Correspondence

IAPT is probably not cost-effective

The recent economic evaluation of an Improving Access to Psychological Therapies (IAPT) service conducted by Mukuria and colleagues is a welcome addition to the evidence base pertaining to this programme. This was a non-randomised comparison but it appears that the authors have used appropriate methods to control for differences between areas. A casual reading of the abstract conclusion would lead one to assume that IAPT is likely to be cost-effective. Indeed, the cost per quality-adjusted life year (QALY) is below the upper threshold used by the National Institute for Health and Clinical Excellence (NICE), and below the lower threshold in a sensitivity analysis where the EQ-5D was used. However, the cost per QALY is somewhat misleading. The most useful results from this study are the cost-effectiveness acceptability curves shown in Fig. 2. Here it is revealed that at the NICE upper threshold of £30 000 per QALY, there is about a 38% likelihood that IAPT is cost-effective, increasing to just over 50% if the EQ-5D is used to generate QALYs. If the lower threshold is used, then there is even less chance that IAPT is cost-effective. The overall conclusion of this paper should be based on Fig. 2 and it should be that on the basis of this study IAPT was probably not cost-effective.


Drop out from treatment in the World Mental Health Survey initiative

We read with interest the study by Wells et al where the important issue of adherence to treatment services has been addressed. Although the study analysed the data generated from the robust methodology of the World Mental Health Survey, which is a landmark in the field of psychiatric epidemiology, it needs to address some of the conceptual issues of treatment adherence particularly relevant to the low-/lower-middle-income countries.

Long-term follow-up and regular treatment is mostly prevalent in high-income countries that have an organised mental healthcare service. In countries having lesser mental healthcare resources, such coordinated provision of treatment is lacking, when treatment is sought from general medical services, the patient is only provided symptomatic relief and neither the provider nor the client has any knowledge about long-term follow-up. Such lack of communication between them is mostly due to deficiency of mental health infrastructure in terms of either quality or quantity. One may argue that traditional or non-conventional modes are the main treatment providers in such countries. But for them often the treatment proceeds on an ‘as and when required’ basis. For spiritual and religious healers the client would often be attached to them in a special bond of faith or gratitude for generations, such as in the guru–chela relationship. In such situations, a question such as ‘Did you complete the full recommended course of treatment? Or did you stop before the [provider] wanted you to stop?’ seems irrelevant. We propose that a little extra effort to standardise this question across different settings would have made the methodology of Wells et al more robust.

Slightly different definitions for mental health treatment drop out have been used in previous studies. The authors have very rightly pointed out that this is one of the reasons for the differences between drop-out rates found in national surveys and corresponding subsamples of the present study. So, if such a ‘slightly different definition’ of drop out influences their rates in high-income countries where the determinants are less heterogeneous, we can obviously assume that its effect on the low-/lower-middle-income countries will be marked.

Although the authors have made elaborate attempts to find the predictors of drop out, they did not take into account many potentially relevant factors related to patient (e.g. stigma, functional impairment, satisfaction with treatment), professional (e.g. communication skills, clinical expertise) and service delivery (e.g. environmental obstacles). Apart from this, the fact that the centres in some countries were not representative of the whole population influenced generalisability of the study. Overall, this unique effort by the authors is praise-worthy and will go a long way in understanding the dynamics of treatment drop outs from a global perspective.


Author’s reply: I thank Basu & Arya for their kind words about our paper and for their reaffirmation of the importance of addressing adherence to treatment. However, although they note that, ‘in countries having lesser mental healthcare resources, such coordinated provision of treatment is lacking’, our results (online
Table DS2) show that coordinated treatment is typically lacking even in higher-income countries. Indeed, the median number of visits in the past 12 months among patients receiving treatment for mental disorders in general medical services is no different in high-income (1.5) than in low-/lower-middle-income (1.4) countries and only slightly higher in upper-middle-income countries (2.1). We also found that the proportion of patients prematurely terminating primary care treatment of mental disorders is quite high in high-income countries (35.4%) as well as in lower-income countries (52.5% for both groups).

Although Basu & Arya consider the World Mental Health question on stopping treatment irrelevant to relationships with spiritual or religious healers, great care was taken in crafting the question sequence in which this question was embedded to be broadly applicable across treatment sectors and countries. The sequence began by asking respondents whether they ever in their life saw any of the professionals on a long country-specific customised list, for problems with their emotions, nerves, or use of alcohol or drugs. Respondents who reported having done so were asked whether they saw each type of professional for such problems in the past 12 months and, if so, number of visits, perceived helpfulness and whether or not they were still seeing the professional for these problems. Only those who said they had stopped seeing the professional were then asked, ‘Did you complete the full recommended course of treatment? Or did you quit before the [provider] wanted you to stop?’ I agree with Basu & Arya that the framing of this question and of the response options may not have been the most natural way to describe an on-going relationship with a spiritual or religious healer, and I agree that customisation might well yield important new information. However, we would expect reports of having ‘stopped’ to be lower-bound estimates of the extent to which care for on-going emotional problems lacked continuity, so the high proportions of patients in lower-income countries who gave such reports are cause for deep concern. Basu & Arya also note correctly that data on reasons for terminating treatment, including stigma, were not reported in the paper. Such data exist in the World Mental Health Surveys and will be presented in future reports.

Psychological therapies in anorexia nervosa: on the wrong track?
Recently, in a randomised controlled trial, specialist supportive clinical management (SSCM) has proven to be more effective than the Maudsley Model of Anorexia Nervosa Treatment for Adults (MANTRA), a treatment specially designed to address the disorder according to a rather complex rationale in comparison with SSCM. Specialist supportive clinical management, originally ‘non-specific supportive clinical management’ administered to a control group in a previous randomised controlled trial, was found to be more effective than two specialised treatments – cognitive–behavioural therapy and interpersonal therapy – and was as effective as these treatments at 5-year follow-up.

Specialist supportive clinical management was originally defined as clinical management and supportive psychotherapy, as revealed by its original definition:


Authors’ reply: We share Gutierrez & Carrera’s frustration about the difficulty in treating adults with anorexia nervosa. However, we disagree with their interpretation of our findings, and several other points they make.

First, in our trial specialist supportive clinical management (SSCM) was not superior to our new treatment, the Maudsley Model of Anorexia Treatment for Adults (MANTRA). In fact, outcomes for both interventions were similar. Moreover, in the
subgroup of patients with lower initial body mass index (<17.5 kg/m² at the beginning of treatment) there was some suggestion that patients receiving MANTRA showed greater weight gain than those receiving SSCM, but this was not statistically significant (P = 0.15) as the study was not powered to detect subgroup differences. Second, the original New Zealand trial – where SSCM compared well against cognitive–behavioural treatment and interpersonal therapy – included many patients who had a relatively mild, less chronic form of anorexia. In this earlier trial, SSCM effects seemed to wane in the long term.¹

Second, contrary to Gutierrez & Carrera’s assertion, there is plenty of evidence that the personality features, neuropsychological profile (thinking style) and aspects of altered socioemotional processing found in anorexia are not just an epiphenomenon of malnutrition but have trait characteristics which are accentuated in the starved state.²

Taken together these findings suggest a definite place for SSCM, especially in the treatment of less severe cases of anorexia. It may be that a more complex treatment such as MANTRA, which is trait-focused and where patients are taught skills that help them to tackle a range of maintaining factors, is more effective in more severe cases. Our trial was too small to tease this out. However, a larger study is now under way that should be able to answer this question.³

To suggest an ‘either/or’ dichotomy between a treatment focus on self or starvation seems remarkably simplistic to us. In fact, if an exclusive focus on reducing starvation was the key curative step in treatment, in-patient refeeding for anorexia should be used much more often, as this reverses poor nutrition most quickly. Yet, in-patient treatment has significant problems: it is unacceptable to many patients and has high relapse rates.

In a large-scale international survey of patients with eating disorders and their families, there was strong agreement between these stakeholders that specialist expertise and personal qualities of staff, expert psychological interventions and nutritional assistance (advice and intervention) combined are the key components of effective treatments and services.³

Clearly, we are a long way away from having a cure for adults with anorexia. Given the very limited evidence base, there is still much to learn about what works for whom and at which stage of illness. The past few years have seen the burgeoning of neuroscience data related to anorexia nervosa, which opens the way to treatments targeted at dysfunctional neurocircuitry.⁴,⁵ Ultimately, we predict that significant improvements in treatment outcomes in adults with anorexia are only going to be achieved through adding such ‘targeted brain-directed’ adjuncts to talking therapies and nutritional intervention.


Spirituality is not bad for our mental health

We note with interest the conclusion of King et al’s study,¹ which states that ‘people who have a spiritual understanding of life in the absence of a religious framework are vulnerable to mental disorder’. A second, equally important finding is that ‘religious people were similar to those who were neither religious nor spiritual with regard to the prevalence of mental disorders, except that the former were less likely to have ever used drugs [...] or be a hazardous drinker’. This lack of difference, as with the key conclusion concerning those who are spiritual but not religious, runs counter to the substantial body of evidence collated by Koenig et al.,²–⁴ who conclude that religion/spirituality are generally associated with better mental health.

King et al point out that ‘the cross-sectional nature of the data means that we cannot attribute cause and effect to any relationship between spiritual beliefs and mental health, and they draw attention to important differences between the UK and North America (where the bulk of previous research has been conducted). The headline conclusion of the study may nonetheless leave professionals and others with the impression that ‘spirituality’ is bad for one’s health, an impression that we believe would be mistaken.

Our post-modern culture is geared increasingly to a way of life that does not question deeply such things as the meaning of birth and death, why we are here and what it is all for. Instead, social norms often emphasise aspiration to goals of material ambition and success. For many, it seems that this can result in estrangement from the most fundamental spiritual needs and values of humankind (a theme that comes up at meetings of the Royal College of Psychiatrists’ Spirituality and Psychiatry Special Interest Group).

With the decline in religious observance, the numbers of ‘spiritual but not religious’ (19% in this study) are rising, and perhaps more so in the UK than in the USA. Wrestling with the deepest questions about life is in the nature of the human condition. However, without a religious faith that can also provide a person with both community and support, the road is long and hard and the journey often a lonely one. Previous research (Pargament,⁵ pp. 111–128) suggests that spiritual struggles have the potential for either good or bad mental health outcomes, and we wonder whether the kind of society in which we are now living is less than supportive of the good outcome.

We know that spiritually informed therapies are effective in the field of substance misuse,⁶ and mindfulness-based approaches derived from spiritual practice are now recommended by the National Institute for Health and Clinical Excellence for relapse prevention of depression.⁶ Further, we believe that spirituality has an important secular dimension which is finding expression in the recovery movement in psychiatry.

We must therefore guard against any misreading of this study by King et al that would suggest spirituality is bad for mental health. We do, however, support strongly research that is able both to delineate causal pathways and provide comparison between the cultures and contexts of the USA and the UK.
Declaration of interest
A.P. and C.C.H.C. are both members of the Executive Committee of the Royal College of Psychiatrists’ Spirituality and Psychiatry Special Interest Group. However, the views expressed in this letter are their own and not necessarily those held by the Group as a whole. C.C.H.C. is an Anglican Priest and Director of the Project for Spirituality, Theology & Health at Durham University.


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Author’s reply: Cook & Powell are surprised that our findings run counter to research conducted in the USA. They are also concerned that people may conclude from our data that spirituality is bad for mental health. Rather than bad, our main finding is that a religious or spiritual life view confers no advantage in terms of mental health. Our results are not so unusual. Although reviews have suggested that religious and spiritual beliefs and practices are associated with better mental health, the evidence often comes from poor-quality studies and effect sizes reported are small. This is not surprising from a theological point of view; the idea that religious people are protected from the impact of life’s difficulties runs counter to the theology of most major world religions. Furthermore, the evidence base that spiritually informed therapies are effective is tiny, partly because funding for trials is hard to obtain and there have been very few well-designed studies. Religious belief and practice has its main impact on health through lifestyle habits (e.g. less consumption of tobacco and alcohol) and social support. Also, as Cook & Powell note, the context in Europe is quite different to that in the USA. Professing a religious or spiritual belief in Europe may be regarded as strange or even derisory. Such beliefs and practice are more mainstream in the Americas, although even there the occurrence of such beliefs is declining.

In findings from a large prospective study across Europe, published after this paper, we have shown again that holding a spiritual or religious life view may be associated with later mental health problems, but that the effects are weak. More importantly, however, we confirmed that there was no mental health advantage for such beliefs. These prospective data give clues to the direction of the association. It seems that holding a spiritual life view predisposes people to depression. As Cook & Powell say, a spiritual search may often be a ‘lonely one’. However, it remains possible that the search for spiritual answers does not itself cause depression; rather, people already vulnerable to depression search for spiritual answers.

Spirituality is not bad for our mental health
Christopher C. H. Cook and Andrew Powell
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