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Letter to the Editor

A rejoinder to Grool et al. (2010)

We read with great interest the response from Grool and colleagues (Grool et al. 2010) on our article ‘Localization of white-matter lesions and effect of vascular risk factors in late-onset major depression’ in the Journal (Dalby et al. 2010). We agree overall with their methodological considerations about manual labelling and slice thickness in magnetic resonance imaging (MRI). However, we have some important comments to their critique. Limitations to manuscript length may sometimes compromise a thorough discussion of all important aspects in an article and we regret if Grool et al. (2010) found our discussion of the methodological issues incomplete.

First, we used a 3-T MRI scanner in our study. In the SMART-MR study (Geerlings et al. 2010) that Grool and colleagues refer to in their letter, they used a 1.5-T MRI scanner. The use of 3-T MRI improves the detection of more subtle white-matter changes than 1.5-T MRI due to better spatial resolution and increased signal-to-noise ratio. This has been shown for white-matter changes in both normal adults (Neema et al. 2009), multiple sclerosis (Di et al. 2009), and epilepsy (Phal et al. 2008). Therefore, we expected to detect more lesions and thus a higher lesion volume than most previous studies using 1.5-T MRI.

Second, we appreciate the effort of Grool et al. to calculate medians of our deep white-matter lesion (DWML) volume. However, we would have preferred to provide the original data for this purpose. The true median values of DWML volume in our sample is 0.18 ml for patients and 0.26 ml for controls, and not 0.4 ml and 0.5 ml, respectively, as Grool et al. (2009) and colleagues (Grool et al. 2010) found.

When excluding the proposed outliers in the patient group with small lesion volumes (z = −0.22, p = 0.83), none of the proposed outliers in the control group in Fig. 1 with high lesion volumes (z = 0.47, p = 0.64) and additionally three possible outliers in the patient group with small lesion volumes (z = −0.22, p = 0.83).

Finally, we recognize Grool et al.’s comment about possible selection bias. Generally, recruitment to most clinical studies introduces the risk of selection bias. Because this study involved depressed patients admitted to a psychiatric hospital or treated in out-patient facilities, our patients may be presumed to be more severely ill and presenting with more comorbidity than patients seen in general practice. Selection towards more severely ill patients may cause an overestimation of the effect measures, which may not be demonstrated in large population-based studies. Our control group was recruited from advertisement in a local paper, and responders were carefully screened by telephone prior to the formal, structured diagnostic interview, partly to exclude controls wanting to participate because of suspected symptoms of depression and dementia. Another type of potential selection bias is the recruitment of ‘super-normals’, i.e. not being representative of ‘average’ controls. In the present study, the distribution of socio-demographic variables and vascular risk factors, except for social class and smoking, was very similar in patients and controls. Adjusting for social class did not alter the result of no significant difference in DWML volume between groups (t = −1.19, p = 0.24); these results were not shown in the original article. We have no reason to believe that our control group is not representative of the background population.

Declaration of Interest

None.

References


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