Author’s response to reviews

Title: A retrospective analysis of determinants of involuntary psychiatric in-patient treatment

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Author’s response to reviews:

Dear Professor Stylianidis,

enclosed please find the revised version of our manuscript. We thank the reviewers for their comments and detailed suggestions which helped us to improve our manuscript. We hope that our revision will be satisfactory for the reviewers and are looking forward to receive your editorial decision.

Best regards

Mario Schmitz-Buhl

Responses to the Reviewers’ Comments

Louk Van der Post (Reviewer 1):

General remarks

1. The authors explore the social and clinical factors that are associated with involuntary psychiatric admission in the German city of Cologne. The main limitation of this study is the fact that it followed a retrospective design in stead of a prospective one. It is also a limitation that retrospective studies that identify factors associated with involuntary
admission already have been done many many times before and therefore it is not surprising that this study does not add new perspectives.

➔ We agree with the reviewer and acknowledge that our study has some limitations due to its retrospective design. However, we think that the design of our study in combination with the statistical methods we used give it a somewhat unique position among many previous retrospective studies. We agree with the reviewer that in the original version of our manuscript we had not made this point sufficiently clear. Therefore, we rewrote most parts of the manuscript and addressed the study design question on page 7 lines 1-11. In the revised version of our manuscript we point more explicitly to the strengths of our study compared to previous retrospective studies. This includes its statistical approach, which has not been used in previous studies in this field.

2. The English certainly needs revision of a native speaker.

➔ The manuscript was revised by a native English speaker.

Background

3. In this paragraph the authors do not pay attention to the fact that laws and regulations for involuntary psychiatric admission differ between countries. Also the way (community) mental health care is organised makes a big difference. Therefore it may be useful to mention with each reference the country where the research took place.

➔ We thank the reviewer for this comment and followed this suggestion when rewriting Background Section of the manuscript. In the revised version of our manuscript, we now mention that laws, regulations and the way how mental health is organised differ between countries and play important roles when we look at quotas and rates for involuntary admissions. We now cite a larger number of international papers and mention the country of origin of those studies (page 4 line 15 to page 6 line 9).

4. I like to see a clear and concrete description of the aim of this research at the end of the introduction paragraph. What are the specific questions the researchers want to find answers to and what could be the clinical relevance of the findings? In other words, why is it important to know what the authors want to know (see also my final remarks at the end).

➔ We thank the reviewer for this comment and followed his suggestion. At the end of the Background Section, we have added a concise description of the research aims of our study (page 7, lines 12 to 14).

Methods
5. We need here a clear description of the specific Cologne mental health care setting. Also a short description of the way the PsychKG NRW works could be useful. The foreign readers need definitely more context. Otherwise, they cannot understand and interpret the results of this study.

→ We thank the reviewer for this suggestion. In the revised version of our manuscript, we added a comprehensive description of the major features of the Cologne mental health care setting and some information about the PsychKG NRW (first part of the Section Methods: page 7, line 20 to page 8 line 17).

6. The explanations in the statistical analysis paragraph are not sufficient.

What do the authors mean by “We weighted the samples of voluntary patients according to their share of all voluntary patients of the respective hospital?” How was the random sample of voluntary patients matched with the sample of involuntarily admitted patients?

→ In the revised version of our manuscript, we give a more detailed description of the procedure of random selection in the Section “Study design and data sources” (page 8, lines 23 to 28). Furthermore, we give a more detailed explanation of why and how we weighted the samples for subsequent analyses (page 9, lines 14 to 20). We hope that this makes the rationale for our analysis procedures clearer.

7. Furthermore, for a person unfamiliar with the technique of decision tree analysis using CHAID, the explanation of this technique is not easy to understand. The same applies to the description of the results of decision tree analysis in the results paragraph. Why using this specific technique? What are the advantages compared to multivariate binomial logistic regression?

→ We thank the reviewer for pointing out the need to provide a clearer rationale for using the CHAID modeling technique in our study. Logistic regression analyses lead to large sets of regression factors especially in situations such as our where many potentially interacting variables in large datasets need to be considered with a view to derive a predictive and clinically useful model. It has been argued that preventive tools based on main effects regression approaches do not adequately reflect the contingent nature of clinical risk assessment processes (Steadman HJ et al. A classification tree approach to the development of actuarial violence risk assessment tools. Law Hum Behav 2000; 24: 83-100). The decision tree approach allowed us to develop a hierarchical classification of risk factors in specific patient subgroups leading to explicit suggestions for improvements of the Cologne mental healthcare systems. In addition, we analysed a large dataset with a mixture of categorical and continuous variables, a situation in which continuous data would have to be transformed into categorical data if binomial logistic regression was to be used alone. This would have led to a loss of information because of the necessary data transformation step for continuous data. Alternatively, we could have used both linear and logistic regression analysis, which would have limited the ability of our analysis to identify interactions between the continuous and the categorical variables. Previous comparative studies have shown equivalence of the linear regression and decision trees approaches, while
others showed advantages of one over the other (see, for example Liu et al., 2011, who identified and analysed eleven informative studies comparing various types of classification tree analyses, logistic regression and neural network models in the field of forensic psychiatry). We finally decided to stay with CHAID because

i), it allowed us to study both categorical and continuous variables in a single modeling approach, and

ii), it provided a data-mining approach which is especially helpful in the situation of multiple potentially interacting predictor variables and large datasets as in our study, and

iii), we employed decision trees for risk classification, subgroup determination and the development of concrete suggestions for mental healthcare system improvements.

CHAID has been used previously in mental healthcare research for example to identify predictors of successful outcomes of methadone treatment (Murphy & Comiskey, 2013) and vocational rehabilitation for patients with affective disorders (Sanchez, 2018).

References:

In the revised version of our manuscript, we provide more detail on how the decision tree analysis using CHAID works and what the differences and advantages to other analyses are (page 10, line 3 to page 11, line 3). We hope that the rationale for our procedure is clearer now.

8. In the discussion paragraph I found the sentence CHAID was superior to logistic regression [25] as it allowed us to identify and further characterize the risk groups described above. But this explanation is rather vague. At least for a person not familiar with the technique.

See our response to the previous query. Following the suggestion of both reviewers, we now provide the readers with a more detailed rationale for our choice of analysis methods. In the revised version of our manuscript, we give a more detailed explanation of the advantages of CHAID compared to logistic regression in the Methods section (page 10, lines 15 to 19).
Results

9. Please refer to the tables (numbers) when you present the data in this paragraph. Present the data here with the percentages (proportions, means etc.) and the test results (X2 p value etc.). Instead of presenting these concrete data in the discussion paragraph.

→ Thank you for this suggestion. We now present all data in the Section Results. Data are summarized in Table 3. We refer to the tables and figures when we comment on the results in the text. We omitted the results data from the Discussion Section.

Discussion

10. The discussion paragraph shows redundancies. The same facts are repeatedly mentioned and overlap substantially with the results paragraph. This isn’t useful.

→ We reviewed the Discussion section and omitted repetitions of facts already described in the Results section of our manuscript. In the revised version, all result data are presented in the Results section.

11. The fact that elderly and retired people and patients with organic mental disorders are overrepresented in the sample of patients treated under the PsychKG NRW, means that in Cologne patients with dementia are admitted to general psychiatric hospitals. This is not the case everywhere. In neighbouring countries (like the Netherlands) these patients are being admitted to specialised psychogeriatric nursing homes. So the way mental health care is organised influences the outcome of this kind of study. This has to be discussed in the discussion paragraph. A comparison with studies in other countries which different ways of organising mental health care and different outcomes regarding diagnostic groups and age is lacking here.

→ We thank the reviewer for this comment and particularly for the valuable information provided. In the Background section of the revised version of our manuscript, we now mention the fact that the way how mental health care is organised influences the outcome of studies like ours (page 4, lines 8 to 14), and we give information on where other studies were performed (page 4, line 15 to page 6, line 9). In line with our study, a previous study from Germany and a study from Switzerland reported an overrepresentation of elderly and retired people and patients with organic mental disorders among involuntarily treated patients. We followed the reviewer’s suggestion to discuss the differences in health care organisation between countries and their impact on detention rates in people with organic mental disorders (page 15, line 26 to page 16 line 8).

12. At the end of this paragraph the notion pops up (although implicitly) that the authors aim at reduction of involuntary treatment. Apparently the authors feel that involuntary admissions has to be prevented as much as possible. And presumably this forms the
reason for their wish to know the pre-dictors of involuntary admissions. This is all very much OK off course, but… this has to be explained in the background paragraph!

→ We agree with the reviewer (see also answer to query Nr. 4). At the end of the Background section we have added a concise description of our re-search aims (page 7, lines 12 to 14).

Lily Peppou (Reviewer 2):

In the present manuscript, authors set out to explore the risk factors for com-pulsory admission in a metropolitan area of Germany in an endeavour to inform the design and implementation of preventive interventions. To this end, they have extracted data from hospital records.

I think the topic is of primary importance and I really enjoyed reading some-thing from Germany. Most of the existing literature on the topic emanates from UK, the Netherlands and the Scandinavian countries.

13. Nonetheless, authors have not appropriately placed their study in the in-ternational literature, highlighting the Germany case, especially the Cologne case, as I presume that legislation on involuntary hospitalization is different at different parts of Germany.

→ We agree with the reviewer und thank her for this comment. In the re-vised version of our manuscript, we now review several studies from differ-ent countries and place our study in an international context (section Background: page 4, line15 to page 6, line 9). In addition, we now provide the readers with detailed information on the legislation on involuntary hospitalization in the German state of North Rhine-Westphalia (section Methods/Setting: page 8, lines 7 to 17).

14. Concomitantly, a lot of information about the study methods is deducted from reading the Discussion, which is a flaw of the manuscript.

→ Thank your for this suggestion. We now provide all information about the study methods exclusively in the Methods section.

Please find below my comments/recommendations/queries per section:

Introduction

I think the authors have appropriately justified the rationale of their study (i.e. to design preventive interventions) and place it in the wider medical, ethical and legal context.

15. However, I find their literature review poor and focusing largely on Ger-man findings, rendering their study unattractive for an international audi-ence. The vast majority of
European studies, which have explored similar research objectives (i.e. to identify risk factors for compulsory admissions) concentrating either on patient characteristics or systemic characteristics (e.g. the configuration of mental health services) are not included into the introduction. For example:

- Bindman et al. 2002, Social Psychiatry & Psychiatric Epidemiology
- Craw & Compton, 2006, Social Psychiatry & Psychiatric Epidemiology
- Myklebust et al. 2012, Nordic Journal of Psychiatry
- Salize & Dressing, 2004, British Journal of Psychiatry
- van der Post et al. 2009, Psychiatric Services

In this context, authors should better argue for the added value of the presented study in relation to an international audience.

→ We thank the reviewer for this comment and the detailed suggestions. In the revised version of our manuscript, we now include all studies suggested by the reviewer and some additional international studies (section Background: page 4, line 15 to page 6, line 9). Hence, our study is now more visibly placed in an international context. We hope that will increase international reader interest in our study. At the end of the Background section, we explain why we think that our study adds important information to the already existing literature (page 6, line 24 to page 7, line11).

16. Furthermore, "legal traditions" (p. 4, line 16) requires elaboration and "quota" (as opposed to rates) (p.4, line 16) requires clarification.

→ In the revised version of our manuscript, we have deleted this sentence. By “legal traditions” we meant laws, regulations and the way how municipal courts and police services are organized and operate. We now clarify this (section Background: page 4, lines 8 to 14).

Methods

This is a retrospective cross-sectional study, using hospital records to glean information on variables.

Sample
17. I would like to know more about the Mental Health Act in this region of Germany. Some information about the process and the criteria would have been helpful.

We thank the reviewer for her comment and interest. In the revised version of our manuscript, we have included a new paragraph about the setting in which our study took place. Here we give a comprehensive description of the major features of the Cologne mental health care system (page 7, line 20 to page 8, line 6) and the PsychKG NRW (page 8, lines 7 to 17). We hope that the context of our study has become clearer now.

18. Moreover, I could not assess the generalisability of study findings. Are these 4 psychiatric hospitals the only hospitals in Cologne treating involuntarily admitted patients? Are there any general hospitals? Is there outpatient civil commitment in this region?

Yes, these four psychiatric hospitals are the only hospitals in Cologne treating involuntarily admitted patients. Each of the four hospitals provides care to the inhabitants of a sector (specific parts of the city), ranging from approximately 501,000 to approximately 109,000 inhabitants, depending on the size of the hospital. Three out of the four hospitals are psychiatric hospitals. The fourth unit is the Department of Psychiatry of the University of Cologne. There are several general hospitals in Cologne, but none of them has a psychiatric department.

In previous years, the northern part of Cologne had been served by a psychiatric hospital which was located outside Cologne. At the time of our study (in the year 2011) there was still a remaining part in the north of Cologne (about 100,642 inhabitants) served by that hospital. Cases from this northern part of Cologne are not included in our study. This was taken into account when calculating the detention rates in our manuscript (page 11, lines 12 to 13). Since 2012, the complete City of Cologne has been allocated to the four participating hospitals of our study.

There is no outpatient civil commitment in any part of Germany.

In the revised version of our manuscript, we clarified these issues and give a comprehensive description of the major features of the Cologne mental health care system (section Methods/Setting: page 7, line 20 to page 8, line 17).

19. Moreover, it would have been interesting to compute the rates of compulsory admissions in the region of Cologne, if this is feasible. This would have been an interesting finding for an international audience, as international diversity in the rates of involuntary hospitalizations in European countries is of outmost importance.

We thank the reviewer for this suggestion. The rate of compulsory admission under the PsychKG NRW in the year 2011 in Cologne was 1.96 per 1,000 inhabitants (1,773 detentions in the four hospitals per 906,477 inhabitants in the City of Cologne after exclusion of the northern part which was still covered by a hospital outside Cologne in the year 2011, see above). In the
revised version of our manuscript, we added this information (first paragraph of section Results: page 11, lines 12 to 13).

20. Regarding the involuntarily admitted group, authors state that in this group both patients primarily admitted on an involuntary basis were included as well as patients whose status changed from voluntary to involuntary during hospitalization. I am wondering whether these two groups are different in terms of their clinical and socio-demographic profile. A sensitivity analysis with both sub-groups mingled (as the authors did) as well as with two sub-groups separated would greatly enhance the robustness of the study methods.

We thank the reviewer for raising this question. A review of the literature shows that most previous studies report on involuntary “admissions”. Several of these deal with this issue similarly as we did, in that they included both patients primarily admitted on an involuntary basis and patients whose status changed from voluntary to involuntary during hospitalization (e.g., Wheeler et al 2005, Curley et al 2016, Umama-Ugada et al 2018). Thus, the use of the term “involuntarily admitted” in the literature is not always precise. We are not aware of any study looking at potential differences between the two subgroups.

Our sample of patients treated under the PsychKG NRW included n=1,401 patients primarily admitted on an involuntary basis and n=352 patients whose legal status changed from voluntary to involuntary during hospitalization. We followed the reviewer’s suggestion and conducted a sensitivity analysis by comparing each involuntary subgroup with the voluntary group. For most comparisons, results were similar to the analysis with both sub-groups mingled. Differences occurred in only two aspects when we analysed the two subgroups separately:

i), patients primarily admitted on an involuntary basis were on average older compared to patients whose legal status changed from voluntary to involuntary during hospitalization (49.0 vs. 44.3 years). Accordingly, in terms of age, the small subgroup of n=352 patients whose legal status changed was similar to the voluntary group (44.3 vs. 44.0 years, p=.75), while the large subgroup of n=1,401 cases primarily admitted on an involuntary basis was older than the voluntary group (49.0 vs. 44.0 years, p<.001). Hence, the finding of an older age of the involuntary group was entirely driven by the subgroup primarily admitted on an involuntary basis. In line with this finding, the percentage of cases with dementia and other organic mental disorders (ICD-10: F0) was higher in this subgroup (19.8% in the subgroup primarily admitted on an involuntary basis vs. 13.4% in the subgroup whose legal status changed vs. 3.9% in the voluntary group).

ii), the large subgroup of n=1,401 cases primarily admitted on an involuntary basis were more often without previous treatment compared to the voluntary cases (41.3% vs. 37.3%, p<.01). There was also a tendency for initially involuntary cases to have less outpatient psychiatric/psychotherapeutic treatment before admission compared to the voluntary cases (29.7% vs. 33.1%, p<.05). In contrast, the small subgroup of n=352 patients whose legal status changed from initially voluntary to involuntary during hospitalization was less often without previous treatment compared to the voluntary cases (29.8% vs. 37.3%, p<.01). There was also a tendency
for the cases who changed from voluntary to involuntary status to have more often outpatient unit/day hospital treatment before admission (27.3% vs. 22.5%, p<.05) and more often at least one previous psychiatric in-patient stay compared to the voluntary cases (82.1% vs. 77.2%, p<.05). Interestingly, the percentage of cases with psychosis was particularly high in this small subgroup (41.5% in the subgroup whose legal status changed vs. 28.3% in the subgroup primarily admitted on an involuntary basis vs. 17.7% in the voluntary group).

Taken together, findings from this sensitivity analysis suggest that there is a clear tendency for elderly people with organic mental disorders and little previous treatment to be admitted primarily on an involuntary basis, while patients with psychosis may often be admitted voluntarily and then change to involuntary status. This is an interesting finding that merits further study. It does not challenge the main results of our investigation. We decided to not include the sensitivity analysis in the revised version of the manuscript, simply for reasons of length.

21. More information about the sample size of the control group is required. Information about the random selection (how was random selection performed?) is necessary as well as the rationale for selecting its size. Why not include all voluntarily admitted patients? Was it for feasibility reasons solely or was there another justification?

→ We followed the reviewer’s suggestion and added more information on the procedure of random selection. It was for feasibility reasons solely that we did not include all voluntarily admitted patients. We hope that we made this point clearer in the revised version of the manuscript (section Methods: page 8, line 23 to page 9, line 2).

Instrument and Procedure

22. I think information gleaned from the hospital records should included in the main text of Methods, rather than being summarized in a Table, as a supplementary material. It is highly important information and I would like to know more details about data extraction from these records (especially how the 5 raters gleaned this data) in order to assess whether investigator bias may have arisen. Authors make a note about inter-rater reliability in the Discussion section (Strengths and Limitations); however, readers miss important information about the ways whereby information was gleaned or/and rated.

→ We followed this reviewer suggestion. In the revised version of our manuscript, we now present a list of the items that were extracted from the patient records (Table 2, page 27, lines 3 to 4). Also, we describe the data extraction process in more detail (Methods section, page 9, lines 3 to 7).

Information was gleaned directly from the hospital records. There was no rating of any items. In the original version of the manuscript, we had used a wrong wording and we thank the reviewer for this comment that helped us to correct this mistake and clarify the point. By “interrater reliability” we meant the certainty that data were always gleaned with the same care and
accuracy. We omitted this paragraph from the Discussion section/chapter “Strengths and Limitations”.

23. Much of the paper’s discussion addresses the influence of diagnosis. For this reason, the classification system (I presume the ICD-10 has been employed) used and the ways whereby diagnosis was assigned (was diagnosis assigned by the rates or by hospital staff?) should be explicitly stated and discussed.

➔ In Germany, the WHO ICD-10 classification is routinely used in clinical care. ICD-10 diagnoses were assigned by the clinicians. In our retrospective study, diagnoses and all other data were extracted from medical records. In the revised version of our manuscript, we made this information more explicit (section Methods/chapter Study design and data sources: page 9, lines 7 to 9 and Table 2, section E “Diagnosis related data”, page 27).

Statistical Analysis

24. I am not an expert at CHAID analysis. I think the manuscript will be improved, if authors could add some information about the superiority of this method over regression models. In this way, readers could better grasp the strength of this study, as opposed to studies utilizing multiple regression models.

➔ See our answer to query Nr. 7 by Reviewer 1. In the revised version of our manuscript, we provide more detail on how the decision tree analysis using CHAID works and what the differences and advantages to other analyses are (page 10, line 3 to page 11, line 3). We hope that this makes the rationale for our selection of statistical procedures clearer.

25. Overall comment: the Methods section is poor in terms of the information it provides. A lot of information regarding the methodology of the study is deducted from the Discussion section and as a result of this, I think authors should definitely re-write their Methods section.

➔ See our answer to query Nr. 14. We agree with the reviewer and followed her suggestion. We now provide all information about the study methods exclusively in the Methods section. Accordingly, we omitted this information from the Discussion section.

Results

26. Sample characteristics is not included into the results.

➔ We followed the reviewer’s suggestion and added some information on the entire sample in the first paragraph of the Results section (page 11, lines 8 to 19). The complete characteristics of the voluntary and involuntary sample are summarized in Table 3 (page 28 to 30).
Discussion

27. I particularly enjoyed reading the Discussion. I think study findings are interesting and worth reading. Nonetheless, the part where present findings are discussed in relation to existing literature is poor (for the reason explained in the Introduction).

→ We thank the reviewer for this comment. In the revised version of our manuscript, we enriched the discussion of our findings in the light of previous studies. We hope that the work we put into reviewing the international literature and putting our findings in context will increase interest in our study by international readers. We also thank the reviewer for her suggestions in Query Nr. 15.

28. Moreover, I think the importance of symptom severity (overall or by symptom area: e.g. psychotic symptoms, disorganization severity, etc) and insight (lack of insight) as clinical variables in driving involuntary hospitalizations is concealed in the paper. From my point of view, these two variables are very important in explaining involuntary hospitalizations and having not included an assessment of these clinical variables is a noteworthy limitation. Of course, I can understand that by extracting data from medical records measuring symptom severity was not feasible, but I think this limitation should be acknowledged.

→ We agree with the reviewer and followed this suggestion. In the revised version of the manuscript, we acknowledge this limitation which is pertinent to retrospective studies (page 18, line 25 to page 19, line 5). Moreover, in the Background section we comment on the advantages of prospective studies (page 6, lines 15 to 19).

29. I think authors rightly address the limitation of using hospital records with respect to the reliability of this data source; however, they miss another limitation attached to medical records, i.e. that data routinely collected for clinical purposes are usually incomplete for research purposes. My concern is not about the missing information percentages (that authors acknowledge as a shortfall). Rather, it is about the missing variables that may constitute the confounders of the study design.

→ See our answer to query Nr. 28. We agree with the reviewer. We added this limitation which is pertinent to retrospective studies (page 18, line 25 to page 19, line 2).