Author's response to reviews

Title: Chain of care for patients who have attempted suicide: a follow-up study from Baerum, Norway.

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Revision to Manuscript "Chain of care for patients who have attempted suicide: a follow-up study from Bærum, Norway."

We are very grateful for the positive comments made by the reviewers on this manuscript. We are pleased that Referee 1 finds the article “interesting and well written,” and we are pleased that Referee 2 gives us the chance to make the manuscript even better.

Our revision is made according to the comments made by referee 2.

We refer to comments by August G Wang by numbers and our responses in the cover letter are in-between the numbers. RM refers to Revised Manuscript and all changes in the manuscript are marked by red. As requested, we have received help from a native English speaking colleague to improve the English used.

1. The reviewer wrote: “The authors have made a study about a very important subject, which is the possible impact of aftercare after attempted suicide. The Norwegian “Bærum model” has been operating for years, and offers some positive hope. It is therefore needed with studies about the possible impact. However, so far there have only been descriptive studies. It is therefore a bit disappointing that the present study is of historical prospective nature, comparing results for those accepting versus those refusing aftercare.

2. The authors acknowledge the need for randomized controlled trials, but explain that this has not been possible in Norway yet.

3. The reviewer wrote: “Nevertheless, the study has some interesting points, and therefore is justified, but can not answer whether this type of aftercare has some positive impact or not. The editor must decide, whether there is some place for this type of study.”

We understand that these three critical comments relates to the design of our study. As stated in the manuscript (RM, p 15, second paragraph), we certainly acknowledge, “to determine with the highest level of scientific certainty the effectiveness of interventions, studies need to be designed as randomised-controlled trials “. As also stated in the manuscript (RM, pp 7 & 8, fourth paragraph), The Bærum Model implies an individual evaluation of needs, and assistance by the community suicide prevention team may not be relevant for all eligible patients. Thus, randomization to community team or not would break with the clinical guidelines for the model and the health care personnel. Therefore, we have chosen the next best design, namely a prospective cohort design in which we compared the risk of a repeated suicide attempt and suicide between patients who received team intervention in addition to treatment as usual with patients who just received treatment as usual.

Due to selection issues which can not be adequately handled with our design (RM, p 15, second paragraph), the strength of our conclusion is limited. However, we believe that the results give a scientific reasonable indication of the effectiveness of the intervention model. We hope the editor will agree that the present study is valuable in presenting results from a unique data set, covering more than 20 years of systematic data collection related to follow-
up, and with one person (second author GD), in cooperation with liaison psychiatrist at the general hospital, being responsible for data quality and consistency throughout the study period. Further, we present the repetition rate of 12% in the abstract so that other scientists in future studies can compare repetition rates in Bærum with repetition rates among general hospital treated suicide attempters in communities with no established chain of care systems (RM, p 16, first paragraph; abstract). In addition, high quality data on follow-up of suicide attempters are rare. We present in the manuscript highly interesting descriptive statistics that we are sure will be appreciated in the scientific community of suicide researchers.

4. The reviewer wrote: “Table 1 is a bit confusing, in fact it is 2 tables in one. The percentage calculated for variables in a horizontal way, could possible be more informative if calculated vertically for each of the two groups.”

We agree with the reviewer that Table 1 is, in fact, consisting of two tables. We have split them into two tables in RM, Table 1 and Table 2.

5. The reviewer wrote: “In the results, it is stated, that 52% accepted the aftercare. This is a very low figure and should be more explained in Discussion.”

We agree that our description of the Bærum Model is somewhat unclear when it comes to the main aim of securing follow-up for all attempters, community team or not, and we have clarified this point all through the ms (e.g. RM, p 7). Thus, our results do not show that 52% accepted the community team as part of aftercare, implying that 48% were offered, but did not accept... The interpretation of this figure is rather that for several reasons, non acceptance being only one of many options, about half of the attempters did not receive service from the community suicide prevention team. Another main reason, as implied in the ms (RM, p 7), would be that the staff at the hospital would not see any need for the team, ordinary health and social services deemed to be sufficient, and thus never offer this service to the patients.

6. The reviewer wrote: “In this same context, we need some information about the statement saying that patients are contacted shortly after discharge. Is there information about how long time after? …. Also we need information about the contacts, what was there form and how many were they? Was it mostly at the hospital, at home visits or otherwise? And was it a possibility for telephone contact?”

We acknowledge the importance of giving more details on the model. These issues are dealt with on (RM, pp 7 & 8)

7. The reviewer wrote: “The repetition rate is extremely low, and it will be almost impossible to bring it to a lower state, and even more difficult to calculate any real difference. We therefore need a description of the catchment procedure for the repetition figure. Where was this information about repetition obtained? And how was this information captured? The diagnoses at emergency and medical departments used in these cases will possible cover a broad range. What records were captured and how were the final procedure for decision of a new suicide attempt in this study? Was there any independent committee of evaluators about the repetitive events?”

Compared to the comprehensive review study by Owens and co-workers (RM, p 16, first paragraph) which documented a repetition rate of 15% within twelve months from the first
recorded suicide attempt, our estimate between 11-14% can hardly be understood as extremely low.

In the manuscript we have explained that: “The outcome variable ‘repeated suicide attempt’ was defined as a new record of suicide attempt within six months, twelve months and five years after the index attempt. The index attempt refers to the first recorded suicide attempt. In order to be recorded, the reattempt had to lead to emergency unit or hospital admittance in Asker & Bærum” (RM, p 9, second paragraph). The inclusion criteria for a suicide attempt are also defined (RM, p 8, second paragraph). We interpret Dr. Wang’s concern to be related to the problem that suicide attempters may move from the catchment area and make a new attempt somewhere else in Norway. We acknowledge this problem. However, we have defined a narrow time interval of six months to deal with the problem.