The present cohort study aimed to examine patterns of adherence to antihypertensive medications and statins to compare subjects with incident dementia vs those who remained dementia free over a three-year period. The main finding is that the odds of dementia were greater for people with moderate adherence to antihypertensive drugs compared to other adherence patterns.

Thank you for the opportunity to review this well written paper, I only have a few comments/questions below.

Comments

1. Page 2: line 44- The reference cited doesn't support the position that most adherence studies use the PDC method. I'm sure the authors must have a SR or similar reference handy that would be more suitable.

2. The use of GBTM to identify distinct patterns of medication adherence in this study is very interesting and would appear to be a novel (& presumably enhanced) use of this method in adherence research. Admittedly, I am not well versed in the fine points of this method, but would ask the following questions:
a. Has this method been used in a population with neurodegenerative disorders, namely aMCI, AD, or mixed dementia?

b. If so, how was the modelling adjusted to account for the trajectory of cognitive decline patterns?

c. If not, can the authors explain why it is appropriate to use this method in people whose cognition declines over time, and at different, non-linear manner?

d. It would be useful if the authors would add 1-2 sentences to explain this method and its advantages.

3. It would be useful for contextual purposes if the authors would compare and contrast the incidence of dementia (preferably stratified according to subtype) in their cohort with that of similar studies, or even by the most recent stats provided by yearly Alzheimer's reports.

4. Given the possibility that some subjects may have screened as normal with the CASI, yet had mild dementia, please comment on the effect of such misclassification as a potential limitation.

5. Page 13- limitation #3- this is by far the biggest challenge in using large databases to identify changes in adherence in older people, considering the incidence of cognitive impairment and Alzheimer's disease and related disorders, and the role of informal or formal caregivers. As noted by the authors, they were unable to capture this with their dataset. Yet the appearance of "good" medication adherence is more often than not a façade attributable to caregiver assistance, or patient misreporting their adherence. Furthermore in MCI, refill data may also suggest good adherence, but a home visit reveals otherwise. The authors should consider elaborating regarding the aforementioned in their discussion.

Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

Yes
Are the conclusions drawn adequately supported by the data shown?  
If not, please explain in your comments to the authors.

Yes

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?  
If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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