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Letter to the Editor

Well controls in case-control studies

We were interested to read June's *Psychological Medicine* in which one of the methodological problems found in psychiatric case-control studies was examined (Schwartz & Susser, 2011). The authors gave a hypothetical example of the use of 'supernormal' well controls where a certain disorder was an exclusion criterion for controls but not for cases, creating a difference in the prevalence of this disorder between the groups. This disorder had its own strong relationship with the exposure and overwhelmed any which may have existed between the exposure and the intended outcome, leading to misleading results. They point out that this practice has no conceivable benefits and should be discouraged.

Schwartz & Susser cite our work quantifying the prevalence of this and other problems in the general psychiatric literature for the years 2001 and 2002 (Lee *et al.* 2007). We found 32/408 (8%) of the studies were guilty of using supernormal controls and that the problem could not be excluded in a further 145 (36%). This left 231 (57%) of the studies reporting applying the same recruitment criteria to cases and controls, in accordance with best practice.

However, the particular issue of differential recruitment criteria was only the 14th most prevalent methodological problem of 17 examined. The most prevalent problems were poor reporting of the sampling of participants and poor descriptions of the

cases, including whether incident cases were recruited. In general, non-reporting of key methodological issues was at least as great a problem as the reporting of poor methodology.

Since we collected our data, the STROBE (STrengthening the Reporting of OBServational studies in Epidemiology) Statement has been published (von Elm *et al.* 2007). It is a consensus statement documenting best practice of reporting observational epidemiology and is now officially adopted by at least 106 biomedical journals (STROBE, 2011), and four of the six journals we examined. The two journals which neither refer to the Statement in their instructions to authors nor refer to the Uniform Requirements published by the International Committee of Medical Journal Editors (which themselves refer to the Statement) are the *British Journal of Psychiatry* and *Psychological Medicine*, ironically the very journals which published these two methodological articles.

We suggest it is of particular importance for more general journals to insist on the use of guidelines of this kind because the existence of the powerful biases which may overwhelm true findings in case-control studies may not be acknowledged by all researchers in all domains of study. It is hoped that better reporting will lead to better methodology and therefore more valid results in observational epidemiology, including case-control studies in mental health research.

Declaration of Interest

None.

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Letter to the Editor

Location and progression of white-matter lesions, lacunar infarcts and atrophy associated with motivational and mood symptoms in patients with symptomatic atherosclerotic disease: things to ponder

The recently published article 'Location and progression of cerebral small-vessel disease and atrophy, and depressive symptom profiles: The Second Manifestations of ARterial disease (SMART)-Medea study' (Grool *et al.* 2011) generates much interest. The authors have done justice to the topic. We take the opportunity to highlight a few scientific facts related to the study. The main aim of the authors was to study the correlation between white-matter lesions (WMLs), lacunar infarcts and atrophy with motivational and mood symptoms in patients with symptomatic atherosclerotic disease. We think that the baseline blood investigations did not include full blood count, renal profile and thyroid function test, which would help identify and exclude metabolic causes such as anaemia, uraemia and hypothyroidism or hyperthyroidism. These metabolic causes could be the reasons for features such as anhedonia, energy loss, concentration problems, depressed mood and appetite disturbance. The exclusion criteria in this study seemed to be rather loose.

We feel that the reference used to categorize and define the different types of brain infarcts was not mentioned properly. The sentence 'We defined lacunar infarcts as infarcts of 3–15 mm in diameter and located in the frontal, parietal, temporal ...' suggests that the definition of brain infarcts was arbitrary. The most important question asked is whether the 15 mm size for the lacunar infarct was still considered as a cut-off mark. It is pertinent to mention that an earlier study debated the acceptance of 15 mm size as a criterion for lacunar infarct (Cho *et al.* 2007).

The Patient Health Questionnaire-9 is a subjective tool of assessment. The ill-defined points of the scale (i.e. 'on several days', 'on more than half the days' or 'nearly every day') may confuse the patients and lead to inaccurate information. We feel that a preferable method should objectively state the number of days per week for example (0 days, 1–2 days/week, 3–5 days/week, 6–7 days/week). We also wonder how the Patient Health Questionnaire-9 was filled out. In a cohort of patients with concentration problems and anhedonia, the information gathered from the patients themselves is questionable.

Overall, the paper by Grool *et al.* (2011) is an interesting article and we applaud the meticulous work of the authors and especially the editor for publishing such an informative paper.

Declaration of Interest

None.

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A reply to Sakthiswary & Das (2011)

We read the response of colleagues Sakthiswary & Das (2011) to our article 'Location and progression of cerebral small-vessel disease and atrophy, and