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Letters to the editor

OVERVIEW

Please submit letters for the editor's consideration within three weeks of receipt of *Clinical Medicine*. Letters should ideally be limited to 350 words, and sent by email to: clinicalmedicine@rcplondon.ac.uk

Sarcopenic obesity: under recognised and over treated?

Editor – Cruz-Jentoft and Landi's fine review of the growing importance of sarcopenia (*Clin Med* April 2014 pp 183–6) omitted to discuss the increasingly recognised condition of sarcopenic obesity. At an individual level the classification of overweight and obesity by body mass index (BMI) as a measure of (excess) fat and lean tissue mass is increasingly recognised as flawed,[1] and many older people with apparently 'healthy' BMIs may in fact be sarcopenic.[2] Meta-analyses consistently show that mortality and morbidity associated with overweight and obesity only increase at a BMI above 30 kg/m^2 in the elderly. In addition, the incidence of cardiovascular disease, mortality and all-cause mortality is higher in those with sarcopenic obesity than those who are 'simply' obese. Sarcopenia probably lies at the heart of the so-called obesity paradox – the finding that modest overweight is beneficial. Thus in the elderly, weight loss interventions are best offered to patients who are obese rather than overweight (by BMI definition) and who have functional impairments, metabolic complications or obesity-related diseases that can benefit from weight loss. Physical activity and exercise should form part of any weight loss therapy, but are of particular importance in the elderly. ■

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Delirium: a synthesis of current knowledge

Editor – van Munster and de Rooij are two highly experienced delirium investigators, but I am concerned about four points in their article on delirium (*Clin Med* April 2014 pp 192–5).

1 A key element in their article is the change from the fourth edition of the *Diagnostic Statistical Manual* (DSM-IV-R) to the fifth (DSM-V). However, the 33 references do not include the source on DSM-IV-R or DSM-V. I consulted both the American Psychiatric Association website (www.psych.org) for DSM criteria on delirium and the hard copies of DSM-IV-R and DSM-V. Neither DSM-IV-R nor DSM-V criteria define 'acute onset' as 1 day. DSM-V suggests 'several days', but in practice most investigators, except me, ignore this and do not report speed of onset.
2 The authors are concerned about underdiagnosis of delirium, which is common outside geriatric medicine or old age psychiatry. However, the opposite process – overdiagnosis – is prevalent.[1] Labelling acute behavioural change in dementia as a delirium instead of behavioural and psychological symptoms of dementia (BPSD) is the leading reason for this. There are many reasons for overdiagnosis; in a country with a national health service, general practitioners (GPs) experience difficulty in convincing hospitals to admit patients with BPSD, whereas labelling it 'delirium' is the instant ticket to hospital admission. Diagnosis related group (DRG) funding in some hospitals favours delirium over dementia.
3 Although the authors label their article 'a synthesis of current knowledge', they have completely ignored dissenting views in medical journals that publish the greatest number of delirium articles. This is a logical fallacy known as suppressed evidence. We demonstrated that confusion assessment method (CAM) positive delirium in 647 acute geriatric admissions had no effect on survival in hospital or at 30, 90, 180 or 365 days post admission.[2] Subsequent articles on the Central Coast Australia Delirium Intervention Study (CADIS; ClinicalTrials.gov NCT01650896) showed that a 25% decline in attention, executive function or memory in 24 hours produced a more robust phenotype than the CAM with respect to eliminating false positives, such as BPSD and Parkinson's disease psychosis, and generating high reversibility.[3,4] The phenotype of delirium is to asthma what dementia is to the chronic obstructive pulmonary disease (COPD) phenotype.
4 Every delirium research proposal, investigator guideline and methods section in articles must describe how the investigators tested hearing before any cognitive tests and corrected hearing with portable amplifiers, which are as essential as the stethoscope in cognitive research for older people.[6] ■

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Response

Editor – We would like to thank our colleague for his thoughtful comments and would like to respond point by point.

1 We regret not mentioning the references for the *Diagnostic Statistical Manual* (DSM) criteria. Although both clinicians and researchers can be expected to be familiar with the formal well defined DSM criteria, it was an error to not put the reference in our reference list. We agree with the fact that acute onset does not mean '1 day', as we adhere to the DSM criteria. Our table did mention 'acute' without defining it. We believe that the speed of onset is dependent on the cause of delirium, with postoperative delirium taking around 2 days and sepsis just a few hours.
2 The underdiagnosis of delirium is a frequent problem and might be partly related to the fluctuation of symptoms throughout the day.[1] Missing delirium symptoms could prevent appropriate treatment of the underlying disorder of the patient and could be seen as a medical omission. We agree there is no need for the admission of patients with behavioural and psychological symptoms of dementia (BPSD) to hospital, but this diagnosis is not always easy for a general practitioner (GP) with limited time for observation.
3 Our manuscript aimed to give an overview of delirium by summarising the important aspects and presenting some new insights based on important papers of the recent years. Our review is not exhaustive, and more important highlights have been published recently. We believe the meta-analysis of Witlox has the highest level of evidence on survival and delirium, and we expect delirium researchers of the included studies would have been able to discriminate well between BPSD and delirium.[2] The confusion assessment method (CAM) is not the 'gold standard' test for delirium. The more strict the definition of delirium (according to DSM criteria), the stronger the association with mortality can be expected. This may be an explanation for the lack of association between CAM positive delirium and survival in the Australian cohort.
4 We agree that testing of hearing is important for all diseases that use cognitive testing – not just for delirium, but also dementia and depression. Importantly, hearing loss is also a risk factor for delirium, and this is often underreported. Additionally, there are other important impairments that can influence performance on cognitive functional testing, such as visual impairment and language problems. In general, one could expect that healthcare workers and researchers involved in delirium research take possible impairments into consideration. ■

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Pulmomary embolism in Bradford, UK: role of end-tidal CO_2 as a screening tool

Editor – I read with interest Riaz and Jacob's article on using end-tidal CO_2 as a screening tool for pulmonary embolism (*Clin Med* April 2014 pp 128–33).

I would like to point out that the estimates of the performance of D-dimers and Wells' score presented in the article, including the area under curve (AUC) figures, are severely biased as a positive D-dimer or high Wells' score were used to select patients for inclusion in the cohort in the first place. For example, the reported AUC of 0.52 for the Wells' score should be interpreted as the ability of different values *above* the threshold to discriminate between patients with pulmonary embolism (PE) compared to those without or, to put it otherwise, whether a threshold different to the current one would be more appropriate. Similarly, the reported performance of end-tidal carbon dioxide ($ETCO_2$) applies only to patients preselected for a computed tomography pulmonary angiogram (CTPA) on the basis of a positive D-dimer test or elevated Wells' score, but cannot be assumed to apply to the general population of patients presenting to the hospital with suggestive respiratory symptoms. ■

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Funding of medical education: the need for transparency

Editor – In their reply to Dacre and Walsh's piece on medical education Pereira Gray and Harding (*Clin Med* April 2014 pp 212) could have gone further. Were the providers of clinical attachments – whether hospitals or general practices – to tender competitively for contracts to take students it is our belief that, not only would the price charged more accurately reflect the cost of providing the teaching, but innovation in the way that teaching was provided would flourish too. ■

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