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.

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


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 Tihonen 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.

Author’s reply: Dr Binman 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 al2 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

References


5 Freeman D, Freeman J. At last, a promising alternative to antipsychotics for schizophrenia. The Guardian, 7 March 2014.