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Is alcohol consumption irrelevant to outcome in anxiety and depression?

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Is alcohol consumption irrelevant to outcome in anxiety and depression?

Boschloo and colleagues¹ report that DSM-IV alcohol abuse is not a risk factor for unfavourable outcome in depression and anxiety disorders. This finding, which ostensibly conflicts with clinical experience, could easily be misunderstood to imply that non-dependent alcohol use is irrelevant in these disorders. We do not believe this is a true finding of the study. The paper addresses DSM-IV alcohol use disorder diagnosis at baseline as a risk factor for poor outcome. It does not address continuing alcohol abuse and its effects on depression and anxiety. Latest alcohol consumption statistics show that a substantial proportion of the population drink alcohol heavily. In England, the latest estimates are that 26% of men and 17% of women over the age of 16 drink at levels that are hazardous or harmful to health.² Clearly, only a small proportion of these receive a diagnosis of alcohol use disorder and there is no reason to believe that the situation in The Netherlands is radically different. It would be expected that a significant proportion of the sample with no alcohol use disorder diagnosis would be drinking alcohol at hazardous or harmful levels. Alcohol use disorder is therefore a poor marker for alcohol consumption. Alcohol use disorder diagnosis cannot have an effect on the course of depressive or anxiety disorders as it is a theoretical construct. It is alcohol consumption that has an effect. The current data are extracted from a larger study with different aims^{3,4} and contain a number of flaws when used to examine the effects of alcohol on depression and anxiety. There are no indications of levels of alcohol consumption other than alcohol use disorder diagnosis and there are no data on alcohol diagnosis at follow-up. There is no information about what treatments the sample patients received, although this is likely to have included advice to reduce alcohol consumption. Unsurprisingly, the study demonstrates that those with the poorest depression and anxiety outcomes are those with a baseline diagnosis of alcohol dependence, with a stronger effect with increasing severity of disorder. That is, those patients least likely to have reduced their alcohol consumption during the study had the poorest outcomes. There is a puzzling finding in relation to severity of alcohol diagnosis: there are more patients in the sample with the more severe diagnosis of alcohol dependence than with the less severe diagnosis of alcohol abuse. The diagnostic criteria for alcohol abuse are concerned largely with social dysfunction, whereas those for alcohol dependence are concerned with physical dependence and social dysfunction. It may be that the gap between the onset of moderate social dysfunction and the onset of physical dependence or severe social dysfunction (as determined by the diagnostic criteria) represents a small part of the deterioration resulting from sustained heavy drinking, and

therefore is a poor indicator of population harmful drinking. The paper does convincingly suggest that severe alcohol dependence at baseline is a risk factor for poor outcome in anxiety and depression. It does not demonstrate that continued drinking in other groups is irrelevant to outcome.

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Authors' reply: We appreciate Bailey *et al*'s interest in our study¹ and respond to their comments regarding the found differential effects of alcohol dependence *v.* alcohol abuse and alcohol consumption.

It is evident that heavy alcohol consumption has a major impact on public health because of its negative consequences on the onset and course of various diseases.² However, this does not necessarily imply that the level of alcohol consumption is also causally related to the onset and persistence of psychopathology such as depression and anxiety. For example, Haynes *et al*³ showed that heavy alcohol consumption was not a risk factor for the onset of depression and anxiety in a national sample of adults in Great Britain. Furthermore, additional analyses in our sample showed that neither heavy alcohol consumption at baseline (odds ratio (OR)=0.98, 95% CI 0.71–1.36) nor at 2-year follow-up (OR=0.98, 95% CI 0.69–1.41) predicted the persistence of depression and anxiety.

In contrast, DSM-IV alcohol dependence has strong links with depression and anxiety. For example, previous epidemiological studies have demonstrated robust cross-sectional associations^{4,5} and, in addition, our study showed that alcohol dependence at baseline was an important risk factor for the persistence of depression and anxiety during 2-year follow-up (OR=1.69, 95% CI 1.04–2.75, adjusted for basic covariates).¹ Additional analyses further showed that this association remained significant after controlling for the possible effects of alcohol consumption (OR=2.05, 95% CI 1.19–3.53, additionally adjusted for alcohol consumption). This indicates that impairments related to alcohol dependence, rather than consumption *per se*, have an effect on the course of depression and anxiety. Support for this hypothesis was also provided by our finding that persistence rates of depression and anxiety increased with the severity of alcohol dependence (i.e. number of alcohol dependence criteria).¹

Previous studies have raised the question whether DSM-IV alcohol abuse should be considered a genuine psychiatric disorder as it was characterised by low severity in the general population and showed limited reliability and validity (see Boschloo *et al*¹ for a discussion on this topic). This notion received further support by our finding that 95.7% of patients with alcohol abuse met only one of four abuse criteria.¹ Additional analyses showed

that alcohol abuse was not related to impairments in daily functioning (assessed with the World Health Organization Disability Assessment Schedule II) and had a favourable course (remission rate after 2 years: 93.5%). Apparently, alcohol abuse is characterised by only minimal alcohol-related impairments, which may clarify our finding that it was not related to the persistence of depression and anxiety. Note that alcohol abuse in our sample of out-patients with depression or anxiety or in the general population might differ substantially from alcohol abuse in clinical samples of severe abusers with regard to associated levels of impairment.

Taken together, these findings emphasise the importance of severity indicators, rather than dichotomous diagnoses of alcohol dependence or abuse, in the assessment of alcohol problems. This is in line with the proposal of the DSM-5 Work Group to distinguish different levels of severity within the diagnosis of an alcohol use disorder (www.dsm5.org).

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Depression in the workplace: what is depression?

As a former consultant occupational psychiatrist to the Metropolitan Police and a contributor to the Black & Frost report,¹ I would like to point to the basic flaw in the paper by Gilbody *et al.*²

In Gilbody *et al.*'s account of 'depression in the workplace', they take 'depression' as a given, requiring no further explanation, yet there is currently no psychiatric category so bloated and subject to overdiagnosis. My experience of 900 assessments of individuals at the Metropolitan Police, 99% of whom were certificated as being 'off sick' with depression or one of the other 'common mental disorders', showed that the psychiatrisation of non-specific symptoms led routinely to unduly prolonged sickness absence and thus unnecessary disability.³ Sickness absence was strongly associated with workplace problems and other situational stresses: dislocation to a psychiatric arena frequently paralysed the

practical problem-solving that would have normalised the situation in timely fashion. I found that National Health Service (NHS) mental health services were disconnected from the occupational aspects of patients' lives, and saw diagnosis and treatment as having a life of its own. Antidepressants and periodic out-patient appointments did not seem a model that got many people back to work. Naturally, I support Gilbody *et al.*'s advocacy of closer relations with the workplace but NHS services will continue to be as much part of the problem as of the solution without a review of the narrowly biomedical culture of much psychiatric practice.

Last, Gilbody *et al.* see hope in the Layard Improving Access to Psychological Therapies (IAPT) programme. It is laughable hubris for the profession to imagine that large numbers of very heterogeneous people, many of whom have been out of the workplace for years, can be gathered together under the rubric of 'common mental disorder' and restored to full productivity via a few sessions of talk therapy.

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Severe mental illness in prisoners worldwide

We read with interest the review article by Fazel & Seewald.¹ The authors conclude that severe mental illness is more prevalent in prisoners in low- and middle-income (LAMI) countries than in high-income countries. This may be related to fewer opportunities and services for diverting offenders to health services, a stronger relationship between mental illness and criminality, and different sociocultural factors (e.g. poorer legal representation for the mentally ill in LAMI countries).

We examined in an earlier study the relationship between mental illness and criminality in pre-trial reported Antillean defendants in The Netherlands and The Netherlands Antilles.² Defendants who are suspected to have a mental illness are examined pre-trial by a psychiatrist or psychologist both in The Netherlands and The Netherlands Antilles, before diversion to mental health services takes place. We found no significant difference in the prevalence of psychotic disorders among pre-trial reported Antillean suspects in The Netherlands and The Netherlands Antilles (14.4% *v.* 15.1%) and no significant difference in the prevalence of defendants deemed unaccountable due to a mental disorder (3.9% *v.* 4.0%). There was, however, a much lower registered crime rate of Antilleans in The Netherlands Antilles than The Netherlands (11 *v.* 113 offences per year per 1000 persons; $P < 0.001$) and a much higher rate of pre-trial reports in The Netherlands Antilles than The Netherlands (74 *v.* 8 per 1000 Antillean defendants; $P < 0.001$). Antilleans living in The Netherlands Antilles thus have a low crime rate and a high pre-trial report rate when indicted of a crime compared with Antilleans in The Netherlands, indicating that they are more frequently suspected to have a mental illness.

This finding is in line with the earlier formulated rule of thumb that the frequency of mental illness is higher in countries with low crime rates.^{3,4} Indeed, the LAMI countries included in Fazel & Seewald's review have a combined prison population rate

of 77 per 100 000, considerably less than the worldwide prison population rate of 145 per 100 000.⁵ The higher prevalence of severe mental illness in prisoners in LAMI countries may be explained by lower rates of crime and imprisonment. If so, the relationship between mental illness and crime is not stronger in LAMI countries, but weaker in high-income countries.

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Correction

Negative childhood experiences and mental health: theoretical, clinical and primary prevention implications. *BJP*, 200, 89–91. Reference 5 should read: Kingdon D, Sharma T, Hart D, the Schizophrenia Subgroup of The Royal College of Psychiatrists' Changing Minds Campaign. What attitudes do psychiatrists hold towards people with mental illness? *Psychiatr Bull* 2004; **28**: 401–6.

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