Reviewer’s report

Title: Discontinuation of adjuvant hormone therapy among breast cancer patients not previously attending mammography screening

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Reviewer: Alfred Neugut

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The manuscript addresses a topic which has received extensive research efforts and publications over the past 10-15 years. The investigators do address an issue which has not previously, to my knowledge, been described. The fact that nonadherence with hormonal therapy in breast cancer is associated with nonreceipt of screening mammography is novel but not overly surprising. While I like the paper overall I do have some issues:

1. Presumably some of those who accepted the screening invitation and underwent screening mammography did so because they had already detected a lump and, for them, this would really constitute a diagnostic rather than a screening mammogram. I suppose, if anything, this would bias the results to make the observed estimates even greater, and I do not know any way that the investigators can quantify this phenomenon. Nonetheless, it could be mentioned in the Discussion, perhaps among the limitations of the study.

2. The syntax and general writing of the paper is poor and truly needs a native English speaker to edit it well.

3. On page 4, paragraph 3, the authors state that this study aims to predict the association of nonadherence to mammography with subsequent nonadherence to breast cancer treatment. I would remove this sentence since this study is specifically focused on hormonal treatment for which there is no prior literature. Whether nonadherence to mammography predicts nonadherence to chemotherapy or radiotherapy or surgery is unclear to me and the authors are certainly not addressing it. If they have addressed this with a literature search, then they can state that this is true and give references or state that this has not previously been addressed.

4. The conclusions of the authors seems to me to be wildly over-stated, i.e., that the better survival seen among those who undergo mammography is due to better adherence to hormonal therapy. This is an observational study, and certainly the association between screening mammography use and survival is confounded by general health behavior. That is why we never recommend (or should never recommend) screening modalities in the absence of randomized trials. There are 8 randomized trials that have been conducted to assess
survival and mortality outcomes for screening mammography that have established its efficacy. I assume that these investigators in Sweden have access to at least some of those trials in Scandinavia - if they want to show that somehow the observed survival differences within the randomized groups reflected also differences due to differences in adherence to hormonal therapy, then fine with me. But to draw that conclusion from this observational study is going beyond what is legitimate. They could just as well conclude that some of the benefit of screening mammography is that there is better cardiovascular outcomes among those who undergo mammography and that that accounts for the better mortality.

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?
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Yes

Are the conclusions drawn adequately supported by the data shown?
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No

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