Author’s response to reviews

Title: The gravitational force of mental health services: Distance decay effects in a rural Swiss service area

Authors:

Niklaus Stulz (niklaus.stulz@pdag.ch)
Eva-Maria Pichler (eva-maria.pichler@pdag.ch)
Wolfram Kawohl (wolfram.kawohl@pdag.ch)
Urs Hepp (urs.hepp@ipw.zh.ch)

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Author’s response to reviews:

Dear Dr. Shidhaye,

Thank you very much for giving us the opportunity to revise our paper (BHSR-D-17-00617) and to resubmit our revision to BMC Health Services Research. We appreciate the careful reading by you and the two reviewers, and we have tried to respond to the comments in the enclosed revised version.

Below, we describe how the paper has been revised in response to the points you and the two reviewers raised. Should you or the reviewers have any further suggestions that they consider would enhance the paper then we would of course be willing to take such suggestions on board.

Guttorm Raknes (Reviewer 1)

There are some major points that have to be addressed before this manuscript can be accepted:
1) More consciousness on study design. As far as I can interpret, this is an ecological cross sectional study. This should be stated in the title, or at least in the abstract. This also means that the reporting should be in accordance with the STROBE checklist for cross sectional studies. Several items are not covered sufficiently.

In addition, limitations to ecological cross sectional design should be discussed. This includes causality and the ecological fallacy.

The abstract was updated as follows: “In this ecological cross sectional study, we conducted spatial and statistical analyses in a Swiss mental health services system …” (p. 2 / l. 4).

The limitations of this ecological cross sectional design were addressed in the discussion section: “First, the ecological cross sectional design does not allow for causal inferences and bears the risk of ecological fallacy. We do not know to what extent the caseloads of the communities were affected by different prevalence rates of mental disorders in the communities or by other community-related ecological variables for which no data was available.” (p. 15 / l. 15-19).

We also used the STROBE checklist to revise the manuscript.

2) Statistical methods

a) Stratification of continuous data. I think it would strengthen the results if the municipalities were not stratified, and non-parametric tests were omitted. The high quality of the data should allow for regression analyses on non-stratified data. I understand there will be units with very few observations, and that pooling is tempting.

Please note that the primary analyses on distance effects were based on continuous data. When considering this non-stratified data, linear bivariate regression models fitted data consistently better than exponential models for all service utilization variables of interest (outpatient cases: $R^2=0.174$ vs. 0.067; outpatient visits: $R^2=0.125$ vs. 0.069; inpatient cases: $R^2=0.039$ vs. 0.013; inpatient days: $R^2=0.029$ vs. 0.012). Because of these findings, we used linear regression models for our primary analyses to see whether there is a global distance decay effect when controlling for ecological characteristics of the communities.

However, in these non-stratified analyses, there was very high variance in the caseloads because single events in small communities easily produced “outliers”. We therefore examined the extent and the shape of the distance decay effect within diagnostic subgroups using stratified data in a second step of analysis (see also 2c).
b) Poisson. An important point is that these are number of observations per time unit, and thus Poisson distribution apply. When using Poisson-based methods, lack of observations will be accounted for, and the estimates will be more precise. The statistical analyses assume normal distribution, and it the authors should at least explain why Poisson distribution was not applied.

Poisson regression requires count data (i.e., integer data equal or greater than zero) in the dependent variable. In our study, however, we aimed at analyzing and predicting caseloads (i.e. the case or intensity rate per inhabitant), which are not necessarily integer values. We therefore desisted from using Poisson regression but we complemented our analyses using bootstrapping in order to account for possible violations of the normal distribution (p. 8, l. 15-16).

c) Exponential regression. I understand linear regression is basis for the analyses. For distance decay, negative exponential regression is most common. Figure 3a show nice negative exponential curves. After an approximate reading of the curve, and quick and dirty trendline analysis in Excel, I found that the outpatient rate (y) could be predicted by distance x from the following formula y=1,0057 x e^-0.0028x, R²=0,96. Such analyses combined with continuous data would be interesting.

Linear regression models were retained for our primary analyses on distance effects because they fitted non-stratified data consistently better than exponential regression models (p. 10 / l. 8-11; p. 11 / l. 9-11; see also 2a). However, there was very high variability in the caseloads of the communities due to the numerous very small communities in the service region, in which one single event added very much to the caseload of the community. For our secondary analyses, we therefore pooled communities based on similar traveling times (5 minutes intervals), which allowed us to identify the smoothed shape of the distance decay effects within diagnostic subgroups. Such stratification of data appears to be reasonable and acceptable since the travel times reported in the timetables of public transportation services are only approximations of real travel times anyway (real travel times may vary due to e.g. delays or the fact that we calculated times to the service sites from the main public transportation station in each community). The rank ordered distance range categories required for non-parametric bivariate Spearman rank correlations (cf. p. 9 / l. 6-10). As is reported on p. 10 (l. 23-24) of the manuscript, there indeed were a very close negative relationship between distance ranges and outpatient service utilization (e.g., ICD-10 F2 diagnoses: rs=-.917; n=9; p=.001).
d) Lack of effect sizes. The results of analyses are thoroughly presented, but it is difficult for the reader to interpret the relevance. One honourable exception is that it is mentioned that more than 20 minutes distance translates into a more than 50% reduction in outpatient caseload. But what does $\beta=-0.372$ mean in terms of clinical or practical relevance? The formula above means that for each minute of longer distance, the outpatient attendance rate drops by 2.8%.

We deliberately decided not to comment extensively on the effects sizes of the multiple linear regression models since they only aimed at examining whether there is an overall distance decay effect when controlling for ecological variables of the communities. We however extended the reporting of effect sizes for our second step of analyses (p. 10, l. 16-20): “In municipalities being located more than 20 minutes away from the closest outpatient clinic, the caseload was reduced by more than 50% (Figure 3a). This trend went on up to 60 minutes (standardized caseload: 27%), though it began to level out at a distance of 30 minutes away from the closest outpatient clinic (standardized caseload: 39%).”

3) The manuscript could have been better structured. Preferably according to the STROBE checklist: https://www.strobe-statement.org/fileadmin/Strobe/uploads/checklists/STROBE_checklist_v4_cross-sectional.pdf

We used the STROBE checklist to revise the manuscript.

The discussion part needs tightening up, this is a recommended tool for better discussion sections: http://cancer.dartmouth.edu/documents/pdf/effective_discussions.pdf

We revised the discussion chapter, and we deleted or summed up some parts there. However, we also added some comments to the limitations section.
5) Table and figure captions are insufficient. It should be possible to read tables and figures independently from the text. What, where, when? Explain B, SE, β in table text.

We added explanations of B, SE, etc. to the tables.

Details:

Methods:

Describe design: Ecological cross sectional

“This ecological cross sectional study on distance decay effects in a mental health services system was conducted in the Canton of Aargau.” (p. 5, l. 18-19)

Outcomes should be reported more explicitly and precisely.

The outcomes are reported under the subheading “Utilization of psychiatric services: caseload and treatment intensity” on pp. 6-7 of the revised manuscript.

Were there any missing data, if so, how were they handled?

Regarding our primary analyses on distance decay effects across all diagnoses, there were no missing data. However, cases with missing data on the primary diagnosis were excluded from our analyses within diagnostic groups (see next point).
Results:

Present missing if applicable. See major comments.

“…n=72 (2.1%) outpatient cases were excluded from these analyses due to missing data on the primary diagnosis.” (p. 10, l. 24-25).

“… n=22 (1.0%) inpatient cases were excluded from these analyses due to missing data on the primary diagnosis.” (p. 11, l. 25-26).

Table 1: A short description of the ICD-10 codes should have been added, e.g. F3=affective disorders.

Done.

Figure 1 and 2: It seems a bit strange that some of the municipalities with the longest geographical distances have shorter travel times than municipalities closer to the hospital. Is this correct? Why? Express trains? Patients go to closer hospitals in neighboring regions?

More information in caption needed. Number of patients included?

Yes, due to express trains between larger communities, the longest geographical distances were not necessarily associated with the longest travel times. We added a comment on this on p. 7 of the revised manuscript (l. 22-24): “Note that due to express trains between larger communities the longest geographical distances were not necessarily associated with the longest travel times.”

Furthermore, n’s were added to the caption of Figure 2.
Figure 3 a & b: Is it possible to present non-stratified data? I would be a good idea with error bars, e.g. 95% confidence intervals. Why were results of F0 and F1 omitted in Fig 3a? Consider using clinical terms instead of codes ("Affective disorders", not F3). Vertical lines should be deleted, if any, horizontal lines are better.

Information on non-stratified data can be found in Tables 2 and 3. We desisted from adding further Figures to keep the manuscript short and concise. F0 and F1 are not reported in Figure 3 because there were only few outpatient cases in the diagnostic subgroups (n<200). This was explained on p. 8 (l. 19-21) of the manuscript. Clinical terms have been added to the captions of Figures 3a and 3b. We retained the vertical lines in Figures 3 because they indicate the stratified distance range categories.

Tessa Roberts (Reviewer 2)

The general methodological approach used is appropriate, although there are a few clarifications needed. There is no justification of the choice of variables included in the logistic regression model, which should be explicitly stated. The third research question is vaguely worded, and would be improved by stating specific ecological variables to be investigated as well as offering explicit hypotheses about the expected influence of distance, diagnosis and the ecological characteristics investigated.

The choice of ecological variables was based on their availability in publicly accessible databases. We fully agree that further potentially important characteristics of the communities, for which no information was available, might also have influenced the distance decay effects. In the revised manuscript, we tried to make more clearly that the selection of ecological variables was restricted by their availability, and we added a comment on this to the limitations section (p. 15 / l. 16-25). Moreover, we adapted the wording of the research questions to make more explicitly that the primary focus of the current study was on distance decay effects; that is, ecological characteristics were examined as potential confounders of distance decay effects only, and there was not enough information on ecological characteristics to comprehensively examine their association with service utilization.
A brief line to explain the reasons for the choice of a non-parametric test would be helpful, as would a note on the sample size calculation for the minimum number of cases for the analyses by disorder.

The following comment was added to the analysis section (p. 9 / l. 6-10): “Bivariate associations between the average travel time per distance range category and the corresponding caseload per distance range category were examined using non-parametric Spearman rank correlations in order to account for the rank ordered distance range categories within diagnostic subgroups.” We however admit that the selection of diagnostic subgroups with at least n=200 cases was somewhat arbitrarily: “A minimal subsample size of n=200 cases might be considered arbitrarily but it was intended to render reliable estimates of the distance decay effects within the most prevalent diagnostic subgroups.” (p. 16, l. 24-26)

It's not entirely clear from the authors' description whether travel distance was calculated from individuals' residences or if all calculations were done at the level of municipalities - this could be clarified in the text, and it may be helpful to discuss the amount of variation in travel time within municipalities so that readers can assess the likely accuracy of these measures if they were all at the municipality level.

The manuscript was complemented as follows: “Because public transportation is accessible to (almost) everybody, for this study we considered the travel time between peoples’ residences (i.e., the main public transportation station in every community) and the service facilities (mental hospital or outpatient clinic) by public transportation to be the most valid available indicator of the geographical accessibility of the treatment facilities of the PDAG.” (p. 7 / l. 14-18). We furthermore added a comment on this to the limitations section (p. 16 / l. 1-7): “… travel times between patients’ homes and service facilities were calculated using the main public transportation station in each community as starting point. The analyzed travel times thus were only an approximation of the real travel times of the individuals. However, even if individual door-to-door travel times would have been available from timetables for every inhabitant in the service area, such figures would not have been completely exact (traveling the same way twice almost never takes exactly the same of amount time, e.g. due to delays in public transportation).”
The authors also suggest that some of the variables in the model may be spatially clustered - for instance, proportions of immigrants are likely to be higher in urban areas - which has not been tested, and might warrant the use of spatial regression models. It would be useful to check the spatial distribution of these variables, to inform the choice of spatial or non-spatial regression models.

We did unfortunately not have information on further ecological characteristics such as urbanity or population density. However, regarding available characteristics, there was no indication for multicollinearity in the regressions models. To keep the manuscript short and concise, we did not present this data in the revised manuscript but we added a comment to the limitations section: “We do not know to what extent the caseloads of the communities were affected by different prevalence rates of mental disorders in the communities or by other community-related ecological variables for which no data was available.” (p. 15 / l. 16-19).

In terms of controlling for major confounders, the authors recognise that some important factors are missing. Potential confounders include population density (reflecting urban/rural residence) and education levels. Age is a relevant factor in both disorder prevalence and treatment-seeking, although it might be appropriate to treat this as a categorical variable, using age groups, rather than a continuous variable, given previous evidence of an inverse U-shaped relationship between age and treatment-seeking for mental disorders (e.g. Carragher et al., 2010; Issakidis and Andrews, 2002; Mackenzie et al., 2012; Ojeda and McGuire, 2006; Rost et al., 1998; Roy-Byrne et al., 2009; Starkes et al., 2005). The lack of any measure of geographical variations in prevalence is an important limitation, albeit one that the authors themselves acknowledge. Given this lack of an appropriate denominator, it seems misleading to describe the caseload as "the proportion of mentally ill people receiving any outpatient treatment" (p.14). If any epidemiological studies of the region exist, this limitation could be addressed in part by referencing the extent of spatial variation in prevalence of mental disorders found in previous studies and the proportion of the variation that is explained by the factors controlled for in the current study.

Unfortunately, there is no epidemiological data on the prevalence of mental disorders in the service region (cf. p. 15, l. 16-19). We replaced the sentence "the proportion of mentally ill people receiving any outpatient treatment" by "… the proportion of inhabitants receiving any outpatient treatment at all …" (p. 14, l.19-20).
Likewise, we did unfortunately not have data on the distribution of age groups within communities and therefore had to rely on the available mean age. As mentioned above, we added on comment on the potential bias due to the lack of ecological information to the discussion section (p. 15, l. 16-19).

Finally, it would be interesting to discuss potential explanations for the differing patterns of inpatient service use by people with organic mental disorders.

The following comment was added to the revised manuscript (p.12, l. 14-16): “A possible explanation for … would be that F0 diagnoses such as dementia occurred almost exclusively in elderly patients who are among the least mobile society members.”

We hope that the changes we have made are satisfactory. We would of course be more than willing to take any further suggestions of the reviewers on board. Thanks once again for your efforts in this review process.

Sincerely,

Niklaus Stulz