Sleep problems are pervasive in people with schizophrenia. There is a strong documented link between insomnia and psychosis symptoms, and longitudinal studies suggest that insomnia predicts new episodes of paranoia. We cannot assume that standard interventions for insomnia will be successful in psychosis and no trial of such interventions in psychosis has been done. On this basis, Daniel Freeman and colleagues’ study in The Lancet Psychiatry is well founded. This pilot feasibility trial assesses the benefit of cognitive behavioural therapy (CBT) in the context of insomnia in individuals with a schizophrenia spectrum diagnosis and persistent delusions or hallucinations. Participants were successfully recruited, adhered to treatment, and were followed up to a high level. The CBT intervention was well received by participants, leading to reductions in the primary outcome measure of levels of insomnia 12 weeks after treatment (adjusted mean difference 6.1, 95% CI 3.0–9.2, effect size d=1.9). Despite reductions in insomnia in the large effect size range, there was only a weak indication that the intervention would lead to improvements in psychosis symptoms, including paranoia.

This trial comes at a time when conventional CBT for psychosis, although recommended by NICE, has come under scrutiny, and for good reason: the effect sizes of this first wave of CBT for psychosis are small to moderate in terms of effect on psychosis itself. This outcome was evident nearly 10 years ago; we argued that CBT should concentrate mainly on the affective dimension of psychosis (which is after all where CBT cut its teeth) and on well theorised mechanisms. The affective dimension in psychosis is increasingly understood, as is its link with psychosis onset and persistence. CBT for psychosis has become a very complex intervention and, because the population under study is heterogeneous, it risks losing impact because the effect on individual mechanisms and outcomes is diluted.

Freeman and colleagues’ study is an excellent example of the new wave of CBT interventions in psychosis focusing on the affective dimension and theoretically driven treatment targets, and for which trials are parsimonious and focused on hypothesised mechanisms. The emphasis on feasibility, acceptability, and effect sizes in the present study, rather than on p values, is apposite. Notable is the very high rate of completion of the CBT intervention (96%) and the high acceptability, similar to that achieved in equally focused interventions for post-traumatic stress disorder and command hallucinations in psychosis. The Schizophrenia Commission emphasised the need for new interventions, particularly those with high acceptability, which current, predominantly drug-based, treatments tend not to have. This sleep intervention satisfies this requirement.

However, one of the downsides of focusing on single, but nevertheless important, symptoms is that they are rarely present alone. In this case, sleep difficulty usually accompanies depression and anxiety. The question is raised as to what extent insomnia is embedded in depression; indeed, many patients in the present study were receiving SSRIs and most were severely depressed. This issue is important from a theoretical perspective, because depression and emotional dysregulation have generally been implicated in the ontogeny of psychosis; moreover, depression and suicidal thinking are, over time, virtually ubiquitous in patients with psychosis. The definitive trial should examine depression as a mediator of any effect on paranoia.

Although the effect of CBT on insomnia was in the large range, the effect on hallucinations and delusions was small at best. Importantly, the effect on quality of life and overall fatigue was in the medium range, underscoring the notion that CBT might be most effective when focusing on distress and quality of life. Many individuals with persecutory thinking feel under threat and mitigate this in various different ways, and threat monitoring and hypervigilance can lead to (or perhaps in some cases are the same as) insomnia. Although direct intervention in insomnia will still be a valid therapeutic approach, for some patients, it might increase the perceived threat. In this regard, there might be important subgroups in the case of any effect of the intervention on psychosis, and the possibility of distinct responder and non-responder subgroups could be examined in the next trial.

Finally, the large variation in effect sizes by the method and dimension of sleep assessed in the present study was intriguing, with the Insomnia Severity Index having the largest effect size and actigraphy (total sleep time) the weakest. What was the cause of this variation and what do the secondary sleep outcomes measure that the
primary one does not? Consideration of these questions will no doubt be useful in planning of the next trial.

In conclusion, Freeman and colleagues’ new-wave, high-quality, feasibility trial fully deserves a definitive randomised controlled trial and, like all good science, raises as many questions as it answers.

Max Birchwood
Division of Health and Wellbeing, Warwick Medical School, University of Warwick, Coventry CV4 7AL, UK
m.j.birchwood@warwick.ac.uk

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Improved identification of people at risk of psychosis: is it value for money?

Development of a psychotic illness, including schizophrenia, can be debilitating for the individual, with life expectancy reduced by up to 15 years, mainly due to an increased occurrence of cardiovascular disease. In the past 5 years, clinician preference has moved towards early detection and intervention by assessment of the risks for developing psychoses, with increasing evidence about the effectiveness of early interventions for psychosis. Primary care can play an important part in the early identification of individuals at risk, because people with a serious mental illness have an estimated average 13–14 consultations with their general practitioner every year. However, evidence is scarce with respect to the assessment of factors contributing to the effectiveness of improved detection of individuals at high risk of developing psychosis in primary care. Additionally, in an era of restricted health-care budgets, the assessment of cost-effectiveness for this type of intervention is important. Cost-effectiveness assessments attempt to quantify the trade-off between improved outcomes for the individual and increased costs to the health-care system; in other words, does the intervention represent value for money?

In The Lancet Psychiatry, Jesus Perez and colleagues report both the clinical effectiveness and cost-effectiveness of a theory-based early intervention to improve liaison between primary and secondary care in UK primary care practices. High-intensity and low-intensity liaisons were assessed (26 practices in high-intensity intervention and 28 in low-intensity intervention), as was practice as usual (50 practices). The authors’ report that practices randomly assigned to the high-intensity intervention referred more individuals for first-episode psychosis to the early intervention services than did the other two practice groups (high intensity vs low intensity; incidence rate ratio [IRR] 1·9, 95% CI 1·05–3·4, p=0·04), although for individuals at high risk of psychosis the increase was not significant. As a result, high-intensity practices referred both more true-positive and false-positive cases of psychosis confirmed after assessment.

The increased referral of individuals at high risk of psychosis suggests that the high-intensity intervention is clinically effective, but what about its cost-effectiveness? Implementation of the high-intensity liaison intervention was estimated to cost £1459 per