Tobacco smoking and depression: results of a cross-sectional study

A recent study by Boden et al. concluded that there is a cause–effect relationship between cigarette smoking and depression, in which tobacco use increases the risk of symptoms of depression. In a large longitudinal study, Kang & Lee showed that smoking caused depression. Shahab & West reported evidence from a cross-sectional survey that ex-smokers feel happier following cessation.

These results may have very important clinical consequences – if smokers can be reassured that their mood can be improved after smoking cessation, it could motivate patients in their attempts to quit. Our own data are consistent with such findings and with the current literature regarding the relationship between depression and smoking status as well as gender. We performed an investigation focusing on depression symptoms among 1021 unrelated blood donors categorised as former smokers, current smokers and never smokers. The sample distribution was former smokers, n = 131; current smokers, n = 254; and never smokers, n = 636. Former smokers were individuals who had reached 6 months of tobacco abstinence. Using a cross-sectional design, the participants were selected during the period from October 2004 to August 2008. Inclusion criteria were: to be Brazilian of European descent, ≥18 to ≤65 years old, male or female and eligible for blood donation. Exclusion criteria included other addictions, current use of any psychopharmacological medication and major psychopathologies, except major depressive disorder. All participants completed a standardised self-report questionnaire that included demographic characteristics and a smoking history. Depression symptoms were evaluated by the Portuguese version of the Beck Depression Inventory (BDI). The BDI scores were analysed as a continuous measure or as a cut-off of ≥15 indicating depressive symptoms.

Level of education was higher among never smokers (n = 164, 25.8%) compared with current smokers (n = 40, 15.7%) and former smokers (n = 24, 18.3%) (χ^2 = 21.56, P < 0.001). This suggests that current and former smokers might share a premorbid behavioural profile different from never smokers. More current smokers had a BDI score ≥15 (current smokers, n = 38, 15.0%; never smokers, n = 47, 7.4%; former smokers, n = 9, 6.9%; χ^2 = 13.43, P = 0.001). Average BDI scores were also higher among current smokers (mean 7.4, s.d. = 7.8) compared with never smokers (mean 5.2, s.d. = 6.5) and former smokers (mean 5.0, s.d. = 5.6) even after adjustment for gender, age and years of schooling (F = 10.93, P = 0.001). There were no significant differences between former and never smokers on depression indices. There was no significant interaction between smoking status and gender – that is, females had higher depression scores than males, regardless of smoking status, pointing to the cross-gender validity of the association. Beck Depression Inventory scores were significantly correlated with Fagerstrom Test for Nicotine Dependence scores (r = 0.16, P = 0.01) and average daily number of cigarettes smoked (r = 0.16, P = 0.01). The results of our relatively large sample suggest that depression scores are lower among former smokers, despite the similar profiles in other characteristics such as education and gender across all three groups.

This issue has been raised by other authors. Wu & Anthony verified in a longitudinal study that although smoking increased the risk for depression, antecedent depresed mood was not associated with later cigarette smoking. A review by the National Institute of Mental Health pointed out the danger posed by over-reliance on the self-medication hypothesis. According to the authors, this misconception may have led to a grossly inadequate attention to tobacco-smoking in mental health settings. Munafò et al. have suggested a causal relationship between cigarette smoking and depression.

The interpretation of our results should be cautious, since cause–effect relationships cannot be explained in cross-sectional studies, where recall bias is always a possibility. Former smokers may differ from current smokers both in terms of their primary depression and nicotine dependence severity. As Fagerstrom & Furlberg pointed out, less dependent smokers may quit more easily and remaining dependent smokers may need more intensive treatment. Another scenario is that previous depressive symptoms might have predisposed some individuals to smoke, and when symptoms faded, they stopped smoking.

Our preliminary results are consistent with these findings, suggesting that former smokers have a better mood than current smokers. If confirmed in future follow-up studies, this evidence will certainly stimulate new approaches for smoking prevention in adolescence and smoking cessation techniques for adults. If smokers can be reassured that their mood may actually improve after smoking cessation, once the withdrawal syndrome has ended, this knowledge could motivate patients in their attempts to quit. We agree with this position and suggest that it is equally valid for both genders.

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Does medication benefit the long-term psychiatric outcomes of children with ADHD?

Langley and colleagues' reported 5-year follow-up outcomes of young children with attention-deficit hyperactivity disorder (ADHD) and the maternal and social factors related to the prognosis. The findings provide evidence of high comorbidity of antisocial behaviours associated with ADHD, drawing attention to the long-term outcomes of the disorder. Yet, in my opinion, additional information needs to be clarified regarding the findings.

The authors showed that medication use was not significantly associated with conduct disorder diagnosis or other antisocial behaviours. However, this interesting result was not discussed in detail in the article. What I am interested in is whether medication could reduce the risk of developing psychiatric diseases. Recently, studies have shown that treatment with stimulant drugs for ADHD could reduce the risk for some psychiatric disorders. In a systematic review, Wilens et al. reported that medication in childhood was associated with a reduction in the risk for subsequent substance misuse. Both studies indicate that medication can benefit psychiatric outcomes. In Langley et al.'s study, most of the participants (63%) received prescribed stimulant drugs, but the psychological outcomes were not optimistic regarding the prognosis of conduct disorder. Does this result suggest that medication is not beneficial for children with ADHD in the long term? What can account for it? In addition, why did children who were prescribed medication have more ADHD symptoms than those no longer using medication?1


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Authors’ reply: We agree that the influence of prescribed medication on the long-term psychological outcomes associated with ADHD is an interesting and important area of research. However, we regret that our study is not best placed to address these issues.

Our study utilised a naturalistic design, identifying children recently diagnosed with ADHD through child and adolescent mental health services and paediatric clinics in the UK. As such, no restrictions or controls were placed on the prescription or continuation of stimulant medication in this group. To adequately test the questions posed by Dr Yang, specifically designed trials are required – well beyond the scope of our article.

Our findings indicated that prescription of medication at follow-up was associated with higher rates of ADHD symptoms, but not with the other psychological outcomes we assessed (including conduct disorder and substance use). Because our study does not provide sufficient data on stimulant use over time and because the majority (90%) were prescribed stimulant medication at some point, we did not expand further on the reasons for these findings, nor can we speculate on why those prescribed medication at follow-up had more ADHD symptoms.

We are therefore grateful to Dr Yang for highlighting this important area for research, but regret that we cannot address these queries using our data.

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Erasing trauma memories

Recent elegant research has raised the salient issue of altering traumatic memories and its treatment implications. Kindt et al. suggest that ‘if emotional memory could be weakened or even erased, then we might be able to eliminate the root of many psychiatric disorders, such as post-traumatic stress disorder’. In a similar vein, Schiller et al. reported that ‘fearful memories can be wiped out for at least a year using a drug-free technique’. The prospect of erasing distressing memories is indeed compelling and has led to widespread media coverage.

However, this issue elicits important ethical and clinical considerations: first, would we want to erase trauma memories, and second, is it clinically helpful to erase such memories?

Loss of knowledge about the past or oneself may be ethically problematic, although reducing suffering clearly may take precedence. Our sense of self is constructed from autobiographical memories, and the authenticity of how they link and our trust in this narrative is important for well-being. Furthermore, losing memory can compromise a victim’s ability to provide legal evidence: autonomy and beneficence may trump justice, but it would be better if the evidence could be used and the victim did not suffer.

Paradoxically, erasing memories of trauma may not in itself reduce suffering and could even lead to the reverse. In clinical cases where explicit memory of an event has been lost, for example owing to a severe head injury or drug rape (e.g. via flunitrazepam), extreme distress can ensue. The clinical literature suggests that avoidance of trauma memories is associated with worse rather than improved outcome.

We note that the data in the above papers do not in fact indicate memory ‘erasure’. Rather, both studies found that fear
responding (the emotional component of the memory) was reduced while declarative memory (knowledge about the event) was left intact. The data therefore point towards easing the pain but not the knowledge of the trauma memory: an important ethical and clinical difference.

We need to challenge the erroneous public perception of a science seeking to ‘erase’ painful memories as such media headlines are obscuring the true interpretation of the data and what treatment development seeks. Such consideration may help prevent us from inadvertently misleading people (especially those who have suffered trauma) to believe that we are pursuing an ‘eternal sunshine of the spotless mind’.


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Early intervention in psychosis

Professor Singh is the expert on early intervention services and provides a characteristically scholarly and elegant reappraisal. 1 Although I am surprised that the Lambeth Early Onset (LEO) 2 and OPUS 3 trials are interpreted as quite so definitive (when most of us read them as very promising but far from conclusive), I am reluctant to disagree with an admired colleague in his area of expertise. However, I must take issue with one conclusion in his ‘future directions’. Singh argues that generic community mental health teams (CMHTs) have no evidence for them and that ‘The logical next step in the move from institutions to community is from generic community teams to specialist teams’. In this I believe he is mistaken.

Community mental health teams suffer from having evolved before the era of extensive mental health services research. Nobody ‘owns’ them, so few have actively researched them; they have most often been the comparators in randomised controlled trials of other innovative specialist teams. Despite this, research-based conclusions can be drawn about their comparative effectiveness. The body of assertive outreach research is overwhelmingly greater than for any other specialised team. What a series of over 60 assertive outreach team trials shows is that reductions in in-patient care are more highly dependent on the nature of the comparator services than the experimental services. 4 Where these comparator services are poor and fragmented there is a substantial reduction; where they are not, then there is little or no reduction. Often this has been where the comparator is a generic community team. 5

We have rather myopically interpreted these findings as a failure to demonstrate superiority of the specialist team over CMHTs. However, ‘As health services enter a period of economic austerity’, 6 we need to recognise that the findings tell us much more than that. What they demonstrate is that generic CMHTs have routinely matched the specialist teams in major outcomes yet for a significantly lower cost. 7 They are, in short, more cost-effective and therefore currently our best buy.

Experimentation and innovation in specialised teams must continue if we are to progress. However, if we conduct research we must pay attention to its findings, no matter how unwelcome. The current evidence supports the superiority of CMHTs, no matter how much that they may grate.


Author’s reply: Professor Burns rightly reminds us that, unlike specialist teams, community mental health teams (CMHTs) have never had strong advocates and have not been actively researched. His point about the wide variation in CMHT outcomes as comparators in trials illustrates this: lacking a clear role, responsibilities and remit, CMHTs have struggled to delineate what they do well, shed what they do not, and ensure that their staff keep up with the changing evidence base for therapeutic interventions. Specialist teams do not do anything special which is out of CMHT reach. Specialist teams are simply better placed to engage patients and deliver high-quality interventions because of the specialist focus that allows clinicians to develop and hone specialist skills. This is the history of improvements in medicine, where specialisation is both an outcome of academic advance and a vehicle for service improvement. It is in the nature of general teams to deliver generic care; there is no evidence that pouring extra resources into CMHTs would turn them into specialist equivalents.

The latest National Institute for Health and Clinical Excellence (NICE) guidelines on schizophrenia reviewed the clinical and cost-effectiveness of CMHTs and concluded:

‘Despite the fact that CMHTs remain the mainstay of community mental healthcare, there is surprisingly little evidence to show that they are an effective way of organising services. As such, evidence for or against the effectiveness of CMHTs in the management of schizophrenia is insufficient to make any evidence-based recommendations’ (p. 336). 1

The health economic review adds:

‘The available evidence on health economics is unclear. The non-significant differences between standard care and CMHTs, and between pre-intervention period and intervention period, suggest that CMHTs provide no real cost savings or extra costs’ (p. 337). 7

Reluctant as I am to disagree with an esteemed colleague, there is little evidence to support the superiority of CMHTs over specialist teams. What they demonstrate is that generic services are as good as, if not better than, the best specialist teams.

Our understanding of mental disorders and the complexity of treatment has moved on considerably from the time when CMHTs were originally established. In this rapidly changing world, it is difficult to see how generic teams can deliver all the recommendations of the 22 NICE guidelines in mental health.
The ‘conflict’ between generalism and specialism is not restricted to psychiatry, it is writ large in the historical development of 20th-century medicine. In the 21st century, CMHTs need to evolve and innovate and carve out a niche where the generalist can flourish, either in primary care or in high-quality, rapid assessment teams that complement specialist services, rather than compete with them.


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Correction

Impact of cannabidiol on the acute and psychotomimetic effects of smoked cannabis: naturalistic study. *BJP*, 197, 285–290. The title of the paper should read: Impact of cannabidiol on the acute memory and psychotomimetic effects of smoked cannabis: naturalistic study. The online version of this paper was corrected in deviation from print and in accordance with this correction.

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