

Authors' reply: The letter from Rodger *et al* uses our editorial to rehearse the well-worn arguments of the small group of so-called 'critical psychiatrists' who are active and vocal in criticising core aspects of the practice of psychiatry as a medical subspecialty underpinned by science. The views expressed in the letter are mainly tangential to the views we expressed in our editorial and the authors have made assumptions and accusations that are unsupported by our text.

We are very keen to encourage informed and constructive debate to advance patient care and mental health. However, it is important to make a distinction between the freedom that is properly enjoyed in academic debate and the responsibilities that come with professional practice. At present, those who work as psychiatrists are expected to practise in accordance with evidence-based standards. The standards we adhere to will of course change over time as the evidence base develops. This is expected by patients and colleagues and required by regulators.

We continue to believe that our patients are best served by seeing psychiatrists who are trained to make a thorough assessment, come to a diagnosis and shared formulation with the patient of their problems and use this to draw up an evidence-based management plan. It seems strange to us that this should be surprising, contentious or upsetting to the authors of the letter.

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Concerns regarding an evaluation of MTFC-A for adolescents in English care

We are writing to highlight concerns regarding conclusions offered by Green *et al* in their evaluation of Multidimensional Treatment Foster Care for Adolescents (MTFC-A) relative to usual care for at-risk youth in English foster care.¹ We commend the authors for undertaking an independent review of MTFC-A. However, we offer some observations to help contextualise the efficacy of the evaluation with respect to the primary conclusion that MTFC-A did not result in better outcomes than usual care.

Green *et al*'s evaluation employed a two-arm, single-blinded (assessor) randomised controlled trial embedded within an observational quasi-experimental case-control study. An intent-to-treat (ITT) analysis was employed specific to the MTFC-A versus usual care comparison. The authors state that the study was intended to be powered at $\beta=0.80$ to detect half a standard deviation difference between ITT and usual care (with a target n of 130), and was powered $\beta=0.95$ to detect the same effect between ITT and usual care in the quasi-experimental study (with a target n of 90). However, the target allocation for the trial was not met. The trial randomly allocated only 34 participants ($n=20$ MTFC-A and $n=14$ usual care). Based on these numbers, we estimate the study was actually powered at $\beta=0.29$ in the ITT analysis to detect half a standard deviation difference between conditions assuming equal variances, and at $\beta=0.28$ assuming unequal variances.

Substantive conclusions therefore seem to be based on a substantially underpowered design (as far as we can tell from the detail presented in the original manuscript). Further, the quasi-experimental arm was described as a case-control design. However, it was not a matched case-control design. This is evident from multiple baseline differences between groups, some of which remained after an intensive set of propensity-score

weights was applied and after elimination of cases with probability of assignment to MTFC-A above 0.95 and below 0.05. Depending on the distribution of assignment probabilities, this may have resulted in relatively limited 'data trimming' in order to attain desired allocation probabilities near 0.50. The observed differences included not only age but also the primary outcome scores.

Notwithstanding concerns regarding statistical power for the trial, the authors reported intervention by baseline risk interactions in the only adequately powered arm of the study (see Table 5). Given prior demonstration of MTFC-A intervention by baseline risk interactions,² these results may have been more appropriately presented as a hypothesised replication. Statistical power is also a concern for the reported analyses of offending; $\beta=0.034$ to detect the observed ITT odds ratio of 1.24 using an allocation of 20 and 14 cases, and $\beta=0.031$ in the quasi-experimental arm to detect the observed ITT odds ratio of 1.07 with 93 and 92 cases. Interpretation of effects should therefore be treated with caution.

We raise one additional point of clarification regarding prior MTFC-A implementations. The authors state that the context of intervention in the UK differs significantly from that in the originating US studies, since 'these were focused on convicted delinquent youth where the alternative [to MTFC-A] was incarceration', thereby concluding that the 'control condition in the US studies approximated [...] to juvenile custody'. Actually, similar to the usual care condition in the Green *et al* study, the standard control condition in US MTFC-A studies is group care,³ not incarceration.

We offer these points by way of lending interpretation to the efficacy of Green *et al*'s results and to suggest caution in accepting the conclusion that MTFC-A may not result in better outcomes than usual care among at-risk adolescents in English care.

- 1 Green JM, Biehal N, Roberts C, Dixon J, Kay C, Parry E, et al. Multidimensional Treatment Foster Care for Adolescents in English care: randomised trial and observational cohort evaluation. *Br J Psychiatry* 2014; **204**: 214-21.
- 2 Leve LD, Chamberlain P, Smith DK, Harold GT. Multidimensional treatment foster care as an intervention for juvenile justice girls in out-of-home care. In *Delinquent Girls: Contexts, Relationships, and Adaptation* (eds S Miller, L Leve, P Kerig). Springer Press, 2011.
- 3 Chamberlain P. *Treating Chronic Juvenile Offenders: Advances Made through the Oregon Multidimensional Treatment Foster Care Model*. American Psychological Association, 2003.

Conflict of interest: The authors have collaborated with US colleagues on projects using the MTFC-A programme.

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Authors' reply: Harold & DeGarmo correctly refer to points regarding sample size and power that we already made in the discussion section of our paper. Despite this, we did point to the strengths of the study in the representativeness of the cohort within a real-world implementation setting, the fact that the study was conducted independently of treatment originators and UK implementation team, careful attention to triangulation and masked rating of primary outcome data (something often not undertaken in this kind of context), and the low attrition rates to endpoint. We stated that the convergence of findings from our mixed-method design and the confidence intervals of the outcome estimations gave some confidence to inferences from the results.

Harold & DeGarmo also question whether there was indeed a difference in the standard control condition (usual care) for participants in the US and UK studies. There are certainly likely to be differences in the nature and uses of group care between the two countries, given the differences in their child-welfare and juvenile-justice systems. However, the point we were making is that, in the USA, the MTFC programme for adolescents has been principally found to be successful when targeted at young offenders, in studies that have used a variety of measures of recorded reoffending to assess its effectiveness.^{1–3} This emphasis on the effectiveness of MTFC-A with young offenders is also clear from the programme developers' own website (www.mtfc.com). By contrast, the participants in our study were young people with complex emotional and behavioural difficulties, 93% of whom were in care because of abuse or neglect and less than a third of whom had a recent criminal conviction. The differences between the populations served by MTFC-A are clearly evident in an article comparing outcomes for high-risk adolescent girls written by the programme developers in the USA and their English colleagues⁴ and may perhaps partly explain why the results of the English evaluation were less positive than those in the USA.

- 1 Chamberlain P. Comparative evaluation of foster care for seriously delinquent youth: a first step. *Community Altern* 1990; **2**: 21–36.
- 2 Chamberlain P, Reid JB. Comparison of two community alternatives to incarceration for chronic juvenile offenders. *J Consult Clin Psychol* 1998; **66**: 624–33.
- 3 Leve L, Chamberlain P, Reid JB. Intervention outcomes for girls referred from juvenile justice: effects on delinquency. *J Consult Clin Psychol* 2005; **75**: 1181–4.
- 4 Rhoades K, Chamberlain P, Roberts R, Leve L. MTFC for high risk adolescent girls: a comparison of outcomes in England and the United States. *J Child Adolesc Subst Abuse* 2013; **22**: 435–49.

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Are we reinforcing the anti-medical model?

The results of Penttilä *et al*'s meta-analysis emphasised the importance of the duration of untreated psychosis (DUP) in long-term recovery from schizophreniform illness.¹ Timely initiation of effective treatment has been demonstrated to improve outcome, but the modality of treatment is currently under much debate. Robust evidence exists for the efficacy of antipsychotic medication² but recent studies have proposed psychological interventions, specifically cognitive-behavioural therapy (CBT), as an alternative first-line treatment.

In a recent randomised controlled trial, CBT was used as a single intervention, instead of conventional antipsychotic treatment.³ To our complete surprise, one of the exclusion criteria was treatment with antipsychotic drugs. We wonder how ethical approval was granted, despite Tiihonen *et al*'s robust demonstration of reduced mortality over a considerable follow-up period for patients receiving antipsychotic medication.⁴ We feel that this will set a dangerous precedent of offering psychological treatment as an alternative to evidence-based treatment. In a clinical setting, adherence to drug treatment is already a significant issue and there

is potential to reinforce the idea that antipsychotic medication is harmful and unnecessary. We feel that this would further disadvantage an already vulnerable group of patients.

This issue has recently received a fair degree of coverage in the media, with articles such as Freeman & Freeman's piece in *The Guardian* fuelling long-held popular beliefs that antipsychotics are ineffective and in fact damaging to health.⁵ Given the well-documented drawbacks of antipsychotic drugs, it is understandable that patients and professionals will invest hope in non-drug alternatives. However, a large meta-analysis with over 3000 participants shows at best a small effect size for CBT.⁶ In reference to Penttilä *et al*'s paper, we would be interested to read subgroup analyses of specific first-line treatments and wonder if outcomes would differ between modalities.

While we would endorse any treatment, drug or non-drug based, that is proven to reduce DUP, it is vital that we do not lose sight of the fact that antipsychotics are the only evidence-based first-line therapy in psychotic illness.

- 1 Penttilä M, Jääskeläinen E, Hirvonen N, Isohanni M, Miettunen J. Duration of untreated psychosis as predictor of long-term outcome in schizophrenia: systematic review and meta-analysis. *Br J Psychiatry* 2014; **205**: 88–94.
- 2 Leucht S, Arbter D, Engel RR, Kissling W, Davis JM. How effective are second-generation antipsychotic drugs? A meta-analysis of placebo-controlled trials. *Mol Psychiatry* 2009; **14**: 429–47.
- 3 Morrison AP, Hutton P, Wardle M, Spencer H, Barratt S, Brabban A, et al. Cognitive therapy for people with a schizophrenia spectrum diagnosis not taking antipsychotic medication: an exploratory trial. *Psychol Med* 2012; **42**: 1049–56.
- 4 Tiihonen J, Lannqvist J, Wahlbeck K, Klaukka T, Niskanen L, Tanskanen A, et al. 11-year follow-up of mortality in patients with schizophrenia: a population-based cohort study (FIN11 study). *Lancet* 2009; **374**: 620–7.
- 5 Freeman D, Freeman J. At last, a promising alternative to antipsychotics for schizophrenia. *The Guardian*, 7 March 2014.
- 6 Jauhar S, McKenna PJ, Radua J, Fung E, Salvador R, Laws KR. Cognitive-behavioural therapy for the symptoms of schizophrenia: systematic review and meta-analysis with examination of potential bias. *Br J Psychiatry* 2014; **204**: 20–9.

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Author's reply: Dr Bindman and Dr Kripalani have suggested an analysis of the association between DUP and outcomes in subgroups by specific first-line treatment modalities. Unfortunately, it was not possible to analyse this in our meta-analysis, since none of the original studies had used only one treatment modality, but a combination of them in the early phases of treatment. As Bindman & Kripalani point out, and based on current knowledge of the efficacy of treatments in the early phase of schizophrenia, it would not be ethical to study treatment without antipsychotic medication in a first-episode clinical sample.¹ Also, DUP is usually defined as ending at the initiation of antipsychotic medication, which in clinical practice usually occurs about the same time as other treatment modalities begin; therefore, the included studies give only a little information on the effects of different treatments. However, it is interesting to note that de Haan *et al*² investigated the effect of delay in intensive psychosocial treatment by comparing this effect with delay in treatment with antipsychotic medication; and found that delay in psychosocial treatment may be a more important predictor of negative symptoms than delay in antipsychotic treatment.

The discussion about the possible effects of antipsychotics has been rather intense recently. However, the current guidelines for treatment of psychosis and schizophrenia clearly indicate that