Author’s response to reviews

Title: Effect of Diagnosis Related Groups Implementation on the Intensive Care Unit of a Swiss Tertiary Hospital: a Cohort Study

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Author’s response to reviews:

Dear Editors,

Ladies and Gentlemen,

We thank you for the review of our manuscript entitled “Effect of Diagnosis Related Groups Implementation on the Intensive Care Unit of a Swiss Tertiary Hospital: a Cohort Study”. We thank the reviewer for the constructive comments/critics provided and addressed/implemented them all to the revised manuscript attached. Please find below a point-by-point response to the concerns raised and explanation to the adaptions performed.

As requested, the manuscript was thoroughly revised again by native English speaking reviewer (Acknowledgments section).

Please find specific responses to the inputs/concerns raised:

Inputs from reviewer #1:
“… I would recommend performing additional sensitivity analyses to test the robustness of the conclusions.”

We have discussed the concern raised with an expert in the field (acknowledgement) to understand this input. Exponential smoothing is a reliable method for forecasting in health care with a reference already provided (Methods section, line 149-152, page 7).

To test whether our conclusions are sound or rather depended on the model used (i.e. to address robustness – as proposed) we had already complemented the forecasting model by a Poisson regression analysis. In order to further underline the robustness of our conclusions, we now tested our hypothesis again using a third and independent method (revised manuscript, Methods section, line 141-142, page 6 and line 156-158 and 162-165, page 7). We performed a sensitivity analysis based on forecasts with Newey-West standard errors. Consistently, this independent method reported significantly more internal admissions of patients with a low severity of disease and less external admissions with a low severity of disease underlining that our conclusions were sound (revised manuscript, Results section, line 200-202, page 9). We provide this additional data in a new Figure (Supplements Figure 2).

“For instance, the authors used a "forecasting model based on exponential smoothing", why this choice was implemented and would the results be different if a different forecast approach would have been used? In exponential smoothing, there are one or more smoothing parameters to be determined (or estimated) and these choices determine the weights assigned to the observations. How would the results change according to different weighting parameters?”

To test our pre-set hypothesis, we have chosen methods that base on differing models and assumptions with the aim to increase robustness of our conclusion. The forecasting model, the Poisson regression and now the forecast with Newey-West standard errors fulfil this aim.

Weighting parameters affect the model fit in forecasting models. We set the weighting parameter ($\alpha$) to yield best model fit (i.e. smallest mean of the squared errors (MSE)) and used a smoothing constant $\alpha$ of 0.3. The method section was modified accordingly (revised manuscript, Methods section, line 152-153, page 7).

Inputs from reviewer #2:

“I expect that most changes seen when hospital systems move to DRG-based reimbursement will be in the activity that the hospital can easily control, like the high-risk elective surgical population. "Gaming" ICU admissions is much harder if they are emergency admissions or the care system is hierarchical with centralisation of some services like ECMO, neurosurgery etc. I would have been surprised if a change to DRG-based remuneration had a major effect on the emergency workload of an ICU. The manuscript might be improved by some brief comments on the mechanism by which a change to DRGs feeds through to ICU admissions. In the Discussion the authors say "We only detected minor effects of SwissDRG especially on admission policies......". If the hospital had changed admission policies in response to DRGs this would
have been one such mechanism. However, I would be surprised if the ICU explicitly changed its admission policy.”

No request for a change in ICU admission policies was issued as a reaction to the Swiss-DRG introduction at our institution. This information was explicitly added to the revised manuscript (revised manuscript, Introduction section, line 79-80, page 4).

Before uncovering mechanisms leading to changes in patient flows, we needed to know whether and which patient populations are affected by DRG, which was the first aim of the study. Our research efforts identified an increase of in-house admissions with low severity of disease and a decrease of such external patients.

At first glance, one would expect ICU-admission policies to be sound. However, there is profound evidence that e.g. the availability of free ICU beds heavily affects ICU admission. On the one hand, a patient with low severity of disease can eventually be handled elsewhere in the hospital, a patient with a high burden of disease can be subjected to palliative procedures. Thus, the financial pressure of a DRG-based reimbursement could modify resource allocation. These potential mechanisms, together with the reference supporting it, were added in the revised version of the manuscript (revised manuscript, Introduction section, line 80-86, page 4; Ref [21]: Arch Intern Med. 2012;172(6):467-474).

The shift of unprofitable patients from the private to the public sector, or from secondary to tertiary hospitals, was already described as potential mechanisms leading to changes in patient flows (Discussion Section, line 263-270, page 11). In order to introduce these hypotheses, we describe them in the introduction section (revised manuscript, Introduction section, line 80-84, page 4).

Because of the limited ICU bed availability, we agree that the observed movements of in-house and external patient populations may also be linked (revised manuscript, Discussion section, line 252-253, page 11 and line 271-272, page 11-12).

Since observed changes may be the result of the DRG introduction, we agree that some admissions, such as the ones which cannot be controlled, are possibly not (or less) dependent on a DRG-based reimbursement (revised manuscript, Discussion section, line 273-275, page 12 and line 285-287 page 12).

“There is no "limitations of this study" paragraph in the discussion. There are two obvious limitations, the paucity of post DRG data compared with pre-DRG data and the exclusion of half of the winter season, typically the busiest for ICUs. Whilst I understand that more post-DRG data were not available, this is still a limitation. There are also almost certainly assumptions made in the models used that are not explicitly mentioned.”

We briefly mentioned the main limitations of the study in the Conclusion section of the manuscript. Limitations are described in detail in the revised version of the manuscript (revised manuscript, Discussion section, line 314-323, page 13).
We agree that the observation period was short and that especially the DRG implementation period was short too. Since we decided to match the DRG-period with the same period of all years with available data of good quality, we assumed that season is a factor affecting admissions. Thus, admissions occurring during the months October to December were excluded throughout the whole study period, an information provided in the text (Methods section, line 110-114, page 5). We also commented this in the limitation section of the revised manuscript (revised manuscript, Discussion section, line 314-323, page 13).

“The authors say "In patients with low severity of disease in-house admissions became more frequent than expected whereas external admission were less frequent". I suspect these two are linked. Most ICUs are resource-constrained (too few beds) and run at high occupancy. If external referrals were reduced, it is likely that the capacity would be used for in-hospital patients. I don't think they should be viewed as separate findings, one was the driver and the other followed.”

The interpretation of the reviewer that reduced external admissions would drive admission of internal patients with a low severity of disease is a hypothesis, which may be correct. We mentioned and commented this hypothesis in the revised manuscript (revised manuscript, Discussion section, line 252-253, page 11 and line 271-272, page 11-12).

Again, we thank to the reviewers for all constructive inputs, which helped to further improve the paper. All authors have reviewed the article and agree with the submission. The material is not under consideration for publication elsewhere, nor has this information been previously reported. None of the authors have a financial conflict of interest.

Yours Sincerely,

R. Schüpbach