

## Correspondence

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## The case of Stop Smoking Services in England

A recent editorial in *Nature* concerning therapy deficit and the urgent need to invest in research to enhance the effectiveness of psychological treatment is timely and may prove influential for our field.<sup>1</sup> The piece used the example of the Improving Access to Psychological Therapies (IAPT) programme and the lack of resources to establish the causes of variation in outcome across a service that provides access to support to more than 600 000 people. We wish to alert interested researchers to a slightly more positive example: the Stop Smoking Services in England. These were established in 1998 to help address the single largest preventable cause of premature death in the country and now treat 800 000 smokers each year. Research has begun on establishing the aspects of support that account for the very large variation that exists between local services<sup>2</sup> and specialist practitioners within services.<sup>3</sup> The research has found, for example, that group-based treatment is linked to higher success rates than one-to-one treatment or drop-in clinics, and that services which use particular 'behaviour change techniques', such as showing smokers their expired-air carbon monoxide readings to boost motivation to stop, have higher success rates.<sup>4</sup> This work has led to the development of competence assessment and training programmes (e.g. [www.ncsct.co.uk](http://www.ncsct.co.uk)),<sup>5</sup> but it is only a beginning. Because it can make use of routinely collected, national data, this kind of research can continue to be carried out extremely cost-effectively and save many thousands of lives. However, significant additional funding is required to evaluate improved treatment programmes, based on findings such as these, by means of randomised controlled trials. We hope researchers who are interested in enhancing the psychological treatment provided by IAPT and similar programmes can learn from the early progress made on optimising the Stop Smoking Services.

## Declaration of interest

R.W. undertakes research and consultancy and receives fees for speaking from companies that develop and manufacture smoking cessation medications (Pfizer, Johnson & Johnson, McNeil, GlaxoSmithKline, Nabi, Novartis, and Sanofi-Aventis). He also has a share of a patent for a novel nicotine delivery device.

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- 3 Brose LS, McEwen A, West R. Does it matter who you see to help you stop smoking? Short-term quit rates across specialist stop smoking practitioners in England. *Addiction* 2012; **107**: 2029–36.

- 4 West R, Wallia A, Hyder N, Shahab L, Michie S. Behavior change techniques used by the English Stop Smoking Services and their associations with short-term quit outcomes. *Nicotine Tob Res* 2010; **12**: 742–7.
- 5 Michie S, Churchill S, West R. Identifying evidence-based competences required to deliver behavioural support for smoking cessation. *Ann Behav Med* 2011; **41**: 59–70.

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## Nicotine dependence in patients with schizophrenia

We read with interest the important and clinically relevant study by Krishnadas *et al.*<sup>1</sup> Patients with severe nicotine dependence had greater scores on the positive subscale of the Positive and Negative Syndrome Scale (PANSS) and patients with mild–moderate dependence had greater scores on the PANSS negative subscale compared with non-smokers. As rightly pointed out by the authors, this finding is in contrast to a previous study<sup>2</sup> carried out in the same area and to other similar studies.<sup>3</sup> The reason appears to be the use of the Fagerström Test for Nicotine Dependence (FTND), which has limited psychometric properties in patients with schizophrenia. The authors argue for the validity of the FTND in schizophrenia, by citing the article by Weiberger *et al.*<sup>4</sup> Notwithstanding the methodological superiority of the study in terms of presence of a proper control group, the difficulty of using FTND in people with schizophrenia cannot be denied on a pragmatic basis. It has been widely accepted over the past few decades that dependence is a more holistic concept and cannot be attributed only to the amount or duration of smoking. However, Steinberg *et al.*<sup>5</sup> has questioned the relevance of the items of the FTND – such as time to first smoking, difficulty abstaining in forbidden places and frequency of smoking in the first hours after waking up – by means of a factor analysis study in patients with schizophrenia. In fact, modification of the FTND for the serious mentally ill population has been suggested in view of individuals' frequent impairment in judgement and insight. Such a scale was also useful in Krishnadas *et al.*'s study because all the patients were residents of supported accommodation and there was lack of any objective assessment of nicotine use. Moreover, the emphasis on the amount smoked even in a cross-sectional study like this would have better helped to verify the authors' statement that 'those with severe dependence have successfully overcome negative symptoms by increasing their level of nicotine dependence' (pp. 309–310), although a longitudinal study is essential in settling this issue. We advocate the concept of pack-years in this regard.

The authors adjusted the results for many covariants but left out several important variables which may act as important confounders, such as use of smokeless nicotine, other substance use, presence of physical disorders, type of antipsychotics and other psychotropic medications. They have also not mentioned whether the consent from participants was taken or not. The fact that daily dose of medication was greater in the severely dependent group raises the possibility of a pharmacokinetic interaction or indicates the presence of a poor prognosis subtype with neurobiological underpinnings, which should be clarified in future studies. In Krishnadas *et al.*'s study, the majority of patients were smoking to relax, to socialise better or to alleviate their loneliness, anxiety and depressive symptoms. This makes a strong case for a holistic treatment approach, rather than just prescribing anti-psychotic medication, as many of the mentioned attributing factors can be addressed with a multimodal treatment approach.

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- 4 Weinberger AH, Reutenauer EL, Allen TM, Termine A, Vessicchio JC, Sacco KA, et al. Reliability of the Fagerström Test for Nicotine Dependence, Minnesota Nicotine Withdrawal Scale, and Tiffany Questionnaire for Smoking Urges in smokers with and without schizophrenia. *Drug Alcohol Depend* 2007; **86**: 278–82.
- 5 Steinberg ML, Williams JM, Steinberg HR, Krejci JA, Ziedonis DM. Applicability of the Fagerström Test for Nicotine Dependence in smokers with schizophrenia. *Addict Behav* 2005; **30**: 49–59.

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**Authors' reply:** We agree with Basu & Nebhinani that recent studies have questioned the psychometric properties of the FTND in this population. Indeed, as Steinberg *et al* suggest, we may have underestimated nicotine dependence by using the FTND.<sup>1</sup> We acknowledged this shortcoming in the article. We conducted a principal components analysis on our data-set, in accordance with Steinberg *et al*. Our results revealed a two-factor structure similar to that of Radzius *et al*, explaining 53% of the total variance.<sup>2</sup> The first factor reflected the degree of urgency to restore nicotine levels after night-time abstinence, and the second factor reflected the persistence with which nicotine levels are maintained during waking hours, thereby tapping into different domains of nicotine dependence itself. This is in contrast to Steinberg *et al*, who found two factors that were non-meaningful. In addition to other limitations acknowledged by Steinberg *et al*, exploratory factor analysis techniques have a number of methodological concerns. Most importantly, interpreting the results of any exploratory analyses like principal components analysis is heuristic and may not necessarily reflect the truth in the given data.<sup>3</sup> This is probably one of the reasons why studies that have used such approaches have shown inconsistent factor structure for the FTND, even in non-psychiatric samples. Such studies should be interpreted with caution. In addition, as Basu & Nebhinani rightly point out, reducing a complex, overlapping and holistic concept such as dependence to a few simple meaningful factors may not be theoretically correct or possible.<sup>4</sup> At a pragmatic level, a measure such as pack-years (which only measures amount and duration of smoking) may be a useful measure of lifetime nicotine consumption. We are, however, unaware of any studies that have validated the FTND (or its modifications) or pack-years using a gold standard diagnostic criterion for nicotine dependence in the schizophrenia population. The closest we came was Patkar *et al*, who found a significant correlation ( $r = 0.89$ ) between the FTND scores and DSM-IV diagnosis of nicotine dependence.<sup>5</sup> Although it is possible that psychopathology may have affected the FTND scores, in our study, the scale administration was facilitated by two clinicians (S.S. and S.T.) thereby lending some objectivity to the measurement.

All participants gave written informed consent. We considered antipsychotic type as a covariate in the model. With regard to other potential confounding factors, our relatively small sample

size meant that we did not have enough power to stratify the sample or to add more covariates into the model. It should, however, be noted that adding variables that may themselves significantly covary with nicotine dependence (independent variable) – such as smokeless nicotine/substance use and physical comorbidity – would, in view of controlling for their effects, have decreased the variance explained by nicotine use itself and therefore have been deemed inappropriate in this setting.<sup>6</sup>

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## Methodology and reporting of systematic reviews and meta-analyses

In their study, Brugha *et al*<sup>1</sup> discussed the search strategies employed by the compilers of the systematic reviews and meta-analyses that they analysed. We wish that they had pursued this issue in more detail.

Brugha *et al* wrote that 'Authors generally gave comprehensive details of search strategies employed, including details of electronic databases searched, exact search terms, dates covered by search and other methods used' (p. 447). In examining many systematic reviews and meta-analyses of psychiatric literature in the course of our work with the PILOTS Database, an online index to the worldwide literature of post-traumatic stress disorder (PTSD) that we produce at the National Center for PTSD, we have often observed the inadequacy of the search strategies described by their authors. It is evident that few of these studies have made proper use of the controlled indexing vocabularies used by databases such as MEDLINE and PsycINFO or displayed evidence that the thesauri in which these controlled vocabularies are published have been consulted. The reader familiar with these tools will often have reason to question the reliance that can be placed on systematic reviews and meta-analyses whose authors have not consulted them.

In Lerner & Hamblen,<sup>2</sup> we explain in detail the importance of properly using controlled vocabularies in the compilation of systematic reviews and meta-analyses, demonstrate problems that may arise from not doing so, and offer suggestions for improving the literature searches underlying these compilations.

Declaration of interest

The PILOTS Database is produced by the National Center for PTSD.