses something like this:

a. Mental health and other needs, as assessed by the Salford Needs Assessment Schedule for Adolescents (SNASA), will decrease over time.
b. ‘Caseness’, as assessed by Kiddie Schedule for Affective Disorders and Schizophrenia (K-SADS), will decrease over time.

We have changed the hypotheses as suggested.

5. Page 12, line 17: It looks as though PTSD may have been omitted from the list of diagnoses mentioned at the six month results.

None of the young people had a diagnosis of PTSD at six months. This has been included.

6. Might table 2 be better presented as a bar chart to display the results in a more visual way?

Table 2 is now presented as a bar chart.

7. Conclusions made on the last line of page 14 and Page 15, line 4-6:

‘This suggests that services are generally meeting the mental health needs of these young people...’

As the authors point out elsewhere, without measuring some aspect of the intervention of services, it is difficult to see how this conclusion can be drawn. It also appears to contradict the tone of the end of discussion (page 15, last line) which states: ‘these findings highlight the scale of the challenges faced by those providing health care to young people in detention and underlines the need to address their health problems.’

It is possible that young people’s needs diminish over time as they become settled in a secure environment, even without access to relevant interventions. This is conceivably more likely in the domains of education, risky behaviour and relationships, the areas which young people’s needs reduced significantly over time. For example substances are much less available in prison and prisoners are away from their usual social context. These needs may well reoccur later in the transition back to the community. There are other factors that may influence recovery in the early stages of imprisonment, as noted above with reference to Brown and Ireland’s work, as well as for example family/professional support, access to visits, employment, prison programs, etc. These and others factors need some consideration and discussion as confounding factors in the reduction of
mental health symptoms and needs.

We have changed the discussion/conclusion to reflect these comments.

This is a well designed longitudinal study which has managed to retain a good number of subjects over the duration of the study given the difficulty of the research setting. The authors have given a strong rationale for the research and used standardised measures which relate to previous research. I have a query about the testability of the first hypothesis and the authors need to be careful of overstating the findings beyond the scope of the study (see the specific points noted).

Reviewer: Tina Maschi, PhD, LCSW, ACSW

The authors do mention that the majority is mostly white, all male, and there was a high attrition rate. It is recommended that all of these factors should be addressed in the limitations of the design. The authors also note that the study does not account for possible treatment effects while in prison. Therefore, it is also recommended that future directions for research should also examine what treatments might be helpful while in prison. This has important implications for prison rehabilitation efforts.

I commend the authors for this prospective study on youth's mental health and comorbid issues in prison. Pending page or word limitations, there are a few areas where additional information and/or clarification of information would further bring out important issues raised by the study.

1. The authors report that the majority is mostly white, all male, and that there was a high attrition rate in the sample. Therefore, in the limitations section, also adding that the inferences drawn may differ by race (and gender).

We have added this to the limitation section and highlighted future research.

2. In the findings section on the longitudinal mental health effects, it is unclear if improvement would continue over time especially for those youth serving longer sentences. In light of the research coming out on the physical and mental health effects of long term confinement. Therefore, simply noting that the temporal effects found in this study may or may not sustain over time. This limitation then suggests the future research recommendations that longitudinal studies should examine longer durations of incarceration.

We have added this to the limitation section and highlighted future research.

3. The issue of attrition is another limitation as noted by the authors. If any information is available as to the reasons for the attrition. The authors might consider an analysis of significant differences of available baseline data of differences in groups (attrition vs. non-attrition sample) and brief discussion. In some prison studies, participants are dropped out of a study because they are placed in solitary confinement or administrative segregation. Regardless of the reason for attrition, a brief mention that the inclusion of the full sample might have changed the results.

Figure 1 shows the reasons for attrition and Table 1 shows a comparison of baseline data for those retained at follow-up vs. those lost to follow-up. There were no significant differences between the groups for demographic characteristics or mental health need at baseline.

4. What I found the most interesting about this study is that it provides evidence that some youth show improvement in multiple domains of functioning in a secure care setting. Therefore, the argument for rehabilitation (over punishment) can be briefly discussed, even if it is in the future
research recommendations section recommending that future studies should examine what are the therapeutic or treatment effects of a prison setting that foster health and well-being and rehabilitation among youth in prison.

We have included this in the recommendations for future studies.

5. Additional recommendations for future studies that should be mentioned is to examine racial/ethnic difference, gender, age and differing prison settings (minimum to maximum) prison settings, and geographic locations, including different countries.

All of the additional recommendations have been addressed.

6. Another recommendation for future research is to explore the role of access to formal resources (prison programming and staff to inmate ratios) as well as informal resources, such as level of family contact and community (continued contact with schools or spiritual) on improved outcomes of youth in care.

This has been included.

Thank you for the opportunity to review this manuscript. I hope the authors find this feedback helpful.

Reviewer: Paul Mitchell

This paper addresses a significant gap in the knowledge base regarding the mental health needs of young people in the secure estate and justice system; there have been a number of prevalence studies but very little is known about how young people’s mental health needs and risks change during the first few (and often crucial) weeks in custody. This issue has important implications for screening policy and models of service delivery.

There are two specific points arising from the text;

1. Page 9 – SNASA is used as one of the tools. Were the scores based only on problem severity rated during the interview? There is an algorithm that also factors in other scores such as subjective rating of problem severity, motivation to change, and carer distress. It would appear that the algorithm was not used, but it should be made clear.

The research team decided in this study to base the scores on problem severity rated by the young person during the interview, mainly due to difficulties in obtaining information from carers. We have included in the methods section details of the algorithm but made it clear it was not used in this study.

2. Page 10 – “caseness” for the study is based on scores for depression and psychosis; other areas of mental health need covered by both K-SADS and SNASA are not used, particularly PTSD and ADHD. The rationale for excluding other areas of need should be made clear. Alternatively (and this could greatly strengthen the paper) other mental health needs should also be incorporated if the data are available.

Caseness for the SNASA used the mental health domain which includes depressed mood, deliberate self-harm, anxiety symptoms, post-traumatic stress problems, hallucinations, delusions, paranoid beliefs, and hyperactivity. Caseness for the K-SADS just included depression and psychosis. The caseness for the K-SADS has been redone to include PTSD, anxiety disorders and ADHD.

General comments
These relate to the discussion section;

3. The attrition rate impacts on the power of the study, but the caseness should be described and discussed in more detail (even though the findings may not achieve statistical significance) because it may have implications for service delivery. Specifically, which mental health needs persisted over time and which did not, and what were the needs that increased and achieved caseness over the course of the study.

More detail has been added about caseness and the implications for service delivery.

4. Also, some discussion of the correlation (or lack of) between SNASA and K-SADS caseness would be helpful, as would discussion on why SNASA caseness is higher.

We have highlighted the reasons for more fluctuation and higher rates of caseness using the SNASA.

5. Finally, the discussion should highlight areas for further study; widening the range to include other mental health needs including PTSD and ADHD; increasing the size to take the likely attrition rate into account; looking at the impact of specific interventions or service contact on the persistence of caseness.

We have included other mental health in the caseness for the K-SADS. All other areas for future study have been included.