Antipsychotics and borderline personality disorder

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I am surprised that there were no randomised controlled trials (RCTs) available at the time of study on the usefulness of quetiapine, although some RCTs of aripiprazole and olanzapine were. A few open-label studies have been done highlighting the usefulness of quetiapine in reducing impulsivity and affective symptoms, and it is evident in clinical practice that it does have some beneficial effects on mood instability and aggression.

It is a pity that forest plotting could not be done, which would have shown how much variation existed among studies and the degree of precision of each study, although one can understand the various difficulties faced by the authors.

Lastly, I would like to seek clarification regarding somewhat conflicting statements in the paragraph ‘Implications for practice and research’; it initially states ‘nor can low-dose antipsychotics be advised for cognitive–perceptual symptoms as earlier recommended by the American Psychiatric Association Practice Guidelines’, but later states ‘the SGAs (aripiprazole, olanzapine) should be the first choice for treating cognitive–perceptual symptoms’. The authors state that ‘there was no significant difference between case and control groups needs to be carefully considered.’

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of the participants and I would respectfully highlight that this statement does not seem consistent with the information provided in the accompanying table of sample characteristics. This table states that 58.4% of cases and 43.2% of controls were unemployed. The percentages in this table have some inaccurate rounding but more worryingly, contrary to the authors’ report, there is a clear statistically significant difference ($P = 0.001$ using a $z$-test for proportions).

This also seems to be a highly relevant and clinically significant difference that may have introduced considerable bias into this study and merited the attention of the 14 authors. In the discussion the authors state ‘the increased availability of skunk cannot alone explain why our control group members are less likely to prefer higher-potency types than the cases group across time’. The requirement to hold down a job may be a highly significant reason why controls smoked cannabis of lesser potency less often than the unemployed. Moreover, individuals who are unemployed are highly likely to have poorer social and health status, which further serves to obscure the true role of cannabis in this study.


Authors’ reply: Among the sociodemographic variables we reported in Table 1, it is correct to point out that unemployment rates are statistically significantly higher in the cases compared with controls ($P < 0.001$). This difference has already been reported in previous epidemiological studies and there is no evidence that this arises from a bias in the sample selection. However, it is rather a potential confounder. In our paper we did not discuss if or how employment status might have influenced our findings, because, together with other relevant variables, we controlled for it in the statistical analyses. Thus, the higher rate of unemployment in cases than controls might partially account for the drop of the crude odds ratio (OR) of 8.1 (95% CI 4.6–13.5) to the adjusted one (OR = 6.8, 95% CI 2.6–25.4), which occurred when we controlled for confounders including unemployment. However, the odds ratio still remains strikingly high and statistically significant ($P < 0.05$), indicating that our findings cannot be explained by the effect of employment status or by any of the other social variables listed.

Lastly, we wish to comment on the suggestion that controls’ preference for low-potency cannabis might be consequent to their need to continue being able to work. Would this not indicate that high-potency cannabis is more likely to negatively affect social functioning perhaps via its detrimental effect on mental health? Exactly what our findings suggest.

Marta M. Di Forti, Department of Psychiatry, Institute of Psychiatry, De Crespigny Park, London, UK. Email: martadiforti@kcl.ac.uk; Craig Morgan, Robin M. Murray, Institute of Psychiatry, London, UK. doi: 10.1192/bjp.196.4.333