Author's response to reviews

Title: Does monitoring need for care in patients diagnosed with severe mental illness impact on Psychiatric Service Use? Comparison of monitored patients with matched controls.

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Author's response to reviews: see over
Author reply

Dear Miss Angelina Ilievska and Prof David Roe,

Herewith we send you the revised version of our manuscript. Below we give further details per reviewer comment.

REVIEWER 1: Abraham Rudnick

Thank you for the detailed review of our paper. We have incorporated almost all comments. Below we give further details per comment.

Reviewer's report:

First, I would like to state that, to my mind, this article is important as part of a growing body of practice and research related to the use of outcome measurement to facilitate improved care, program evaluation and system change in relation to mental health services. The article suggests that assessment of psychiatric patients' needs and other parameters related to them, such as severity of psychopathology, increases outpatient services use but not inpatient services use.

Below I note a few concerns I have with the article as submitted for my review:

1. The authors mention in the abstract and elsewhere that they are interested in efficiency of services, but they seem to have measured an indicator of effectiveness, at least as approximated by intervention-induced service use (particularly as related to inpatient service use). The authors should either clarify in the article why they use the notion of efficiency, or replace it with the notion of effectiveness (or possible cost-effectiveness, although cost was not directly noted in the article, and the authors end the article with the statement that cost-effectiveness should be evaluated).

We agree with the reviewer that we do not report on efficiency, in the present paper. The total project does study efficiency and cost-effectiveness, but mentioning it in the abstract and introduction of the present paper is confusing. To prevent induction of false expectation, we removed the word "efficiency" throughout the paper.

2. The authors should elaborate on and possibly explain (on page 4 in the third paragraph) their expectation that results would depend on duration of previous treatment.

We added a few sentences to the introduction to explain this (see below). Also note that we gave this variable a different name in the revised version, better describing the content (see comment #3 of reviewer 2. Page 4, second paragraph:}
Furthermore, patients at different stages of illness may respond differently to treatment [8]. Patients new in care have acute severe psychopathology, but a relatively intact social network, with higher likelihood of return to pre-onset employment. These first episode patients, particularly those with psychotic disorders, often have low insight and therefore are less likely to formulate specific care needs. Patients in persistent care, however, are more likely to formulate care needs as a result of lack of treatment response and chronic social complications. Therefore, the use of needs-based treatment plans may be associated with different changes in service use depending on treatment status at baseline. A third category is patients in a new episode, defined as having had no care for more than a year, but presenting again after a relapse of previous illness. These patients likely will present with care needs representing a mix of those with first-episode and persistent illness.

3. The authors should delete the second paragraph on page 6 (starting with “Ethical...”) as it practically repeats sentences written in the previous page, and in those sentences in the previous page they should add whether the patients signed consent or not.

We removed the paragraph, as requested, and we added a sentence on consent forms to the other paragraph. Page 6, second half of first paragraph:

Ethical committees in Maastricht, Utrecht and Groningen have confirmed that by law routine outcome data collected for the purpose of management information is not within their remit as long as patients are aware of the purpose (including scientific publications). Patients are asked during the interview to confirm that the data may be used anonymously for the purpose of research. The interviewer reports the answer on the form.

4. The authors should clarify what they mean by moderate mental illness or give examples, rather than only reference that, on page 8 (near the top).

We added the definition of SMI and MMI to the methods section. Page 7 and 8:

Definition of SMI and MMI
SMI patients had a diagnosis of schizophrenia or non-affective psychotic disorder (DSM IV 295, 297 or 298) or affective psychosis (296, 301.13) or borderline disorder (301.13). In addition, other criteria for SMI were applied because registration of diagnosis is not always complete. Thus, a score of 15 or more on the positive symptom scale of the BPRS defined SMI, as did the combination of impaired functioning (one of the two GAF scales <45; clinicians tend to overestimate the GAF - therefore, the traditional cut-off of GAF scores below 40 for SMI was raised to 45) and need for care in at least two of four a priori selected domains (accommodation, welfare benefits, alcohol and drugs). SMI is a patient characteristic: if a patient met criteria at one assessment, he or she was included in the SMI group for all assessments [5].

Patients scoring less than 45 on one of the two GAF scales and presenting with a single need in one of the four a priori CAN domains are defined as moderate mental illness (MMI) [5].

5. The authors should clarify on page 8 near the bottom why the set the alpha at 0.1 rather than the more standard 0.05.
We set alpha to 0.1 only when searching for interaction with previous treatment, because the alpha of 0.05 is an arbitrary cut-off point and when searching for interaction, power is relatively low. However, the interaction term we found had a p-value of 0.03. Therefore, we removed the phrase between brackets: "(alpha was set at 0.1)".

6. The authors should complete incomplete references (such as reference 6, where volume/issue number is missing, and reference 10, where volume/issue number and page numbers are missing).

Reference number 6 (Delespaul et al, 2005, (F)ACT-planning met een schone lei) is not published in a journal, but it is a book section. Unfortunately, there is not much more to add. Unfortunately there is no alternative in the English language.

We updated the Wiersma-reference.

7. The authors should consider deleting the statistical testing results from the abstract.

We removed the betas and the p-values from the abstract.

8. The authors should consider replacing the word impairment with the word functioning (on page 5, in the 4th last line), in line with the WHO definitions of these terms.

Here we describe the Global Assessment of Functioning (GAF), the 5th axis of the DSM IV. For scientific purposes it is advised to assess a GAF score in relation to psychopathology and in relation to the resulting level of impairment (DSM-IV-TR,...). Readers who are familiar with these two components of the GAF may not understand when we give this subscale a different name. In addition, a global assessment of functioning with respect to functioning may be confusing.

9. The authors should consider deleting the word slightly (in the 5th line of the second paragraph on page 10), as it is not clear how they can estimate that, or clarify why they think it is only slightly.

We removed the word slightly.

10. The authors should consider adding a reference at the end of page 10, to empirically support their empirical claim in that sentence.

We specified the text and added a reference. Page 12, line 1:

In addition, a small group of patients with common, less severe mental disorders, outside the range of SMI or MMI, were not excluded to avoid a loss of power and, in addition, because it may be argued that all patients treated in mental health services represent a selection based on severity, given that only the more severe half of psychiatric patients is treated by mental health professionals, rather than the GP [18].

11. The authors should consider adding an elaboration on which inpatient care differences are apparent in countries with more bed capacity (in the 2nd paragraph on page 11).
We think adding an elaboration on this is beyond the scope of the present paper. The sentence gave a reason why patients in FACT may be different from patients in care as usual. In order to meet this comment, we replaced this by another (better) reason. Page 12, second paragraph:

Second, in a sub region of the CNCM, Function Assertive Community Treatment (FACT) was in place since 2002, and FACT is associated with different patterns of psychiatric care consumption [19].

12. The authors should consider empirically grounding their explanation that "The differences in care consumption between the CNCM and NN regions can indicate an overcapacity of inpatients beds in the CNCM region" (in the last paragraph on page 12), because if that is not the true empirically, their explanation is not sound; I assume such local data is available publicly or otherwise.

We refer to this issue as a possible explanation for why there is more inpatient care consumption in the CNCM region. The discussion in the sentence below ("It has been shown ....... number of beds not reduced") does elaborate on overcapacity. However, to make it clear that overcapacity is just a hypothesis we changed the word can into may. Page 13, last line:

The differences in care consumption between the CNCM and NN regions may indicate an overcapacity of in-patient beds in the CNCM region.

Managers in the CNCM region do state that bed capacity is relatively high. However, there are no valid bed capacity figures per Dutch province. The figures we do have are not complete and not valid for this purpose – problem is that the labelling of beds is not consistent in the Netherlands precluding cross-province comparison. Moreover, the province of Limburg is surrounded by international borders, so overflow to and from adjacent areas is lower in Limburg than in the rest of the Netherlands. In addition, one of the provinces in NN admits patients from large cities in the West of the Netherlands, because of undercapacity in that region. Except for this NN province, we assume that severity of symptoms in NN and CNCM patient populations are similar (same culture, same ethnicity=Dutch, similarity as regards other risk factors). If similar patients are admitted more often, bed capacity is a logical explanation. (NOTE that even though bed capacity in this NN province is relatively high given overflow from large cities in the West, admission figures in Limburg are still higher).

13. The authors should consider adding a qualification to their proposal that professional carers be deployed variably in inpatient or outpatient care, as this solution may tempt professional carers to overuse inpatient services, and as inpatient work does not usually lend itself fully to such flexibility (considering that some professional carers such as nurses have to work in shift schedule as part of inpatient work).

The use of need-based treatment plans should prevent the overuse of inpatient services. In order to meet the comment of the referee, we added a few phases to the paragraph. We feel that further elaborating on this is beyond the scope of the present paper. Page 14, above conclusion:

Professional carers should assign patients to inpatient and outpatient treatment, based on need based treatment plans as described in the present paper. Ideally, this is in the context of team-based
community care, with the possibility to deliver services flexibly across in-patient and out-patient care solutions. This way the availability of in-patient or out-patient care is easier to adapt to the needs in the patient population. However, the health care system may not have this flexibility.

REVIEWER 2: Chiara Bonetto

Thank you for the detailed review of our paper. We have incorporated most comments and below we give further details and answers per comment.

1. The paper is based on the rationale that systematic assessment of needs and other clinical parameters will reflect on changes in out-patient care. The Authors declare that 82% of the CNCM patients were assessed for the first time. So, a real systematic assessment by the 2004 year was not available

In two-third of the region, systematic assessment started in 2004. In addition, the number of patients decreases with the number of follow-up assessments because patients are discharged. These two reasons explain the high proportion of first assessments in the data in the second half of 2004. However, many first assessments in the region that started in 2004 were re-assessed in further, follow-up assessments. Although the proportion of first assessments was relatively high, these first assessments were part of the systematic assessment. We added a sentence on this in the discussion. Page 11, last lines:

Because PCR data were available until the end of 2005, patients assessed in the first half of 2005 could not be followed for a full year and were, therefore, not included in the matching. This resulted in a relatively high proportion of first assessments, but of all these patients, the ones who remained in care had later follow-up assessments. In theory, changes in service provision may occur more often after the first assessment, as previously unknown needs more often may come to light.

2. Care consumption changes were evaluated by differences between the year after and the year before the assessment date: is one year enough as time period in order to see the effect in terms of more out-patient care and less days in hospital? Possible concerns are mainly on in-patient care.

Professional carers in practice should use the information of the needs assessment, likely to lead, given the nature of common therapeutic and social interventions in mental health care, to immediate changes in treatment rather than delayed implementation over many years. In addition, changes in health care consumption are linked to the date of the assessment. If patients are followed for extended periods after this date, one or more consecutive assessments will also be included in the follow-up period.

3. The variable categorized as ‘no care before 2004’, ‘new episode after 365 days out of care’ and ‘persistently in care’ does not indicate the duration of previous treatment. The category ‘new episode after 365 days out of care’ refers to what period of time? Moreover, it could be useful to state an hypothesis of how these different groups might present different patterns of changes in care
We agree that this variable does not include the duration of previous treatment. Duration was dichotomised into 0 days and 1 or more days. Because the variable was used for matching it had rather broad categories and we chose not to differentiate based on the length of care of the patients in persistent care. The third category of this variable indicates that a new episode started. Therefore, we gave this variable a different name and we describe it more precisely in the revised version (also see comment #2 of reviewer 1). Page 7, last paragraph of Psychiatric Case Registers:

Treatment status at the first mental health contact after January 1st, 2004 (hereafter: treatment status at baseline) included three categories: subjects were in care at this date; had never been in care (new patients) or were not in care in the 365 days before this date, but had care before that time (new episode).

4. In the Aims of the study section, it is said that matching between CNCM and control region is performed ‘in order to be able to demonstrate changes independent of trends over time’. It would be useful to specify the meaning of this statement

If a baseline assessment and a follow-up assessment are carried out in the intervention group, differences could be the result of the intervention. However, if the same increase or decrease is also found in a control group, the difference between baseline and follow-up is obviously not a result of the intervention, but of other changes in health care, health care policy etc. associated with changes in outcomes over time (trends over time). This is the reason why epidemiologists include a control group in their study. If a control group is included in an observational study, matched controls give the most valid results [1]. We added to the text (page 5, line 4):

In order to be able to demonstrate changes independent of trends over time (e.g. changes in health care or health care policy) we included patients from a control region in which no systematic and cumulative assessment of needs was in place.

5. In the Aims of the study section, the hypothesis on care consumption should be detailed by specifying which measures of care will change and the expected changes

We added a sentence to this section. Last two sentences (page 5):

We hypothesized that care consumption would change after that date in the CNCM region but not in the control region. In particular, we expected an increase in outpatient care and a decrease in inpatient care.

In addition, we elaborate on this in the introduction as follows (page 4, last paragraph):

Ideally, systematic assessment of needs and other clinical parameters as provided in the CNCM will help clinicians to respond early by making changes in out-patient care, thus preventing further deterioration and hospital admission. Therefore, it was hypothesized that CNCM would be associated with changes indicating more out-patient care and less days in hospital.
6. In the Subjects and matching section, it should be useful to detail the meaning of the variable ‘date of start mental health care episode in 2004 in days since 1-1-1960’. In particular, why did you choose 1-1-1960?

This date is of no meaning for the present paper, but just a standard reference date that the statistical programme uses to be able to calculate dates. In this variable the difference between two consecutive days is always 1 (as opposed to 19981231 and 19990101) and, therefore, this date variable was used in the matching procedure.

7. The propensity score procedure used to balance the covariates in the two groups is real good, but it should be detailed the estimation method. Discriminant analysis? Logistic regression? Other?

The propensity score procedure used is based on regression techniques. By the way, probit regression and logistic regression give very similar results (Stata website). Page 8, last paragraph:

These patients were matched with NN-controls, using propensity score nearest neighbour-matching with replacement (using probit regression estimation method).

8. Why the diagnosis was not considered in the propensity score?

Unfortunately, we did not have diagnosis in our data. Baseline service use was used as a proxy for severity. Discussion, methodological issues (page 11):

The present paper has some limitations. First, because neither diagnosis nor level of psychopathology were assessed in the control region, service use is the best indicator of illness severity that was available in both regions and therefore was used for the matching procedure.

9. The NN patients before matching are 11677 and after matching they are 612. The difference in sample size is enormous. Differences in clinical characteristics and care consumption should be explored because it was possible that the selection procedure would have introduced a bias.

Matching was done to select patients similar to our specific intervention group from the TOTAL NN patient population. Thus, we intended to select a group with characteristics similar to our patient group and thus very different from most patients in the NN total population. Using the total NN population would have lead to bias. Matching was done to avoid this. Note that in the CNCM region, the total number of patients is also much larger than 231. We were only interested in the selective group of patients that met the criteria for CNCM.

10. Data about severe mental illness (SMI) in the control region are not given. Why?

Data on SMI were based on CNCM data. The CNCM is not active in the control region. We added a sentence to the discussion as follows (page 11, second paragraph of methodological issues):

In addition, in the control patients, the SMI variable (based on diagnosis or severity) was not available. However, after matching on mental health care use, we assume percentages of SMI are similar to the CNCM patients.
11. In the Statistical analysis section, the estimated multilevel model should be detailed, in particular how individual change and inter-individual differences in change have been estimated. In the RESULTS and in Table 3 these two measures should be given.

As is stated in the statistical analysis: "Changes in care consumption (after minus before) were the dependent variables in the analyses." Thus, the change between the year before and the year after were included in the model as the dependent variable. This way, the regression coefficients show the inter-individual differences in change. Page 9, last paragraph:

Changes in care consumption (after minus before) were the dependent variables in the analyses. As a result, the regression coefficients can be interpreted as the difference in change between the two regions.

Care consumption in the year before and change between the year before and the year after are presented in table 2. Differences in change between the two regions were presented in table 3 (e.g. regression coefficient = 5.23 in case of inpatient care).

12. The RESULT section seems too much concise with respect to the methodological frame of the paper. It should be useful to develop it.

We only present three tests for interaction and five regression coefficients. Therefore, the results are rather short. In order to meet the reviewers comment we moved one paragraph from the methods to the results section (page 10).

In the matching procedure, 212 matching groups were identified. Two CNCM-patients and their controls were excluded because care consumption of the CNCM patients after the index date was not available. Eighty-five NN patients were excluded because they were not in care at the index date. Because of this, two CNCM patients did not have any controls and were excluded from the analysis. Thus, 208 matched groups were included in the analyses, varying from two to twelve patients, of which 1 to 4 were CNCM patients. A total of 231 CNCM and 612 NN patients were in the final dataset. In the CNCM region, 67.7% was diagnosed with severe mental illness, 22.6% with moderate mental illness and 9.7% with common mental disorder. Thus, ninety percent of the CNCM patients met criteria for severe mental illness (SMI) or moderate mental illness (MMI). Of the CNCM patients, 82% were assessed for the first time, 7% for the second time and 11% for the third to the sixth time. Both in CNCM and in NN, 60% of the patients were male; mean ages were 42.0 and 42.6 years, respectively.

Also note that, although the journal allows combination of results and discussion sections, we chose to shortly, but objectively present the results in the results section and discuss the results separately, in order to provide possible explanations and compare with previous literature. The reviewer may find what she is looking for in the discussion.

13 The last part of the INTRODUCTION sounds like Aims of the study, so it could be move in this section of the paper
The author guidelines state that the background section "should end with a very brief statement of what is being reported in the article." Because this section should be brief, we did not change the paragraph above "aims of the study", but additionally added a summary to the aims. Page 5:

We hypothesized that care consumption would change after that date in the CNCM region but not in the control region. In particular, we expected an increase in outpatient care and a decrease in inpatient care. Treatment status at baseline was hypothesized to be a modifier of changes in care.

14. At the end of The Cumulative Needs for Care Monitor Database section, it is written again a statement about ethical committees and patient’s consent already reported at the beginning of this section

We removed the duplicate sentences. Also see comment #3 of reviewer 1.

15. Table 1: the variables should be reported with the same labels as in the paper (e.g. ‘# days before July 1st, 2004 …’ should report ‘between January 1st 1999 and July 1st 2004’)

We changed the text in the tables.

16. Table 1: Why ‘2 years’ are introduced in the label of the variable # in-patient days?

We thank the reviewer for this remark. This was not correct in the previous version. We matched on inpatient days between and 1999-2004. Table 1:

# days 1999-2004 that patient received
(in- or out-patient) care

# in-patient days 1999-2004

17. Table 1: Was the variable age used as continuous or categorical in the matching procedure? If it was used as continuous, it is not correct to give the distribution of the categorical age.

Age was included in the matching procedure as a continuous variable. However, as an extra service to the reader, we included a description of the age categories. Some readers can more easily interpret a variable when presented in categories then interpreting mean and standard deviation. It is purely a description; the categories of age are not used in any matching or analysis.

In order to meet the reviewers' comment we moved the row describing age to the last row of the table, decreased the font and added a footnote. Table 1:

1 Age was included in the matching procedure as a continuous variable. Categories of age are provided for descriptive purpose only.

18. Table 2: SMS distribution should be move to table 1; moreover, give the same data for NN patients
Also see comment 10 of this reviewer, we do not have data on SMI in the control region.

As the reviewer requested, we removed the description of SMI from table 2. Because table 1 gives information on the matching procedure and SMI could not be included in the matching, we did not put the percentages of SMI in table 1. Instead, we put the SMI figures in the text. Page 10, second half of first paragraph:

In the CNCM region, 67.7% was diagnosed with severe mental illness, 22.6% with moderate mental illness and 9.7% with common mental disorder.

19. Table 3: introduce the measures of individual change and inter-individual differences in change

See comment #11 of this reviewer.

References