

Hazel Thornton also refers to the recently published research which attempts to correlate the effects of multidisciplinary working on survival by Kesson, highlighting that, despite the authors' acknowledgement that their results 'might have been due to bias and confounding factors', the findings were interpreted immediately and implicitly in the accompanying editorial as being supportive of multidisciplinary team working (Brown, 2012). This editorial went on to use the relatively weak evidence of the Kesson study as a basis from which to articulate many of the assumed but unproven benefits of the multidisciplinary team format. It therefore seems to fall into the very category of multidisciplinary team justification of which we are critical.

Taylor and Green accept that the multidisciplinary team model requires refinement but make this subject to the preference they perceive cancer professionals have for multidisciplinary teams. This preference is evidenced only in terms of multidisciplinary team membership being less stressful for participants. A review of the effectiveness of multidisciplinary teams concluded that although the vast majority of multidisciplinary team attendees believed that team working improved standards of patient care and treatment, the studies in which those views were gathered were of weak design (Taylor et al, 2010). There is, therefore, little justification for allowing the perception of cancer professionals to override the need for critical appraisal of the multidisciplinary team system.

The degree of reliance Taylor and Green place on their informal interviewing of patients may be misplaced. One might wonder whether the patients who were 'greatly reassured' by the multidisciplinary team format were given enough information about multidisciplinary teams to be said to understand them sufficiently to give informed comment about them. Were they, for example, aware of the average constitution of the multidisciplinary team, that their case receives an average of only 4 minutes consideration, or what the consent and data issues involved might be? Patient respondents would need to have been apprised of all the facts before being asked their view or the findings of the interviews may not reflect true opinion.

Taylor and Green make the distinction that multidisciplinary teams make 'recommendations' not 'decisions'. A different terminology does little to change the point that the treatment plan is arrived at by the multidisciplinary team without the involvement of the patient.

The quality of evidence, including the recent Kesson study, as to the effectiveness of multidisciplinary teams remains poor and many presumptions of benefit persist. Numerous unresolved concerns relating to cost, patient benefit and issues of consent and confidentiality have been identified. Critical appraisal is warranted.

**Sarah Thornton/David Dodwell**

*Consultant Oncologist*

*Institute of Oncology*

*St James Hospital*

*Leeds LS9 7TF*

*(david.dodwell@nhs.net)*

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## Hypertension-related, but not necessarily age-related

**Sir,**

Although it is well recognized that both dementia (Barnes and Yaffe, 2011) and non-valvular atrial fibrillation (Go et al,

2001) have a sizeable age-related component, it also needs to be recognized that, to a certain extent, it is the duration of exposure to a risk factor, such as hypertension, rather than age per se, which accounts for these disorders being more prevalent in the old than in the young.

Accordingly, where dementia is attributable to vascular factors (Barnes and Yaffe, 2011), and where non-valvular atrial fibrillation is attributable to hypertension (Go et al, 2001), there is the potential for the age-related prevalence of these disorders to be reduced if young adults receive early diagnosis and treatment of hypertension. This can be achieved through population screening of adults (right down to the age of 18 years) (US Preventive Services Task Force, 2003), even though this strategy has not been specifically endorsed by the National Institute for Health and Clinical Excellence (2011).

**Oscar MP Jolobe**

*c/o Manchester Medical Society*

*Manchester M13 9PL*

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## Correspondence

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Professor Rob Miller

Editor-in-Chief, BJHM

c/o Rebecca Linssen, MA Healthcare

St Jude's Church

Dulwich Road

London SE24 0PB

email: [rebecca.linssen@markallengroup.com](mailto:rebecca.linssen@markallengroup.com)

fax: 020 7978 8316