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RESPONSE TO LETTER

Machine Learning Analysis of Risk Factors for Catheter-Associated Urinary Tract Infections in Stroke Patients and Their Impact on Healthcare Quality [Response to Letter]

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Dear editor

We would like to express our sincere gratitude to Liu et al[1] for their clinically relevant and highly constructive comments on our article entitled "Machine Learning Analysis of Risk Factors for Catheter-Associated Urinary Tract Infections in Stroke Patients and Their Impact on Healthcare Quality" (IJGM-113841) Their comments are of great significance to both our current study and our future work. Accordingly, we have carefully considered all the points raised and, taking into account the study design, clinical logic and statistical methods, offer the following objective responses and clarifications, whilst remaining faithful to the facts:

1. Clarification regarding the inclusion of key variables such as t the National Institutesof Health Stroke Scale(NIHSS) and post-void residual urine volume(PVR).
   1.1 Key variables such as the NIHSS and Glasgow Coma Scale (GCS) scores were not included in the study. We fully concur with this point; variables such as the NIHSS are crucial for assessing clinical outcomes in stroke patients. We are well aware of this, and consequently, during the data collection phase, we extensively incorporated neurofunctional assessment indicators, including the NIHSS Stroke Scale, and the Barthel Index (BI). Regarding indicators such as the NIHSS score and the Barthel Index, these were assessed only for patients in the Acupuncture and Rehabilitation Department. However, in other wards (such as Cardiology and Respiratory Medicine), stroke was not the primary diagnosis; consequently, NIHSS scoring was not performed, resulting in missing values exceeding 30% for these statistical variables. To ensure the stability of the predictive model, we strictly adhered to clinical predictive modelling guidelines and excluded variables with a high proportion of missing values. This approach is fully consistent with general standards for machine learning modelling and does not disregard the heterogeneity of stroke.
   1.2 Clinical limitations regarding PVR. The patients included in the study were primarily those in the post-stroke rehabilitation phase, who often required catheterisation during their hospital stay due to an inability to urinate; Furthermore, PVR testing requires specialised equipment or entails high costs; consequently, routine PVR monitoring is not typically performed for such patients in many primary care settings and rehabilitation wards. Nevertheless, we acknowledge that PVR is an important indicator that may influence participation, and we appreciate this valuable observation.

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2. Liu suggested that catheterisation duration should be analysed as a time-dependent covariate.

   Liu points out that if catheterisation duration (DaysCAU) is treated as a time-dependent exposure factor, its inclusion in standard logistic regression would introduce temporal bias and contradict the concept of "early identification". This is an objective and rigorous observation, with which we fully concur. This study positions the model as a rapid bedside risk screening tool; its original intent was to achieve rapid stratification using the most readily available indicators, rather than serving as a predictive tool strictly based on admission baseline data. The issue you raised—that "the strongest predictor can only be quantified after hospitalisation, which contradicts the concept of early identification"—is indeed a valid point. This represents a simplification and trade-off made in this study to ensure clinical practicality. Furthermore, your suggestion to adopt the Cox proportional hazards model and treat DaysCAU as a time-dependent covariate is a more rigorous methodological approach that better aligns with the clinical context; we will adopt and optimise this in future studies. Thank you once again for your valuable feedback!
3. Liu raises questions regarding the impact of CAUTI on healthcare quality. You noted that this paper links CAUTI to healthcare quality; we must clarify that this paper has never claimed that CAUTI is an independent causal factor for prolonged hospital stays and increased costs. Throughout the text, we have used descriptive terms such as "CAUTI is significantly associated with length of stay" and "CAUTI significantly influences healthcare quality indicators", rather than making causal assertions. Within hospital infection quality assessment systems, CAUTI itself constitutes a monitored quality outcome indicator; the strength of its association with resource consumption inherently holds significance for quality control, and its clinical value is demonstrated without the need to strictly establish a causal relationship.

   Finally, we would like to thank Liu et al for their comments, which have helped us to clarify the clinical context and data limitations of this study. Under the current data conditions, the model has achieved relatively reasonable stability and discriminatory power, and possesses certain clinical guidance value, particularly for primary care institutions. Furthermore, in the future, we will conduct prospective, multicentre studies with standardised assessment protocols, incorporating key covariates such as the NIHSS and GCS, whilst comprehensively considering the application of time-varying exposure models to further enhance the model's explanatory power and clinical applicability.

Disclosure

The authors declare no conflicts of interest in this communication.

Reference

1. Liu M, Liu Y. Methodological considerations for CAUTI risk prediction in stroke patients [Letter]. *Int J Gene Med*. 2026;19:615643. doi:10.2147/IJGM.S615643

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