Author's response to reviews

Title: Changes in mental health services and suicide mortality in Norway: an ecological study

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Author's response to reviews: see over
Dear Dr Christopher B. Forrest,

We are pleased that all of the reviewers evaluated the paper as important in its field, and we are indeed grateful for the excellent and comprehensive recommendations given by you and the reviewers. We have now made substantial changes in the manuscript according to the recommendations. Consequently, we think the manuscript has improved considerably.

A point-by-point response to the editor’s and the reviewer’s comments follows:

**RM** refers to Revised Manuscript. All changes in the revised manuscript are written in red.

**Editor: Dr Christopher B. Forrest**
1) Add a brief paragraph on the financing and organization of the Norwegian health system

This is done in the **RM on p 4, second paragraph**

2) Please expand your discussion on competing hypotheses and the role of a variety of confounders in explaining your results

We have now rewritten and expanded the discussion section, and highlighted that: “The present study should be interpreted with caution because statistical associations can be masked by the fact that we have failed to adjust for relevant confounders” (**RM, pp 11-15**).

3) In the Bickley review, a few references are suggested to addition to the manuscript. These references were already included in the submitted manuscript on pp 4 and 5. However, we have now expanded the discussion section by referring to several studies designed similar to ours (**RM, p 13; second paragraph**).

4) Consider reanalyses for males and females

We have now computed separate analyses for males and females (**RM, pp 10 and 11; Tables 3 and 4**)

5) Consider altering the strength of the conclusion that there is no association. One explanation for the negative findings is inadequate statistical adjustment. Another is the weak outpatient measures.

We agree that the conclusion should not give the impression that it was no ‘causal impact’ of increased health services resources on suicide mortality. However, what we did find was no statistical association between increased resources and suicide mortality in the adjusted
analyses. We have changed the conclusions accordingly and highlighted uncertainty by stating that our adjusted analyses ‘indicate’ (RM, pp 3 and 15).

6) Add a table describing the five regions as suggested by Wahlbeck’s review

We have now added a table with descriptive regional figures (RM, Table 2).

7) Enhance the discussion on variable definition

We have now highlighted in the discussion section that we had no measures on whether more individuals were successfully treated, rather we present quantitative measures on increased resources and of more treated individuals. We have also highlighted that we only had one direct measure of outpatient services. (RM, p 12 third paragraph; p 13 third paragraph).

8) Please also address the Statistician’s points about data, statistical model, and collinearity.

We have addressed these issues below:

**Reviewer: Dr. Girdhar Agarwal**

1) Dr. Agarwal wrote: ”Quality of data : The regional figures are available from 1998 onwards. For period before 1998, figures for the whole country are used. Why are not they doing the study for the period 1998-2006, when the actual data are available for the five regions in question. Otherwise they should do the study for the whole country for the period 1990-2006. For year 1998 data, the average of figures for 1997 and 1999. All these factors will have bearings on the analysis.”

We agree with Dr. Agarwal that it would have strengthened the paper if regional figures for the health services variables had been available for the whole period. In order to gain sufficient statistical power (Norway has 5 000 000 million inhabitants and about 500 suicides per year) we chose to analyse the whole period by imputing national figures for the period 1990-1997. We consider the computed regional model to be stronger than a national model because it makes us able to adjust for regional differences and it increases the variability in the data set. However, as Dr. Agarwal points out, this will have bearings on the analysis. Therefore, we have now in addition computed a model with complete national data for the period 1990-2006 to check if the results changed (RM, p 8 fourth paragraph; p 11 second paragraph). We will include this table in the manuscript if the editor finds it necessary.

2) Dr. Agarwal wrote: “Choice of Model I am not cleat about the use of Poisson regression model. Why are not they using “Suicide mortality rate” as outcome variable (Y) and use logistic regression model. In order to use Poisson distribution, they have to check its assumptions, namely mean, variance and cumulants of Y should be approximately equal. In the case of failure of the Poisson assumptions, the negative binomial distribution will be a reasonable alternative model.”

We agree with Dr. Agarwal that test statistics of model fit should have been presented in the manuscript. The negative binomial model is an alternative model to the Poisson model when over-dispersion is a problem. By comparing the deviance, that is, a measure of the discrepancy between observed and fitted values with its degrees of freedom, we obtained
goodness of fit measures. The test statistics of the computed models showed good fit and no signs of over-dispersion. We have now presented the test statistics in Tables 3 and 4 in the RM. Further, we have enhanced the method section (RM, p 8; third paragraph)

3) Dr. Agarwal wrote: Choice of explanatory variables I am sceptical about the choice of “five mental health services variables” (no. of man-labor years, no. of discharges,...etc.). They are studying the impact of mental health services in the improvement of suicide rate in the period 1990-2006. The choice of variables should reflect this fact, e.g. the selected health services should be those, which were not available prior to the year 1990 or the ones which reflect the changes (e.g. increase or decrease in no. of man-labor years etc.)

We are not sure what Dr. Agarwal implies by the statements above. We have chosen all the services variables that were available to us, and these variables show annual changes throughout the whole study period. Prior to 1990, these variables were not available.

4) Dr. Agarwal wrote: “Time should be a covariate as the incidence rate might be changing with time.”

We agree, and time is now added as a covariate (RM, Tables 3, 4 and 5).

5) Dr. Agarwal wrote: “Collinearity (p. 5) Collinearity among explanatory variables is checked by finding the correlations between them. This is not done. If collinearity is present, what measures were taken to handle it?”

To rule out the possibility of a collinearity problem, we computed an adjusted model in which each health services variable was analysed separately. We have enhanced the method section (RM, p 9; first paragraph) and result section (RM, p 11; first paragraph). We have not included this table in the manuscript. However, we will be happy to do it if the editor finds it necessary.

Separate analyses for each health services variable adjusted for annual trends, health region, sales of alcohol, unemployment and education

<table>
<thead>
<tr>
<th>Variables</th>
<th>Women IRR</th>
<th>P-value</th>
<th>Model fit</th>
<th>Men IRR</th>
<th>P-value</th>
<th>Model fit</th>
</tr>
</thead>
<tbody>
<tr>
<td>Man-labour years</td>
<td>0.96</td>
<td>P = 0.72</td>
<td>P = 0.14</td>
<td>1.00</td>
<td>P = 0.99</td>
<td>P = 0.35</td>
</tr>
<tr>
<td>Discharges</td>
<td>1.03</td>
<td>P = 0.41</td>
<td>P = 0.15</td>
<td>1.00</td>
<td>P = 0.98</td>
<td>P = 0.35</td>
</tr>
<tr>
<td>Beds</td>
<td>0.97</td>
<td>P = 0.90</td>
<td>P = 0.14</td>
<td>1.24</td>
<td>P = 0.08</td>
<td>P = 0.45</td>
</tr>
<tr>
<td>Bed-days</td>
<td>1.00</td>
<td>P = 0.95</td>
<td>P = 0.14</td>
<td>1.00</td>
<td>P = 0.12</td>
<td>P = 0.43</td>
</tr>
<tr>
<td>Outpatient consul.</td>
<td>1.00</td>
<td>P = 0.88</td>
<td>P = 0.14</td>
<td>1.00</td>
<td>P = 0.92</td>
<td>P = 0.35</td>
</tr>
</tbody>
</table>
6) Dr. Agarwal wrote: “Result In this section crude RR is expressed in fractions as well as in percentage. Only one format should be used. The suicide rates decline mostly from 1990 to 1994 and levelled off after that. In contrast, the major changes in health services occur during the period 2000-2006. Obviously, there is no association between suicide rates and improved health services. Hence, there is no need to fit any kind of models. In this situation, subgroup analyses should be performed for the periods (i) 1990-1994, (ii) 1995-1999 and (iii) 2000-2006.”

We have now chosen the format of percentages.

We agree with Dr. Agarwal that the crude figures strongly indicate no association between increased resources in mental health services and suicide mortality. However, suicide is a multi-factorial phenomenon. Hence, it is possible that other factors associated with suicide, e.g. alcohol consumption, have increased substantially in the study period and thereby out-weighted the effect of increased services resources. The rationale for fitting a multivariate model is to adjust for this possibility.

As suggested by Dr. Agarwal, we fitted a multivariate model with period (i) 1990-1994, (ii) 1995-1998 and (iii) 1999-2006 as dummy-variables. This model did not alter the results.

7) Dr. Agarwal wrote: “No significant association is found between the suicide mortality and the five mental health services variables. These might be due to the facts mentioned above (Comments 1-5).”

We have now reanalysed all data and actively searched for possible effects of the health services variables:

- We have fitted models separated by gender with complete data at the national level (the only missing figure was sales of alcohol in the year 1998).
- We have fitted models separated by gender with regional data.
- We have fitted models with time as a continuous covariate and as a dummy-variable.
- We have handled the possible problem of collinearity as described above.

8) Dr. Agarwal wrote: “The significant association is found between outcome variable and sales of alcohol, unemployment and education. For these variables, confidence intervals are given. These should be accompanied by exact p-values. In my view, these associations will be marginal since the most of the limits of confidence intervals are close to 1.0. Again these significance might be superfluous in view of comments 1-5.”

We have now accompanied the confidence intervals with exact p-values. By fitting models separated by gender, these variables turned out to be more robust.

Reviewer: Dr. Kristian Wahlbeck

1) Dr. Wahlbeck wrote: “The background section is succinct and clear, but for the international reader some more information is needed about the Norwegian context.
The strengthening of mental health resources is shortly mentioned in the third paragraph, but authors should be more specific regarding how the new resources were allocated in the mental health care system. A note on the size of the relative increase of the national mental health care budget would be helpful. They should also provide a concise description on how the competing explanatory variables, i.e. alcohol use, unemployment, education level and sales of antidepressants, have developed in Norway during the observation period.

We have now explained the financing and organisation of Norwegian health services (RM, p 4, second paragraph). Further, we have explained how the new resources were allocated and given the relative figure of the real growth in expenditures (RM, p 5, second paragraph). A concise description on how the competing explanatory variables changed during the period is now shown in RM Table 1 and described on p 9, first paragraph.

2) Dr. Wahlbeck wrote: “As the analyses are based on regions, a table presenting the five regions, their trends in suicide mortality, the development of mental health services variables in each region, and the trends in explanatory variables for each region should be provided. The results of the Poisson regression analysis are difficult to interpret unless such background data is presented.”

This is now done (RM, Table 2)

3) Dr. Wahlbeck wrote: “The results are highly dependent on the choice of explanatory factors, and the authors should be careful in choosing the relevant characteristics of mental health services. The negative outcome of the study may reflect the authors’ choice of mostly hospital-related explanatory variables. The rationale for the choice of mental health services variables used thus needs to be given.”

We have chosen all the health services variables that were available. Three of the health services variables measure the increased investments in mental health services. These are ‘the number of discharges’, ‘the number of outpatient consultations’ and ‘the number of man labour-years’ (both in hospitals and outpatient services). In addition, we chose to include the variables ‘the number of beds’ and ‘the number of bed-days’. The rationale is to control for unwanted consequences of the policy of downsizing psychiatric hospitals. Several researchers have pointed out that shortened length of stays and fewer hospital beds may have had unwanted consequences regarding suicide. Hence, we have not excluded any available services variables, and we believe that including variables that can be used to investigate if the process of reducing psychiatric hospital beds had any unwanted consequences actually strengthens the paper.

We have highlighted the limitations pointed out by Dr. Wahlbeck in the RM on p 12, third paragraph.

4) Dr. Wahlbeck wrote: “The variables used should be more carefully described. It is unclear how “the length of inpatient stays in days per 1000 inhabitants” was calculated. It seems to be the total number of inpatient days, and not the average length of in-patient stay. If so, the naming of the variable needs to be changed, and it would be highly correlated to “the number of psychiatric hospital beds per 1000 inhabitants”. Inclusion of both of these variables is questionable (as they both simply reflect volume of in-patient care).”
The number of bed-days is calculated by subtracting the patient’s date of discharge from the date of hospital admittance, that is, the number of days a patient remains in hospital (RM, p 7). We have now changed the naming to “the number of inpatient-days per 1000 inhabitants”.

The number of beds is a calculation of accessible beds in the institution as of 31 December each year. In contrast to bed-days, the number of beds is a measure of the institutions’ treatment capacity (RM, p 7).

5) **Dr. Wahlbeck wrote:** Does “number of outpatient consultations” include consultations by all staff? Does it include mental health consultations in primary care and municipal services?

Outpatient consultations include mainly treatment by psychiatrists or clinical psychologists in secondary mental health services.

6) **Dr. Wahlbeck wrote:** Psychiatrists constitute only a part of the workforce in mental health services, and probable the total number of labour-years (including psychologists, nurses, etc) is more relevant for prevention of suicides. If these data are available, authors should include total labour years of all staff in mental health services instead of psychiatrists’ labour years, as their aim is to look at the effect of increased mental health resources, not just the effect of increasing the number of psychiatrists. They might even consider including the number of workforce in outpatient care in the analysis, if such data are available.

We agree and have now included a variable that measures “man labour-years by all personnel” in mental health services (RM, p 6; fifth paragraph).

7) **Dr. Wahlbeck wrote:** The authors state that “Regional figures were available from 1998 onwards. For the period before 1998, figures for the whole country were used.” From this description it is not clear how the authors handled the missing data. Did they substitute missing regional data with aggregate national data? This would reduce the impact of region in the log-linear model.

We did substitute missing aggregated data at the regional level with aggregated national data. This has been clarified in the RM, p 8; fourth paragraph.

8) **Dr. Wahlbeck wrote:** It is also not clear whether regional data were available for each of the competing explanatory variables (regional data is not mentioned for alcohol and antidepressant sales). Authors need to more carefully describe their data set.

We have now changed the text in the method section as follows: “To adjust for competing explanatory variables, we used regional data on education and unemployment; and national data on sales of alcohol and sales of antidepressants, all of which have been reported to be associated with suicide mortality” (RM, p 7, third paragraph).
9) **Dr. Wahlbeck wrote:** “Poverty and deprivation are strongly linked to suicides, but the authors have not included poverty or mean income level among the competing explanatory variables. Is there a rationale for this?”

Poverty is rare in Norway. Therefore, we consider unemployment to be a more suitable variable.

10) **Dr. Wahlbeck wrote:** “Collinearity is the crucial issue in the statistical analysis. Authors should describe more carefully how the dealt with this. What does it mean that they “compared the standard errors of...the [models]?”

We have answered this question above.

11) **Dr. Wahlbeck wrote:** “The authors should be more careful in avoiding wording that implies causality. In the first para, they state that “no impact on suicidality mortality was found”. As this ecological study has studied associations, words such as “impact” should be avoided.”

We have changed the text as requested (RM, p 11, third paragraph)

12) **Dr. Wahlbeck wrote:** “The discussion focuses on content and quality of treatment given, which is an important topic and may explain the negative finding. However, not just content but also organisation of services deserves to be discussed. Have accessible and varied out-patient interdisciplinary and psychotherapy services been developed (which in previous studies have been linked to reduced suicides) or does the focus remain on standard medical out-patient services?”

In the period that we have studied, standard psychiatric mental health treatment has been the focus in the outpatient services. To the best of our knowledge, there is no outpatient services that is specialised in handling suicidal individuals. We have added the following text to the discussion section of the manuscript:

“In addition, there has not been developed varied interdisciplinary outpatient services that are specialised in handling suicidal patients” (RM, p 13; third paragraph).

13) **Dr. Wahlbeck wrote:** “During the period of increase of resources of mental health services, suicide rates declined. In the Poisson regression all of the decline was explained by the competing explanatory factors. However, it cannot be excluded that this was not a result of including mostly in-patient service variables in the analysis. The authors should discuss the impact of their choice of variables in this section.”

A part of the variation in suicide mortality was explained by the competing explanatory variables. Most of the variation remained unexplained.

The aim of the study was to investigate if increased resources in mental health services were associated with suicide mortality. Increased sales of antidepressants and increased number of man-labour years is proxy measures of increased level of treatment in both outpatient and inpatient services. ‘Discharges’ measures the increased level of treatment in inpatient institutions. The additional two measures of inpatient treatment were included to adjust for
unwanted consequences of de-institutionalisation, that is, fewer beds and shorter length of stays (inpatient-days). Unfortunately, the only direct measure of increased activity in outpatient services available was the number of outpatient consultations. Hence, we have used all health services variables that were available.

14) **Dr. Wahlbeck wrote**: “..these changes had no impact on suicide mortality”. Due to the nature of the study, no interpretations about causality can be made, and the authors should avoid such wordings as “impact.

We agree with Dr. Wahlbeck, in non-experimental studies no causal inference can be made. We have now changed the text to: “However, the adjusted analyses indicated that these changes were statistically unrelated to female and male suicide mortality” (RM, p 15).

15) **Dr. Wahlbeck wrote**: In the methods section of the abstract, the authors state that one of their mental health service variables was "length of hospital stay". This needs to be corrected, as the mean length of hospital stay was not included in the analysis. The authors probably mean to say "total number of psychiatric in-patient days"?

We have changed the text to: “number of inpatient-days” (RM, abstract)

16) **Dr. Wahlbeck wrote**: “In the results section of the abstract the authors should not only give the total number of suicides during the observation period, but also indicate to the readers the prevailing trend in suicide rates in Norway during the period 1990-2006.”

We have now added: “(…) the total suicide rate declined by 26%”. (RM, abstract)

17) **Dr. Wahlbeck wrote**: In the first paragraph, authors state that “suicide rate declined most from 1990 to 1994 (crude RR = 0,94......)” This seems to be a mistake, as the equivalent rate in Table 1 is RR=0,79.

The estimate differs because it is an estimate of the annual change in the years 1990-1994. We have changed the text to: “(crude annual Incidence Rate Ratio = 0.94; 95% CI = 0.91-0.97)” (RM, p 9; third paragraph).

18) **Dr. Wahlbeck wrote**: “In the first paragraph, the authors state that “major changes in the health services variables were observed in the end of the period 2000-2006”. It is not clear whether they mean in the end of the period 2000-2006 or in the end of the observation period 1990-2006?”

We have now changed the text to: “In contrast, the major changes in the health services variables were observed in the years 2000-2006 (Table 1)” (RM, p 9; third paragraph)

19) **Dr. Wahlbeck wrote**: The sentence on power analysis (second paragraph) needs to be moved to the methods section. The authors should describe which method was used for the power computation.

We have now moved it to the methods section.
20) **Dr. Wahlbeck wrote:** In the third para of the Results section, the authors say that “the highest suicide mortality was in the... East, ....and West had the lowest suicide mortality. They should specify that this result relates to the second adjusted analysis only.

21) In the third para the authors state “unemployment... was negatively related to suicide mortality”. In the next sentence they say that “decrease in unemployment... predicted a reduction in suicide” : thus the relationship seem to be positive?

22) In the third para the authors mention one-unit increases/decreases. For the sake of readers, the authors should clarify which the units are. (is it for instance a doubling of the number of people with higher education in relation to people with lower education?).

We have reanalysed all data and done separated analyses by sex. The results section is now rewritten.

23) **Dr. Wahlbeck wrote:** “In the adjusted analysis, when main explanatory variables have been taken into account, main differences in suicide rates between regions exist (suicide mortality in the West region is 64 % of the mortality in the East region). Authors should discuss the reasons for this huge regional variation. Could it be explained by e.g. poverty, which was missing among the variables analysed?”

Differences in unemployment and education explain some of the regional differences in suicide mortality. Poverty may also be a relevant explanation. However, Norway is a wealthy country with minimal economical differences between most social groups. Religion may be an explanation, because individuals in southern and western Norway are more religious, which is a protective factor. However, our aim was not to explain regional differences, just to adjust for such differences.

24) **Dr. Wahlbeck wrote:** It should also be mentioned that in Norway 25 % of resources have been allocated to child and adolescent services, which may pay off in decreased suicide rates later only.

We have added the following sentence to the discussion section: “Further, the increased resources in child and adolescent mental health services, which were not addressed in this study, may pay off in suicide rates later on” (RM, p 12; third paragraph).

25) **Dr. Wahlbeck wrote:** It seems unnecessary to repeat the listing of mental health services variables in the results section (as they have been listed already in the Methods section of the abstract)

26) **Dr. Wahlbeck wrote:** In some instances (and inconsistently) the authors use US spelling instead of UK spelling: behavioral - Background, first row; labor - Methods, third paragraph. References:

27. **Dr. Wahlbeck wrote:** Reference 14, first author should be “Pirkola” (letter “a” is missing)

We have made corrections to the RM as suggested by Dr. Wahlbeck.

**Reviewer: Dr. H Bickley**
Several of Dr. Bickley’s comments have been addressed by Dr. Wahlbeck and Dr Christopher B. Forrest. Thus, we will only address the comments of Dr. Bickley which have not been addressed above.

1) Dr. Bickley addresses several highly relevant confounders that may impact our analyses if we include them.

We do not have the data to include them. However, we have highlighted in the **RM (p 12; third paragraph)** that: “The present study should be interpreted with caution because statistical associations can be masked by the fact that we may have failed to adjust for relevant confounders”

2) **Dr. H Bickley wrote:** Is there evidence on whether the people who die were receiving mental healthcare, or whether they had not been engaged in mental healthcare, or had become disengaged from mental healthcare?

The question is highly relevant. Unfortunately, we do not have data to statistically address the question. However, it has been estimated in Norway that about 15% of all who commit suicide were at the time of death in specialist mental health treatment for a psychiatric disorder. We have added this information to the **RM, p 4; first paragraph.**

3) **Dr. H Bickley wrote:** The study assesses the impact of sales of antidepressants. It might be useful to note here in the Methods section paragraph 6, that these are sales to pharmacies, and not necessarily medication acquired by patients, and not necessarily taken by patients. Just because medication is prescribed it does not mean that the patient consumed it. The antidepressants group is made up of different types of antidepressants. Collecting all these into one group could be hiding differences in suicide rate in people taking different types of antidepressant.

We have now highlighted that we addressed sales figures and that drugs sold not necessarily are taken by patients **(RM p 8; second paragraph).** When we analysed suicide mortality separately for men and women, sales of antidepressants became significant for women.