

Drs. Dai and Felson reply

To the Editor:

We appreciate the interest and comments by Yang and Hung¹ in our recent publication, "Sleep Quality Is Related to Worsening Knee Pain in Those with Widespread Pain: The Multicenter Osteoarthritis Study"². Yang and Hung raised 3 issues about potential biases in the methodology in our study around (1) the assessment methods used for sleep quality, (2) confounding factors such as other pain medication used, and (3) the appropriateness of causality in the study conclusion¹.

Regarding the first point, we have recognized in our report that using a single questionnaire item for sleep quality adopted from the Pittsburgh Sleep Quality Index or restless sleep frequency drawn from the Center for Epidemiologic Studies Depression Scale in the past week to estimate sleep quality was a major limitation of the study. However, our findings have suggested that these 2 sleep measures were moderately high using Spearman correlation ($r = 0.69$) and that their relationships were coherent with consistent frequent knee pain and with knee pain worsening when widespread pain was present. To elaborate further, the approach of using a single item to estimate sleep quality has been shown as a valid, simple, and practical method in clinical research³. Earlier reviews have indicated that sleep variables, including sleep duration, self-reported sleep, and number of awakenings, were stable among older adults^{4,5}. Adding data on sleep quality during the 2-year follow-up may introduce reverse causality, in which pain could affect sleep quality. These are minimized by assessing sleep quality as a predictor of later pain.

For the second point, nonsteroidal antiinflammatory drugs were the most commonly used pain drugs for knee pain in subjects in the Multicenter Osteoarthritis Study and at any rate, one would not expect that pain medication mentioned by Yang and Hung¹ would alter the difference between the association of sleep and generalized but not localized pain. Hence, we believe that the pain medications mentioned by Yang and Hung would not confound the relationships assessed in our study.

For the third point raised, we would like to clarify our results. For the frequent consistent knee pain outcome, we found statistically significant results in the cross-sectional analysis for the prevalence of the condition, but not in the longitudinal analysis for the incidence of the condition. However, for knee pain worsening, in longitudinal analyses between baseline and 2 years later for change in knee pain worsening, we found statistically significant results, particularly when widespread pain was present². These results

have indicated potential causal inferences between sleep quality and knee pain worsening, depending on the status of widespread pain. Since this study is not a randomized clinical trial, no causality has been confirmed. We recognized this in our report that "future studies with longterm comprehensive and objective sleep assessments are needed to verify these results to elucidate the effect of sleep on knee pain when other body pain comorbidities exist."².

In summary, we hope that we have addressed Yang and Hung's concerns on biases in the methods in our study¹. These concerns have been recognized in our original report² and are further explained in this correspondence.

Zhaoli Dai^{1,2} , PhD, Research Fellow

David T. Felson^{2,3} , MD, MPH, Professor

¹Centre for Health Systems and Safety Research, Australian Institute of Health Innovation, Faculty of Medicine, Health and Human Sciences, Macquarie University, and The University of Sydney, Faculty of Medicine and Health, School of Pharmacy, Sydney, Australia;

²Boston University School of Medicine, Department of Medicine, Section of Rheumatology, Boston, Massachusetts, USA;

³Centre for Epidemiology, University of Manchester and the NIHR Manchester BRC, Manchester University NHS Trust, Manchester, UK.

Address correspondence to Dr. Z. Dai, Centre for Health Systems and Safety Research, Australian Institute of Health Innovation, Faculty of Medicine, Health and Human Sciences, Macquarie University, Sydney, Australia. Email: joy.dai-keller@mq.edu.au.

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