sample the frequency of deaths is higher in men (162/702 *v.* 136/1006 in women). A power calculation for anxiety disorder shows that we could have detected an unadjusted relative risk of 1.65 in women and 1.71 in men, with an alpha risk of 0.05 and a power of 0.80. The analysis in men is thus not underpowered and if an association with mortality exists in men, it is less strong than in women.

With ageing, people face multiple adverse events including physical multimorbidity and loss of capacities. Personal resources, such as self-efficacy, sense of mastery or control beliefs, and psychological resilience are important in the process of coping with a chronic disease. On the other hand, anxiety disorder, irrespective of the aetiology, could clearly contribute to a worse outcome. This underlines the importance of developing interventions for older persons aimed at maintaining or improving psychological coping resources when health declines. Up to now, very few well-designed studies have been performed with such a large population sample, capable of controlling for main confounders and using a validated anxiety diagnosis including anxiety subtypes. Although future research is needed to confirm our results and the gender-specific association, our study also stresses the importance of including anxiety diagnostic tools in population-based cohorts to improve the understanding of the consequences of anxiety in late life.

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Does assessing quality of life improve patient satisfaction? Yes – unless you look at the results

The need for holistic assessment in psychiatry is becoming ever more pertinent. Therefore, we were pleased to read Boyer *et al*'s randomised controlled trial.[1] The team investigated whether assessing and feeding back a quality of life (QoL) measure to the patient's care team as part of a psychiatric assessment would improve patient satisfaction when compared with both standard psychiatric assessment and with measuring QoL but not informing the care team of the results.

We were also initially pleased to read that 'global satisfaction was significantly higher in the QoL feedback group [. . .] compared with the standard psychiatric assessment [. . .] and QoL assessment groups'. However, on closer inspection, it appears that this interpretation of the results is not correct.

The primary outcome was level of patient satisfaction in the QoL feedback group compared with standard psychiatric assessment. In this comparison, 29 out of 40 patients (72.5%) in the QoL feedback group were 'very satisfied' with their care, compared with 27 out of 40 (67.5%) in the control group. This difference of 5% is far too small to be statistically significant with the sample size used.

Indeed, when we undertook our own basic statistical analysis, we found that the 95% CIs for relative risk ratio between these two groups were −37% to 22% – clearly not significant.

The correct interpretation of these results is that the study actually provides no evidence for assessing and feeding back a QoL measure in preference to simply not measuring QoL at all. The conclusions drawn by the authors, that their findings 'provide strong support for integrating QoL assessment and feedback' and that 'priority should be given to strategies to implement QoL measurements in routine practice' seem particularly unfounded.

Although we agree that QoL measures represent a potentially highly useful clinical tool, we cannot accept that Boyer *et al*'s study provides evidence for this claim in any way.

We felt that the most salient finding from the trial was in fact the far lower satisfaction in the control group of patients who had their QoL assessed but had the results ignored. If we offer an assessment or intervention, we should be careful to follow-up our intentions or the result may actually be detrimental overall.

1 Boyer L, Lançon C, Baumstarck K, Parola N, Berbis J, Auquier P. Evaluating the impact of a quality of life assessment with feedback to clinicians in patients with schizophrenia: randomised controlled trial. *Br J Psychiatry* 2013; **202**: 447–53.

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Authors' reply: We are in agreement with Langford & Badenoch's general comment on the need for holistic assessment in psychiatry. It is currently established that patients' views, and especially quality of life (QoL) measures, should supplement the usual indicators of quality in healthcare.[1] However, we are doubtful about the relevance of their criticisms.

Langford & Badenoch denounced the following sentence: 'Global satisfaction was significantly higher in the QoL feedback group [. . .] compared with the standard psychiatric assessment [. . .] and QoL assessment groups'.[2] This assumption was derived, however, directly from our results (i.e. the proportions of very satisfied patients were 73% in the QoL feedback group, 45% in the QoL assessment group and 68% in the standard group). The comparison performed using a chi-squared test was statistically significant ($P = 0.025$), allowing us to state that global satisfaction significantly differed between the three groups. As we have written in our Discussion, this finding did not prohibit us from suggesting that integrating QoL assessment and feedback with standard psychiatric assessment seemed relevant or that priority should be given to strategies that implement QoL measurements in routine practice.

Moreover, this assumption was in agreement with our study design (i.e. three arms) and the sample size calculation performed for this design. However, we recognise that multiple treatment arms in randomised controlled trials (RCTs) are sources of misunderstanding,[3] especially because there are several possible comparisons.[4] Langford & Badenoch re-wrote our primary outcome for a two-arm RCT as follows: 'level of patient satisfaction in the QoL feedback group compared with standard psychiatric assessment', implying pairwise chi-squared tests. However, our primary outcome and analysis were defined in accordance with the primary objective integrating the three-arm design. The objective was to globally determine the 'positions' of QoL feedback, QoL without feedback and the control group with respect to their relationships to satisfaction; we did not aim to question the relevance of using the QoL measure (which is already recognised in the literature) in 2×2 comparisons between the different arms. The primary criterion was thus analysed using a global chi-squared test, determined *a priori*; it was not analysed using pairwise chi-squared tests (as recommended by Langford & Badenoch), which were not planned and for which the alpha error risk was not controlled. It is also widely recognised that bias may be introduced if decisions regarding data analysis are driven by the data.[3]

Langford & Badenoch also claim that 'The conclusions drawn by the authors, that their findings "provide strong support [. . .]"

and that "priority should be given to strategies to implement QoL" [. . .] seem particularly unfounded'. We did not conclude with these two sentences, which were taken from the Discussion (the function of which differs from the conclusion[5]) without heed to what was written before and after. In fact, we stated that 'Priority should be given to strategies to implement QoL measurements in routine practice', especially because 'clinicians did not optimally use the QoL feedback' and 'obtaining QoL data in an efficient, real-time manner is difficult and rare in clinical practice'.

Last, we were pleased to read that Langford & Badenoch felt that the existence of a nocebo effect in the QoL assessment group with feedback was the most salient finding, as this was an issue that we extensively discussed in our manuscript.

In conclusion, it is important to insist that any result reported in a study must be interpreted considering the objective and the design of the study and, more globally, in the context of current scientific knowledge. In agreement with Karl Popper, we believe that scientific objectivity is based on intersubjectivity and the ethics of discussion. We hope that our answer will close the gap between our scientific work and the understanding of Langford & Badenoch.

1 Boyer L, Baumstarck K, Boucekine M, Blanc J, Lancon C, Auquier P. Measuring quality of life in patients with schizophrenia:an overview. *Expert Rev Pharmacoecon Outcomes Res* 2013; **13**: 343–9.

2 Boyer L, Lançon C, Baumstarck K, Parola N, Berbis J, Auquier P. Evaluating the impact of a quality of life assessment with feedback to clinicians in patients with schizophrenia: randomised controlled trial. *Br J Psychiatry* 2013; **202**: 447–53.

3 Baron G, Perrodeau E, Boutron I, Ravaud P. Reporting of analyses from randomized controlled trials with multiple arms: a systematic review. *BMC Med* 2013; **11**: 84.

4 Schulz KF, Grimes DA. Multiplicity in randomised trials. I: endpoints and treatments. *Lancet* 2005; **365**: 1591–5.

5 Skelton JR, Edwards SJ. The function of the discussion section in academic medical writing. *BMJ* 2000; **320**: 1269–70.

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Correction

Treatment for mild cognitive impairment: systematic review. *BJP*, **203**, 255–264. In the paragraph headed 'B vitamins' in the Results (p. 261) the last sentence should read: De Jager *et al*[30] found in a lower-quality (validity score: 4), 2-year study that executive functioning improved relative to placebo (Table DS2).

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