Journal of Multidisciplinary Healthcare

Dovepress
Taylor & Francis Group

Open Access Full Text Article

RESPONSE TO LETTER

Why First-Hand Accounts From Nursing Assistants Matter? Methodological Reflections on a Role Theory-Informed Qualitative Study [Response to Letter]

Anni Song[1,*], Xiaoling Yan[2,*], Liqun Yao[3,*], Mengwei Shao[4], Huiyue Zhou[1], Na Zhou[1], Huibin Si[1], Jiao Zhou[1], Xiaowan Li[1], Ying Li[1]

[1]Nursing Department, Ninth People's Hospital of Zhengzhou, Zhengzhou, Henan, People's Republic of China; [2]Department of Critical Care Medicine, The First Affiliated Hospital of Xiamen University, School of Medicine, Xiamen University, Xiamen, People's Republic of China; [3]Weifang Municipal Center for Disease Control and Prevention, Weifang, Shandong, People's Republic of China; [4]School of Nursing and Rehabilitation, Shandong University, Jinan, Shandong, People's Republic of China

*These authors contributed equally to this work

Correspondence: Huiyue Zhou; Na Zhou, Nursing Department, Ninth People's Hospital of Zhengzhou, Zhengzhou, Henan, People's Republic of China, Email 17839930172@163.com; 821410218@qq.com

Dear editor

We thank Dr. Zhao et al[1] for their careful reading of our article and for raising a number of thoughtful methodological points. We appreciate the opportunity to discuss these issues in an academic spirit. Below we respond to each of the five main critiques, without altering our original paper, which we believe stands as a valid exploratory study within its stated scope and limitations.

Regarding the Incomplete Application of Role Theory Due to Single-Sided Data

We concur with the authors of the letter that a comprehensive application of Role Theory would ideally incorporate data from various role holders, including registered nurses (RNs), nursing assistants (NAs), patients, and their families. Our study was designed as an exploratory investigation specifically targeting the perspectives of RNs, who are the primary professional group supervising and collaborating with NAs in integrated medical and elderly care institutions (IMECS). We do not assert that the views of RNs alone present a complete understanding of the roles of NAs. In fact, we explicitly indicated in the Strengths and Limitations section that "evaluating the effectiveness of NAs' work should also be analyzed from the patient's perspective", acknowledging this as a limitation. We wholeheartedly endorse the authors' recommendation that future research adopt a paired-sample design, incorporating both RNs and NAs, which would facilitate a more thorough analysis of role expectations, perceptions, and performance.

Proxy Reports Leave NAs' Emotional Experiences Unheard

This is a fair methodological observation. We acknowledge that direct accounts from NAs would provide richer and more accurate data on their internal emotional states. However, in exploratory qualitative research, it is also legitimate to report how RNs perceive NAs' enthusiasm or burnout, as these perceptions themselves influence team dynamics and management decisions. Our original paper does not claim to have measured NAs' true emotional states, but rather reports what RNs observed and believed. We agree that future studies should prioritise NAs' own voices on this topic.

https://doi.org/10.2147/JMDH.S625939
Received: 19 May 2026
Accepted: 2 June 2026
Published: 19 June 2026

Regarding Unverified Assumptions About IMECS-Specific NA Roles

This study was not designed as a comparative study between IMECS institutions and traditional nursing homes (TNHs). The statement in the Introduction that NAs in IMECS "differ" from those in TNHs was drawn from existing policy documents and published literature, not from our own empirical data. This statement was used solely to justify the need for dedicated research in the IMECS context, not as a hypothesis to be tested by our data.

The four themes we identified—role identity, high role expectations, role conflict, and low role authorization—represent RNs' perceptions of NAs' roles within IMECS institutions. We did not claim that these themes are unique to IMECS, nor did we intend to compare them with TNH settings. Whether these challenges are universal across elderly care settings or specific to IMECS is an important question, but it is not a question that our study was designed to answer.

We agree with the letter authors that future comparative research between IMECS and TNH would be valuable, and we would welcome such studies. However, the absence of such comparison in our paper is not a flaw; it is a consequence of our focused, single-setting exploratory design. We therefore respectfully maintain that our findings are valid within the stated scope of our study.

Regarding Team-Based Model Superiority Lacks Empirical Support

First, on the wording of perceived advantages. The letter authors note that the Discussion occasionally presented RNs' perceptions as if they were objective advantages (eg, "the team structure enhanced … allowing for earlier identification of …"). We acknowledge that this phrasing could be read as overstepping what a qualitative study based solely on RN reports can claim. In a qualitative study, what we can confidently state is that RNs believe the team-based model fosters stronger responsibility, better information exchange, and more effective mutual supervision. We should have been more consistent in framing these as perceptions rather than facts. We thank the authors for this useful reminder, and we agree that future qualitative work should maintain clearer distinction between participant views and author assertions.

Second, on the managerial logic behind RNs' preference. The letter authors argue that RNs favor the team-based model largely because it centralizes information, unifies directives and shares responsibility—and that this reflects managerial convenience rather than model superiority. We do not disagree. The original manuscript did not claim to have identified any other logic; we reported what RNs told us. The fact that RNs' preferences are shaped by their managerial position is precisely what one would expect from a study that intentionally sampled RNs. We did not set out to evaluate the two models from a neutral, multi-stakeholder standpoint. Therefore, we accept the characterization that our findings reflect a managerial perspective, and we acknowledge that this is a limitation when generalizing to model superiority.

Third, on the absence of patient and family perspectives. The letter authors point out that we did not include the views of patients or their families, and that families may value the one-on-one model for personalised care and trust-building. This is entirely correct. Our study was explicitly designed to explore RNs' perspectives, not those of patients or families. The absence of these other perspectives is not an oversight; it is a deliberate scope. We agree that any comprehensive evaluation of care models would need to incorporate patient and family views, and we welcome future research that does so. However, the fact that our study did not include these perspectives does not invalidate our findings within our stated scope.

Fourth, on disease severity and care intensity. The letter authors argue that the one-on-one model may be more suitable for patients with disabilities or dementia requiring 24-hour monitoring, and that our recommendation for the team-based model is "clinically unsound" without stratified analyses. Here we would respectfully note that our study did not make a clinical recommendation for all patients or all settings. We reported that RNs in the IMECS preferred the team-based model for reasons of management, supervision, and information flow. We did not claim that the team-based model is clinically superior for every patient subgroup. The question of which model produces better outcomes for which patient population is a complex one that requires large-scale comparative effectiveness research—far beyond the scope of our exploratory qualitative study. We therefore accept the authors' call for mixed-methods and stratified research, but we reject the implication that our qualitative finding is "clinically unsound" simply because it did not address variables it was never designed to examine.

Practical and Economic Concerns in Training and Certification Policies

We thank the letter authors for raising the economic and practical considerations regarding our proposal for a micro-certification training system. However, their critique fails to account for several critical policy developments that have taken place since our study was conducted and that fundamentally alter the premise of their argument.

First, regarding the claim that "current training costs and turnover–training investment relationships are not reported". The letter authors apply an overly rigid economic standard to an exploratory qualitative study. As noted in our original paper, qualitative research such as ours is designed to uncover perceptions, experiences, and emergent patterns—not to conduct health economic evaluations. The purpose of our training recommendation was to generate a directional proposal based on RNs' identified concerns about non-standardised pre-job training. We explicitly acknowledged in our Discussion that further work would be needed to operationalise such training models. The authors' demand for cost data and quantitative turnover analyses misinterprets what a qualitative study can reasonably provide.

Second, and most importantly, the letter authors overlook the fact that China has already initiated a national policy framework for "Unaccompanied Care Services". In April 2025, Chinese government issued the Pilot Work Plan for Hospital Accompanied Care Exemption Services, initiating a national pilot programme running from June 2025 to June 2027. The policy explicitly mandates that hospitals recruit or train medical nursing assistants to deliver 24-hour daily living care for hospitalised patients, supervised by registered nurses, under patients' informed consent. This policy has already been implemented across multiple provinces. The national pilot programme has a defined timeline and institutional framework. Far from being "unjustified", our recommendation aligns directly with a centrally-driven policy initiative.

Third, on the question of training costs and "unrecoverable investment". Contrary to the letter authors' speculation that standardised training may lead to wasted resources, multiple provincial governments have already instituted training subsidy programmes precisely to address the turnover–training dilemma. In Fujian Province, the provincial government has allocated funds from the vocational skills improvement action fund for four consecutive years, providing training subsidies of RMB 1500 per qualified medical nursing assistant trained. Between 2021 and 2024, Fujian trained and certified a total of 6789 medical nursing assistants. Similar training initiatives are underway in Hainan (five-tier vocational qualification system for medical nursing assistants), Sichuan (120–150 hours of modular training, free of charge to trainees, with provincial certification). The letter authors' assertion that "institutions may face unrecoverable training costs" ignores the reality that training in many regions is publicly subsidised. Our recommendation of a micro-certification system would be compatible with these existing subsidy arrangements, potentially modularising training into shorter, competency-based units to reduce the upfront burden on trainees and employers alike.

Fourth, on the concern that micro-certification may "alter functional boundaries" between NAs and RNs and create role conflict. The letter authors hypothesise that RNs might "resist perceived encroachments on their professional domain". However, the available evidence from China's actual implementation of "Mianpei Zhaohu Fuwu" indicates a different trajectory. In the national pilot scheme, the relationship between RNs and medical nursing assistants is explicitly defined as one of supervision and delegation, not competition. Medical nursing assistants work under the guidance of nurses, performing auxiliary tasks within clearly specified boundaries. The micro-certification system we proposed would not expand NAs' scope beyond these existing boundaries. Rather, it would professionalise and standardise the competencies that NAs are already expected to possess within the RN–NA delegation framework.

Fifth, regarding the claim that "the study does not account for … unintended consequences of public health policy interventions". We agree that rigorous evaluation of any policy intervention is essential. That is precisely why we framed our recommendation as a suggestion for future research and practice, not as a finalised blueprint. However, the letter authors' argument that a qualitative study should have pre-emptively conducted health economic analysis is neither methodologically appropriate nor practically realistic. The role of exploratory qualitative research is to identify problems, generate hypotheses, and suggest directions—precisely what we did. The letter authors' expectation that a single qualitative study should also provide cost-effectiveness analysis, stratified clinical outcome data, and implementation risk assessments misrepresents the appropriate scope of qualitative inquiry.

Once again, we thank the letter authors for their rigorous and constructive critique. Their comments have helped clarify several points about our study's design and scope. We hope this academic exchange encourages further high--quality, multi-perspective research on the important role of nursing assistants in integrated medical and elderly care institutions.

Disclosure

The authors report no conflicts of interest in this communication.

Reference

1. Zhao FY, Fu QQ, Zhu JY. Why first-hand accounts from nursing assistants matter? Methodological reflections on a role theory-informed qualitative study [Letter]. *J Multidiscip Healthc*. 2026;19:608451. doi:10.2147/JMDH.S608451