Reviewer's report

Title: Long Term Care in 29 European countries: evidence from an ecological study.

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Reviewer: Jouke van der Zee

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Reviewers report Damiani et al LTC in 29 European countries

General: This is an audacious and impressive attempt to carry out a crosssectional/time series study of Long Term Care in Europe based on international (EUROSTAT and OECD) data.

It is impressive both in a material and in a technical sense (relevant data, sophisticated analysis) and it is audacious, because the authors decided to do the analyses in spite of a lot of: yes, … buts.

But the study deserves to be published. My comments are marginal, not more than minor essential remarks.

Minor essential comm 1)

My first general comment about the study is that it contains too much information. It is both a mostly (that is: on this large scale) unexplored area (so, one would expect the authors to use room for questions about definitions, validity of data, methods, interpretation) and it contains a combination of cross-section and time series data, which, in my opinion, might deserve two separate papers. One paper about ‘Patterns’ and one about ‘Trends’. For the ‘Patterns’ part one could perhaps present one (as robust as possible) figure per variable per country, while the ‘Trends’ part would show whether the countries move into a certain direction and which direction that would be and what possible explanation the authors see.

Only if the ‘Patterns’ part could not be constructed without the author’s use of time series data this general comment is not adequate, but it would make the paper and the line of reasoning far easier to understand. If the editors/authors decide to keep the paper as it is, I’d suggest a clea structure (in the first part we deal with crossectional differences, in the second part we’ll analyze trends.

Minor essential remark 2) My second remark regards the rather stern policy recommendations or rather the recommendation that policy makers start applying the results into their policy preparation processes. This is a first attempt to reveal patterns and trends in Long Term Care based on a crosssectional/time series study with highly aggregated data. It is a long way from these (relevant and intriguing) results to policy applications. This requires studies on a lower aggregation level in order to reveal possible mechanisms behind the patterns and trends revealed.

Specific:
1): the research question: clearly formulated (though better in the last sentence of the background part than in the last sentence of the abstract which sounds a bit tautological)

2): Methods appropriate?

I’d suggest to move definitions and operationalisations (what is LTC, how are the data described) from the appendix to the method section. I realize that this may blur the clarity of the paper, but in this study that makes use of aggregated and routinely collected data a precise documentation (as provided in the appendix) is essential for the interpretation of the principal components. The description of the methods seems adequate to me; the graphs, however, are difficult to interpret (this correlation circle with all these years); can’t they be transformed by an expert in graphics? One more point; there is no definition of Long Term Care; I can understand why, but this’d better be explained than smoothed.

3) are the data sound?

First of all, these are the best data available. They cannot, as far as I know, be replaced by similar or comparable data sets. But, any student in this matter knows that, especially in the sector of frail elderly care, a bed in a long term ward of a general hospital in France is different from a (Dutch and medical) nursing home bed is different from a Danish home for the elderly. That is why I suggested earlier to devote a part of the discussion to this phenomenon; the authors can’t be blamed, but they can point out which elements of the data set are more valid than others and how this can be improved. Even an indicator like subjective health has a different scoring pattern in France (a bunch of grumpy people; see last week’s comment in the Economist) compared to Iceland or Denmark (these differences in grumpiness have their mirror in the bunch of literature on international differences in ‘happiness’).

4) does the manuscript adhere to international standards of reporting?

Yes, see above for what I said about the appendix and the validity of the international data.

5) discussion and conclusion well balanced? Discussion is adequate; the authors might devote some space to alternative explanations (cultural differences; value differences (Roman and Greek Catholic family values versus Lutheran/Protestant/agnostic emphasis on individualism; the international differences in family values are mentioned; the authors might try to refute or confirm this value theory with the data they dispose of.) The conclusions are too firm, in my opinion. This is a first analysis and a source of inspiration for studies at lower aggregation level; so I’d suggest to be more careful and to point to further research on intriguing results.

6) Aim clearly stated? Yes certainly in the background section; less so in the abstract

7) authors acknowledge the work of others? Yes, to a certain extent. I miss references to the international SHARE-studies, that describe a lower number of countries (last version I thought 12) with rich and self collected data; In the SHARE study countries are divided into ‘family countries’ and ‘individual oriented’
countries, which might be useful. I also miss references to international value studies and/or the work of Geert Hofstede who distinguished between feminine European cultures (Nordic countries) and masculine cultures (Latin countries). Might be part of the discussion (alternative explanations). I already mentioned the worldwide happiness studies (Veenhoven and colleagues) where the same happy Icelanders and other nordic tribes pop up.

8) title and abstract; title is rather technical (do all readers understand what an ecological study is? I do, as a social scientist, but some readers might confuse it with an environment study). Abstract is extremely ‘abstract’. Might be a bit more down to earth.

9) language; if you read the paper carefully and dissect the long sentences, the wording and phrasing are concise and basically clear.

Level of interest: An exceptional article

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

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I declare that I have no competing interests

Jouke van der Zee