Reviewer's report

Title: A Case-Control Study of Occupational Magnetic Field Exposure and Alzheimer's Disease: Results from the California ADDTC

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Reviewer: Hester Ward

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Basically, I agree with reviewer 3 in some respects, but not all. There are some methodological elements that are inadequately described, but if more detail was provided, it is likely that the study would be suitable for publication.

For any case control study an important issue is that of bias, which comes in many forms. However, in itself, bias does not necessarily mean that a study should not be published. Ideally sources of bias are recognised in the discussion. However, whilst the authors of this paper can state that they think bias is minimal for specified reasons, it should be up to the readers to be able to assess this for themselves. The only way to for readers to assess for bias is have clear methodology, including, how cases and controls were recruited, response rates etc. In my opinion as the method section stands at present there is not enough details to understand how cases and controls were recruited. There needs to be clear pathways from referral to the centres through to recruitment as cases/controls in to the study. Specifically, were all referrals that met diagnostic and age criteria included in the study as cases/controls? If not, what were the numbers of cases/controls not included? The authors reference Sobel et al's study (ref 4), however, I could not get this on-line & I do not know if it answered the question above.

With regards to the small number of controls to cases, if as I presume, controls are referrals that are non-cases, a relatively small number would not be unusual. The authors' response to reviewer 3's review of this, is from my point of view satisfactory.

There were a few points that came to mind as I was looking at the paper:-
1) The authors mention in the results section that 72% of controls had occupational data compared with 85% of cases. Is this difference statistically significant? It could be an area of bias.
2) There are more women in the study than men, presumably, because women live longer than men. Older women were more likely to be housewives than younger women and so more likely to be categorised as "low MF exposure". To exclude the potential confounders of women/age/occupation, perhaps the authors could conduct logistic regression analysis for men only in order to determine whether the relationship persists with potential confounding factors in the model.