Author's response to reviews

Title: Child mental health care in Dutch general practice: Time trend analyses

Authors:

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Author's response to reviews: see over
Dear Dr. Payne,

Please find enclosed a revised version of the manuscript ‘Child and adolescent mental health care in Dutch general practice: Time trend analyses’ (Manuscript ID 1750867115573278). Thank you for the constructive comments and for giving us the opportunity to revise the manuscript. In this letter, I will outline how we have changed the paper in response to the issues raised by the two reviewers and you.

Your comments

1. I concur with both reviewers that providing a breakdown for 2 or 3 age groups within the study population would be helpful.

We agree with both reviewers that the age range is rather broad and that it would be more informative to present the results for separate age ranges. Therefore, the results for children (0-12 years) and adolescents (13-18 years) are presented separately throughout the manuscript. Time trends were performed for both age groups separately. Additionally, we performed analyses to test the effects of age group and gender on prevalence rates, and rates of prescriptions and referrals (see p. 9: ‘The effects of patient age…’ and ‘The effects of age and gender…’; p. 10: ‘Compared to children…’; p. 11: ‘Compared to children…’; Table 3; and additional file 1).

2. Likewise, I agree with Dr Hetrick that the way in which ranges are presented throughout the manuscript is a little unclear and perhaps unhelpful.

We have changed the way in which the results are presented throughout the manuscript: when we draw the general picture (e.g. the percentage of children being referred), we report data of the year 2008 in the main text and refer to the respective tables for more details. When we report changes over time, the data are reported in the way reviewer 2 suggested (see comment 17 of reviewer 2).

3. Could you comment on the increase in numbers of general practices in the LINH dataset over the study time period - does this relate to changes in population demographic or in the level of local psychological service provision; could this have influenced the findings?

An explanation for the increase in the number of GPs included in LINH has been added to page 7 (‘In 2006, another Dutch GP registration network…’). The resulting increase in the number of patients included in LINH will not have affected our results, since the results are presented as percentages of the total sample.

4. Anxiety, depression and overactivity only account for between 26 and 30% of the mental health diagnoses (tables 2 and 4). What constitutes the remaining >70% majority? Categorising and listing the remainder in some way would be helpful. Certainly, the first sentence in the results section "prescriptions for psychotropic medication", which states "mainly for overactivity and depression" is difficult to interpret in the absence of the remaining information on prevalence.

You rightly remark that the separate diagnoses that were selected for this study (anxiety, depression, alcohol abuse, and overactivity) do not represent the whole category of mental health problems (ICPC-codes P01-P99) and that it would be helpful to know which other mental health problems were recorded by GPs. Since the remaining diagnoses in the ICPC P-chapter constitute a broad and heterogeneous group, we decided not to present results for a category of ‘other mental health problems’. Instead, we reported other frequently occurring diagnoses of child and adolescent mental health problems (p. 9: ‘Apart from these selected diagnoses…’). To clarify that we report results for the total category of mental health problems (P01-P99), as well as for a selection of separate diagnoses, a sentence has been added to page 8 (‘ICPC-codes referring to…’).
that psychotropic medication was mainly prescribed for overactivity and depression has been changed (p. 10: ‘In 2008, the majority…’).

5. In reference to the treatment of child mental health problems, you state that "remarkably however, this increase was not found when only children with overactivity were considered". This comment seems odd to me. The diagnosis of overactivity has increased, and thus the increase in psychostimulants is to be expected and drives the overall increase within the population; one would not necessarily expect a proportional increase in the affected subgroup, and this is consistent with your findings.

When we performed our analyses for children and adolescents separately, the results were more consistent than in the previous version of the paper: in the youngest age group (0-12 years), we found an increase in prescriptions for psychostimulants, which was also found when only children with a primary care diagnosis of overactivity were considered. Therefore, the original conclusion has been changed (p. 14: ‘This increase was not reflected…’).

Reviewer 1
Title and abstract
1. Both the title and manuscript focus on the term ‘child’ mental health. However, the sample reflects those up to the age of 18. I would recommend that the title and text throughout the manuscript is amended to reflect “child and adolescent” mental health problems.

The title and manuscript text have been changed to reflect child and adolescent mental health problems.

2. Since many readers only look at Abstracts it would be helpful to have an additional line on the service context in the Netherlands. I would suggest that this could be put in the 2nd sentence of the Background section of the Abstract e.g. “measures to strengthen primary mental health care services have led to an increase in provision of primary care psychologists and social workers and may have affected GP’s roles in child and adolescent mental health care.” This would aid with the interpretation of the Results in the Abstract.

The Dutch service context has been added to the abstract (‘Measures to strengthen…’).

Background
3. This section provides a very clear description of the service and policy context in the Netherlands and changes in primary mental health care provision. It would benefit from a slight expansion to reflect the broader international context, especially the United States e.g. incorporating reviews of the wider international literature - Zwaanswijk et al (2003) European Child and Adolescent Psychiatry, Sayal (2006) Journal of Child Psychology and Psychiatry.

We have added the suggested references to the introduction (p. 5: ‘Literature reviews…’).

4. It would be also worth including the recent systematic review of identification of developmental and behavioural problems in primary care (Sheldrick et al; Pediatrics 2011).

We have added the review of Sheldrick et al. to page 5 (‘Previous research has indicated…’) and page 13 (‘In line with previous research…’).

5. When describing the principle of stepped care - this should also include that a criteria for referral is that the level of severity of problems means that initial secondary care input is more appropriate.
The statement that, according to the principle of stepped care, direct referral to secondary care is appropriate for some patients with severe mental disorders or complex treatment needs, has been added to page 5 (‘Direct referral…’).

**Methods**
6. Please explain what ambulatory mental health care organisations are.

An explanation of ambulatory mental health care organisations has been added to page 8 (‘Ambulatory mental health care organisations…’).

**Results**
7. When describing the prevalence of mental health problems it is more accurate to say the prevalence of ‘diagnosed’ mental health problems.

References to the prevalence of mental health problems have been changed to ‘prevalence of recorded mental health problems’ throughout the manuscript.

8. Table 2 was hard to interpret initially. It would be better to present the prevalence figures as per 100 persons aged 0-18 years. Furthermore, the 0-18 age group is broad. As different child and adolescent mental health disorders present at different ages, is it possible to present the various diagnostic categories in table 2 by age categories i.e. 0-5, 6-12, 13-18 years? This would be more meaningful clinically.

As requested, prevalence rates in Table 2 are presented per 100 persons instead of per 1000 persons. The results for children (0-12 years) and adolescents (13-18 years) are presented separately. For a more detailed description of the changes made with regard to age ranges, we refer to our reaction to the first comment of the editor.

9. Prescriptions of psychotropic medication – I was not clear whether GP’s or secondary care specialists initiated these prescriptions. It might be that the initial diagnosis and treatment was commenced in secondary care with primary care physicians then taking on longer-term prescribing.

We cannot differentiate between prescriptions that were initiated by GPs and prescriptions by GPs that may have been a repetition of an initial prescription by a secondary health care provider. This has been added to the discussion (p. 17: ‘With regard to…’).

10. Children with overactivity – it should be made clear in the Results (pg 8 2nd paragraph) that this is referring to children with a primary care diagnosis of overactivity. In the Netherlands are children with overactivity also referred to paediatricians or only to secondary mental health care?

Children with overactivity may not only be identified by GPs, but also by secondary mental health care providers or paediatricians. Therefore, we have clarified that we are referring to children with a primary care diagnosis of overactivity (p. 11: ‘When only children with…’).

**Discussion**
11. Findings from the study should be related to recent time trend studies from other countries in Europe e.g. Sourander et al (2004), Sayal et al (2010).

Findings from other European time trend studies have been added to the discussion (p. 16: ‘The latter finding…’).

12. End of paragraph 3 – recent qualitative work in the UK has also highlighted that the limited length of consultations may act as a barrier to parental expression of concerns.
The work of Sayal et al. (2010) has been added to the discussion (p. 13: ‘The limited length…’ and p 18: ‘Teaching GPs to use…’)

13. 1st sentence of the 5th paragraph – as above, should clarify that the paper is referring to the prevalence of diagnosed mental health problems rather than the actual prevalence of problems.

The sentence has been rephrased to clarify that we investigated mental health problems recorded in general practice (p. 14: ‘The prevalence of child…’).

Reviewer 2

Discretionary Revisions
1. Authors should state if ethics approval was required.

Medical ethical approval was not required for this study (p. 8).

Minor Essential Revisions
2. Authors need to be clear about the age group for whom this research is relevant. They consistently refer to children, but their inclusion criteria in the methods includes those up to the age of 18 i.e. adolescents. Related, it would be interesting to see how diagnosis rates and treatment data differ (or not) for children vs adolescents.

For a detailed description of the changes made with regard to age ranges, we refer to our reaction to the first comment of the editor.

3. The background sets up the argument well, however, it is very heavily reliant on Dutch data and to ensure the paper has relevance beyond The Netherlands, some corroborating international data would be useful to include. There are a number of other longitudinal and epidemiological studies that have been carried out that would be relevant. A few examples include the Dunedin Multidisciplinary Health and Development Study has excellent data on the long term impact of childhood mental health disorders into adulthood; The two National Surveys of Mental Health and Wellbeing in Australia have data on service use and Gavin Andrews has published data and Ian Hickie has published data from the SPHERE project highlighting how few young people receive evidence based interventions for mental health disorders; finally Lena Sanci has published work looking at the recognition of mental health disorders in young people by their GP.

We wish to thank the reviewer for her useful suggestions for relevant literature. Several international studies have been added to the introduction (e.g. p. 4, prevalence rates: Boyle et al., Merikangas et al. and Sawyer et al.; long term impact of childhood mental health problems: Goodman et al., Simonoff et al.; p. 5: initiatives to strengthen primary mental health care: Whiteford et al.). Other international publications that have been added are described in reaction to this reviewer’s next comment.

4. Again in the discussion, there are some interesting points made about limited length of consultations and stigma; however, there is a large body of literature that is relevant here and further discussion would be useful; mental health literacy might be particularly relevant as would the paper by Tylee and Sanci in the Lancet (again, Sanci, Rickwood, Jorm and Annemarie Wright).

The work of Tylee et al. has been added to the discussion (p. 13: ‘Parents and youths may not be…’). Mental health literacy programmes have been added to the conclusions (p. 18: ‘Firstly…’), with reference to the work of Haller et al. and Kelly et al. Reference to the work of Sanci et al. has been added to the conclusions (p. 18: ‘Efforts to improve…’).

5. There is a mention of the ‘basic health insurance package’ and this needs to be described more fully and might sit more comfortably with the description of the health system in The Netherlands.
where everybody has access to a general practice. There needs to be more explanation of what this health insurance means in terms of access.

An explanation of the Dutch health insurance system and its influence on the accessibility of GP care has been added to page 4 (‘Because care provided by GPs…’).

6. Authors might consider re-organising the paper such that the methods section conforms to the more traditional organization with headings for design, setting (where information about the health system, insurance etc… in The Netherlands is given); participants where information about the GPs included is given; the intervention (see below for comments about more detail being needed), and analysis.

The reviewer suggested moving the description of the Dutch health care system, which was originally placed in the introduction, to the methods section. However, we feel that the characteristics of the Dutch health care system (p. 4) and the Dutch initiatives to strengthen primary mental health care (p. 5/6: ‘As in other countries…’) form essential background information to understand our research questions and their relevance. We therefore decided to keep this information in the introduction of the paper. Furthermore, we do not perceive our study to be an evaluation of the effects of an intervention (see our reaction to comment 20 of this reviewer), and therefore do not consider it to be appropriate to include a section about ‘the intervention’ in the method section of the paper.

7. Toward the end of the background section there is a statement that doesn’t seem to add anything: ‘similar initiatives have been taken in other countries’ with UK being used as an example. Either include relevant data from this initiative, remove, or it might sit further above in the background in the context of saying something like ‘as in other countries, such as the UK, various initiatives to enhance the role of general practice in caring for those with mental health disorders have been introduced in The Netherlands’.

The paragraph dealing with UK initiatives to strengthen primary mental health care has been removed from the introduction. Instead, other initiatives aimed at strengthening primary mental health care are mentioned on page 5 (‘As in other countries…’).

8. In the last paragraph of the background, the last sentence might more accurately read ‘We will investigate the rate at which different mental health problems were identified by GPs…..’

The sentence on page 6 (‘We will investigate…..’) has been changed as suggested.

9. In the methods section, authors should include a statement about why mood stabilisers were not included as one of the prescriptions.

The reviewer had the impression that mood stabilisers were not included in our study. However, lithium (ATC-code N05AN01), a ‘classic’ mood stabiliser, was included in the category ‘antipsychotics’. Anticonvulsant-mood stabilisers were not included in our study, since they are far less commonly used for mental health problems in Europe than in for instance the US (Zito et al., 2008).

10. In the methods section Authors should outline which of the referrals were to primary and which were to secondary settings.

We have outlined which types of referrals were to primary and which were to secondary mental health care on p. 8 (‘Referrals to primary…’).

11. The issue of waiting lists in mentioned in the discussion but perhaps should be mentioned in the background (or in a reorganized methods section where there is discussion of the context in which
The issue of waiting lists in Dutch mental health care has been rephrased (p. 13/14: ‘As in many
countries…’).

12. Authors should probably make mention of the black box warnings and ensuing controversy with
regard to antidepressants for children and adolescents given the finding of a decrease in
antidepressant prescribing (consistent with Anne Libby’s work).

Our finding of a decrease in prescriptions for antidepressants has been placed in the context of the
increasing consensus that caution should be exercised in the use of antidepressants in the treatment of
youths with depression (p. 14/15: ‘This may be due to increasing consensus…’).

13. There is mention of Youth Welfare Work Officers in the discussion, which again isn’t prefaced
earlier in the paper – this again would sit usefully in a subsection “setting” in the methods
section.

The implementation of Youth Welfare Work Offices is also described in the introduction as one of the
developments that may have changed the role of GPs in child and adolescent mental health care (p. 6:
‘Another development…’).

14. More discussion is required of the finding that 2/3 of children do not receive medication or
psychotherapy – what does this mean – is there a significant under treatment of disorders?

We found that a considerable number of youths with mental health problems received neither
psychotropic medication nor were referred to primary or secondary mental health care. This finding is
discussed in more detail on page 15 (‘This may indicate…’).

15. Relevant to the final discussion point made about developing effective mental health intervention
strategies appropriate for primary health care, authors could include further discussion of the
growing body of literature about ‘simple’ or less intense interventions for less severe/mild
presentations e.g. Parker A et al trial of simple interventions; guideline recommendations for
lifestyle interventions, watchful waiting and psych education for depression etc…

The reviewer mentioned several examples of less intensive interventions, which may be feasible for
application in general practice. Indeed, our recent review (reference 60), revealed interventions
comparable to the ones mentioned by the reviewer. We have added examples of these simple
interventions to page 18/19 (‘…e.g. problem solving treatment…’). The work of Parker et al. has been
added to the conclusion (p. 19: ‘Additional research…’).

Major Compulsory Revisions
16. Authors need to give further consideration to issues relevant to adolescents in terms of access to
and engagement with general practice. This age group are less likely to be reliant on their parents
and therefore may not be visiting their GP regularly as authors state. Further discussion and
referencing relevant to this point is needed through the background and discussion of this paper.
Literature published by authors such as Anthony Jorm, Lena Sanci (especially with Tylee in the
Lancet) and Debra Rickwood may be relevant here. This is where data on diagnoses made for
children vs adolescents (and treatment data for each age group) might be interesting and shed
light on access issues.

As described in reaction to the first comment of the editor, the results for children (0-12 years) and
adolescents (13-18 years) are presented separately. The reviewer doubted whether adolescents visit
their GP as frequently as we have stated in the introduction (p. 4: ‘The majority of children…’). Our
previous research (Zwaanswijk et al., 2005) has shown the percentages of children and adolescents who visit their GP to be similar: 74% of children and 76% of adolescents had been in contact with their GP during the year of assessment. Even among children and adolescents with mental health problems, these rates were remarkably similar: 80% of children and 86% of adolescents had been in contact with their GP during the year of assessment. Other studies (references 20-22) have confirmed these findings. Still, GPs’ identification and treatment of mental health problems may be different for children and adolescents. This has been added to the introduction (p. 7: ‘GPs’ identification…’) and the discussion (p. 13: ‘Particularly adolescents may be…’).

17. Both in the abstract and then in the main paper, use of ranges when reporting data is confusing and perhaps even irrelevant. I think the authors mean to report the change over time and should report the data this way e.g. ‘between 2004 and 2008 the percentage of children diagnosed with mental health problems increased from five to seven percent, respectively’.

Please see our reaction to comment no. 2 of the editor.

18. It appears that some of the main results reported are based on a simple ‘eye balling’ of the frequency data in the tables e.g. greater referrals to secondary care compared to primary care. I think some statistical testing of the differences between referral rates would be appropriate here if they are to be reported as such main findings (e.g. in abstract).

As requested, the difference between referrals to primary and secondary mental health care has been statistically tested. A significant difference was found for both age groups (see p. 11: ‘Over the years, children and adolescents who were referred…’). For instance, of the patients who were referred in 2008, the chance of children being referred to primary mental health care was 11% (95% CI: 6-17%), whereas their chance of being referred to secondary care was 89% (95% CI: 83-94%). In 2008, adolescents had a chance of 19% of being referred to primary care (95% CI: 12-30%), whereas their chance of being referred to secondary mental health care was 81% (95% CI: 70-88%).

19. Authors should further comment on applicability of findings to countries where it is not necessarily the case that most of the population is registered with a general practitioner, both in the background and in the discussion.

The applicability of our findings to countries with different health care systems (i.e. without GPs who function as gatekeepers) has been added to the introduction (p. 4: ‘Even in countries with…’) and the discussion (p. 16: ‘Many of the issues covered here…’).

20. There needs to be further more detailed description of the measures taken in The Netherlands to strengthen the role of general practice in primary mental health care. I assume it was not possible, but authors should highlight that the ideal method/design for this type of study (after a cluster RCT) would be an interrupted time series design where outcome data were measured least three times before the intervention (in this case ‘measures to strengthen the role of primary care providers for people with mental health problems’) was introduced and at least three times after. This type of design helps to account for confounding factors that might impact on the treatment effect. As it is it isn’t clear that the results found are due to these ‘measures to strengthen the role of primary care providers for people with mental health problems’ but at the very least a more thorough and detailed description of this complex ‘intervention’ should be given (it sounds as though consultation liaison was an aspect; was it in line with collaborative care approaches; what aspects were included etc…).

Our study aimed to give insight into time trends in the identification and treatment of child and adolescent mental health problems in general practice over a five-year period (2004-2008), in the light of important developments that took place in Dutch health care during that period. The study was not meant to evaluate the effects of the Dutch policy measures to strengthen primary mental health care.
We agree with the reviewer that such an evaluation would require a more rigorous study design, with measurements before and after the implementation of the policy measures. To clarify this issue, we have added this as a limitation of the study (p. 16: ‘However, to evaluate…’).

21. It would be appropriate to make a comment in the discussion about guidelines for the treatment of various mental health disorders in childhood and adolescents in relation to the findings. One weakness of this study (and probably of the available data) is that it isn’t clear if the treatments offered were appropriate to illness severity or stage e.g. it might be that those referred for secondary care weren’t appropriate for care in primary care and thus referrals to secondary care were appropriate.

Direct referral to secondary mental health care may be appropriate for children and adolescents with severe mental disorders or complex treatment needs. This has been added to page 5 (‘Direct referral…’, see our reaction to comment 5 of reviewer 1) and page 15 (‘Although this may be…’). On page 17, we discuss the fact that our study did not evaluate the accuracy of GPs’ diagnoses of child and adolescent mental health problems (‘We did not evaluate…’).

22. Overall, I am concerned that the conclusions do not reflect the results, which in my opinion have showed a slight increase in diagnosis and overall considerable under treatment (not withstanding comment above that under treatment may not be the case if in fact the identified mental illness were not severe enough to warrant medication or referral to e.g. psychiatrist); and it isn’t clear of the relationship between these findings and the initiatives to strengthen the primary care role in the identification and treatment of mental health disorders; this is primarily because of a weak study design. As such I am not sure how much it adds to the literature/knowledge in the area. These results need to be more clearly articulated and the conclusion might be more along the lines of the need for further education and other strategies and any such further initiatives would ideally be tested in a robust study design.

In reaction to comment 20 of this reviewer, we explained that this study was not aimed at evaluating the effects of the initiatives to strengthen primary mental health care. To take away the impression that we have evaluated the effects of these initiative, the first sentence of the conclusion on page 18 (‘Although GPs’ identification…’) and the first sentence of the conclusion in the abstract have been changed. In the discussion (p. 16: ‘However, to evaluate…’), we mentioned that, based on this design, we cannot draw firm conclusions about the effects of the Dutch policy measures and that additional research is needed. Still, we believe that our study provides valuable insights into time trends in the role that GPs play in the identification and treatment of children and adolescents with mental health problems. In the conclusion, we outline other ways in which primary child and adolescent mental health care may be further strengthened (i.e. improving the identification of child and adolescent mental health problems and providing adequate treatment options in primary care). As the reviewer requested, several strategies to reach these goals are described in the conclusions (also described in our reaction to comment 15 of this reviewer).

We hope these changes will adequately meet the comments. We look forward to hearing from you after you had a chance to examine this version of the paper.

Sincerely,

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