Reviewer's report

Title: Comorbidity and disability in patients with osteoarthritis of the hip or knee

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Reviewer: Julie Keysor

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Overall: This manuscript examines cross-sectional associations of comorbid medical conditions with functional limitations among persons with knee or hip diagnosed osteoarthritis. While there is scientific literature that shows that people with osteoarthritis have more concurrent comorbidities there is little research in OA that directly links comorbidity to functional limitations or other health outcomes and even less that attempts to link comorbidity disease severity to functional limitations. Thus, the general notion of the manuscript would, in my opinion, add to the scientific evidence examining the determinants of functional limitation and disability. There are, however, several limitations to the current paper that limits its usefulness in the field.

Major Compulsory Revisions

First, the paper lacks conceptual clarity. The authors note on page 6 that they are defining disability according to the ICF model. They don’t however actually define disability in the paper. The term disability is often used to represent different concepts—some use the term to represent the functional limitation domain and others use the term to represent the WHO handicap domain (disability in Nagi’s framework). Furthermore, disability in the new ICF model is an umbrella term that indicates problems in health in the areas of impairment, activity limitation, or participation restriction. The term, disability, thus in this context covers a very broad range of health outcomes, and in my opinion, the use of the term in this manner is very problematic.

The authors, however, used clear and acceptable outcome measures pertaining to functional limitation and pain. I would encourage the authors to use terminology pertaining to functional limitation or ‘activity’ if the authors desire to use ICF terminology.

Second, it is not clear to me why the authors examine pain as an outcome for comorbidity in lower extremity OA. The literature review did not explicitly establish that comorbidity is a risk factor for pain and I’m not sure why, with the exception of obesity, that comorbidity would be associated with pain in this population above and beyond the disease pathology of OA. Rationale for this link needs to be stronger or alternatively the outcome of pain could be eliminated since the results do not show many meaningful associations.

Third, though the authors note in the limitations section that it is potentially problematic to use the cumulative Illness Rating Scale, which assesses impact of
the condition on function, with outcome measures of function, this is potentially a
significant flaw to the manuscript. With this measurement and analytical
approach it is probable that the statistical association between the two measures
is biased (inflated). It might be useful for the authors to provide more data or
evidence that assessing comorbidity in this manner and then associating the
measure with measures of function is a valid approach.

Fourth, the writing of the manuscript could be enhanced. The major problem is in
terms of developing logical arguments for example in the introduction and the
discussion. The authors should also be careful to keep terminology at the level of
association and cross-sectional relationships.

I have a few comments about each section:

Introduction: The introduction could be enhanced with a more thorough review of
the literature on comorbidity and the association/prediction of functional
limitations (or pain) among older adults and those with OA. The authors are
encouraged to be careful with terminology and focus for example, 2nd
paragraph, comorbidity is a risk factor for disability... Then in subsequent
sentences the authors discuss a disability outcome, a pain outcome, a quality of
life outcome, and a drug use outcome. With such varied outcomes it is difficult to
interpret the literature.

In paragraph #3 the authors develop the argument that the studies havenât
assessed the relationship between the nature of comorbidity and the
âpresence of specific diseases or combinations of diseasesâ and
disabilities. The authors donât, however, set this up in the introduction well by
showing that it is important to look at comorbidity in this manner. Furthermore,
these studies have most often done a count method. Doesnât that approach
address ânatureâ to some extent? The authors describe nature in terms of
the presence of specific diseases or combinations of diseases but they donât
really address this in the paper. The paper seems to address severity of
comorbid conditions/count of severe comorbid conditions, and then specific
diseases used at an individual level rather than clusters or groups of diseases
which the phrasing âcombinations of diseasesâ seems to imply.

Methods:

There is a fairly low response rate (37%) for this study. The implications of this (it
is potential persons with more comorbidity or more severe disease refused to
participate) should be discussed in the limitations discussion.

More information is needed on the Cumulative Illness Rating Scale such as
validity and reliability estimates (which populations?). More information on the
detail of the items might also be useful. For example, if visual deficit was present
and persons needed corrective eyewear would they score a 2? If so, is that score
really meaningful?
Categories of age are given in tables but this is not described in the methods section. Why were these categories chosen? The authors should provide references to support why they chose the confounders they did. Why didn't they use SES or education?

The study population was selected from orthopedic or rheumatological practices. Are these the main practices that see adults with knee and hip OA? If not, how does this impact the results?

Discussion:

The authors state on page 11 that the patients reported more limitations in activities on the outcome measures. More than who? What is the comparison to? The authors then seem to make a comparison of the scores in this study to a normal population (what does normal mean? National population? Population-based sample?) and then state that scores are comparable. This is confusing.

Paragraph #1 on page 13 is difficult to understand, particularly the links from the first full sentence to the one preceding it.

Tables: Table 4 is very difficult to interpret. Consider only reporting the adjusted scores since unadjusted is nearly the same. Consider dropping pain as an outcome.

What next?: Unable to decide on acceptance or rejection until the authors have responded to the major compulsory revisions

Level of interest: An article of limited interest

Quality of written English: Needs some language corrections before being published

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:

I declare that I have no competing interests.