Author's response to reviews

Title: Evaluation of multisystemic therapy pilot services: protocol of the Systemic Therapy for At Risk Teens (START) trial

Authors:

Peter Fonagy (p.fonagy@ucl.ac.uk)
Stephen Butler (stephen.butler@ucl.ac.uk)
Ian Goodyer (ig104@cam.ac.uk)
David Cottrell (d.j.cottrell@leeds.ac.uk)
Stephen Scott (stephen.scott@kcl.ac.uk)
Stephen Pilling (s.pilling@ucl.ac.uk)
Ivan Eisler (ivan.eisler@kcl.ac.uk)
Peter Fuggle (peter.fuggle@nhs.net)
Abdullah Kraam (a.kraam@leeds.ac.uk)
Sarah Byford (s.byford@kcl.ac.uk)
James Wason (james.wason@mrc-bsu.cam.ac.uk)
Rachel Haley (rachel.haley@ucl.ac.uk)

Version: 2 Date: 12 June 2013

Author's response to reviews: see over
Dear Editors-in-Chief,

We hereby submit the revised manuscript of our Study Protocol paper “Evaluation of multisystemic therapy pilot services in the Systemic Therapy for At Risk Teens (START) trial: Study protocol for a randomised controlled trial”.

We are grateful to the referees for their helpful and comprehensive reviews of our original submission, and hope that we have addressed their concerns adequately with our revisions. As requested, we have highlighted our changes in response to the referees’ points in the manuscript using Track Changes, and here provide a point-by-point response to their comments.

We very much hope that you will now consider the paper suitable for publication in Trials, and look forward to your decision.

Yours sincerely,

Peter Fonagy (Chief Investigator)
on behalf of the authors

Response to the reviewers

Editorial requests:

1) Please ensure the title conforms to journal style for study protocol articles. The title should follow the format “__________: study protocol for a randomized controlled trial.”

Authors’ reply: We have reformatted the title in line with the Editors’ request.

Reviewer 1

1. The manuscript indicates: "There have been 20 randomised trials of MST [48-54], and while the therapy works exceptionally well sometimes [49-53, 55], it does less well in other contexts [e.g. 56, 57-59].” What are the other contexts? Why might it be working less well? What is it about such contexts? Perhaps the authors mean, "...and while the therapy works exceptionally well sometimes [49-53, 55], it does not do so consistently [e.g. 56, 57-59]."

Authors’ reply: We have reworded this sentence in line with the simple suggestion made by the reviewer.
2. The inclusion criteria seem like they will lead to a rather heterogeneous group of children and families. This is likely a necessary part of evaluating MST in a real-world setting, but at the same time, analyses should be conducted to account for site effects as well as variation in the "types" of children/families recruited across sites (and even within sites, potentially).

Authors’ reply: As identified by the reviewer, we are recruiting participants from different referral routes. This is with the intention of increasing generalisability of the study findings to antisocial adolescents whose problems might become apparent in different contexts (e.g. youth justice vs. education). As pointed out in our revisions, studying a broad group of young people showing serious antisocial behaviour rather than solely those recruited from the youth justice system is a unique aspect of the START trial (p. 12) In doing so, our secondary outcome measures will help us characterise this heterogeneity (p. 11) and our analyses will consider subtypes of young people (e.g., with and without callous-unemotional traits) who receive the interventions.

In exploratory analysis we will analyse between-site differences and examine these in the context of adherence ratings (TAM-R measure). There is no plan to use site as a grouping variable in the analysis and it is anticipated that the standard nature of the intervention and its supervision by an independent organisation will ensure that observed variability is accounted for by the adherence and competence of individual practitioners.

3. The qualitative interviews are a great contribution. Analyses of these data are not clearly outlined, however. Consider qualitative interviews of therapists in both conditions as well.

Authors’ reply: We have included more detail on the scope of the qualitative interviews and how the data from these interviews will be analysed using framework analysis. We agree that it would be very interesting to interview the therapists as well, but we currently do not have the resources to do so and therefore have decided to focus on the experiences of the young people and their families.

4. MST involves a very devoted and involved therapist. Should authorities be contacted by such an entity on behalf of a teen, they may be less likely to devote precious resources to monitoring such a teen: Why bother, when there is already a very involved entity keeping watch? However, a teen randomized to the control condition will likely not have such a visible advocate. In this case, authorities may continue to scrutinize such a youth: There is no one keeping watch, so someone needs to do so. In this case, should results favor MST, is it really MST or is it that a youth in MST is not under direct and intense surveillance by authorities as compared to the control condition?

Authors’ reply: This is an important and complex issue. To date, there have been no studies investigating whether youth in MST receive more or less scrutiny from the authorities than those control conditions. Anecdotal reports from the large, multisite unpublished trial in Canada suggested the opposite (Alan Leschied, personal communication), in that for youths returned to court following MST, judges tended to be harsher in sentencing, reasoning that the young person received a gold standard intervention yet still behaved in antisocial ways.

More generally speaking, the true bulk of the work in MST is with the parent and family, and in building parenting skills and strengths as well as indigenous supports. In applying the MST model,
Therefore, the MST therapist is not an advocate for the youth as much as an advocate for the parent and for the parent’s goals to gain better control over their child’s behaviour.

Finally, from a measurement perspective, we will be complementing our official offending records with self-reported delinquency, which may partly address this factor by obtaining the young person’s own account of the type and frequency of antisocial behaviour that they are committing, including behaviour that goes undetected by the authorities. In addition, we will be carefully documenting police contacts as well as youth justice contacts in both treatment arms, to study whether or not there will be group differences in young people’s subsequent involvement in youth offending services (e.g., probation) and with the police.

5. How will the qualitative data be integrated with the quantitative data?

Authors’ reply: We have included some discussion of this issue towards the end of the Discussion.

Reviewer 2

1. This paper reports on an important Department of Health-funded, multisite RCT of multisystemic therapy (MST) for young people. The description of the trial protocol will be of considerable interest to many academics and clinicians. However, as it stands, this paper is not easy to follow and it could be improved considerably by reorganising the material along a more conventional format. Additionally, some extra detail needs to be added in places. The new SPIRIT guidelines should be of help.

Authors’ reply: We thank Reviewer 2 for his/her comments and advice. We have found the SPIRIT guidelines useful in revising this protocol.

2.1 It would be helpful if the introduction could begin by setting the scene, namely that the Department of Health has already funded the implementation of MST in 10 sites and has funded an RCT. It would be helpful to clarify when the RCT started – at the start of implementation or after a bedding-in period. [Minor]

Authors’ reply: We have added a paragraph at the start of the Background section to address the issues raised by the reviewer here, as well as the reviewer’s comments in 2.10 below.

2.2 A simple statement that this is the research protocol would be helpful; in places this work reads like a research update. [Major]

Authors’ reply: The paper now states that this is the study protocol in the title, as requested by the journal’s Editors. We have also made this explicit at the start of the Introduction.

2.3 In the introduction, much is made of conduct disorder (CD), but in places the supporting references actually relate to other disorders (e.g., ADHD, ODD) and to aggression and delinquency
without any diagnosis. In paragraph 2, comment is made about ‘CD and its sequelae’ which implies that CD causes the range of problems mentioned. CD is not, however, the issue for the study – it is antisocial behaviour. That is, the main selection criterion is “high risk of requiring out-of-home placement, specifically when this risk is associated with antisocial behaviour” (p. 10). I think, therefore, that the focus could be more on antisocial behaviour and less on CD, and the description of the risk factors for antisocial behaviour could be better described. [Major]

Authors’ reply: The Introduction now focuses on antisocial behaviour in children and adolescents, and explains the link between serious/persistent antisocial behaviour and the diagnosis of CD. In addition, the risk factors identified for persistent and serious antisocial behaviour are more clearly described in the section “MST as an intervention for antisocial behaviour”.

2.4 There is more of a focus on antisocial behaviour in the second section of the Introduction. However, the information in this section is not a comprehensive or a justified selection of risk factors. How did the authors choose these risk factors? Are they all known substantial risk factors, or just examples (as in “For example low IQ...” (p. 4)? Distinction could be drawn between causal risk factors and correlational-only risk factors (i.e., potential moderators) for antisocial behaviour. This would clarify what the intervention would need to address to reduce antisocial behaviour. It would also clarify what the authors will address and why when they say they will ‘evaluate potential moderators’ (p. 25). I think all the moderators to be investigated should be presented in the introduction. [Major]

Authors’ reply: We agree with the reviewer that the original description of risk factors and targets for moderating risk was inadequately focused. Risk factors for antisocial behaviour are now described in the section “MST as an intervention for antisocial behaviour” and we have discussed moderators in the Aims.

2.5 Examples of interventions to address the risk factors for antisocial behaviour are given. The choice of these examples also seems a bit random. Some of these are example are dubious (e.g., social skills approaches and restorative justice do not necessarily affect empathy). A throw-away line that “even psychopathic traits can be improved by treatment” (p. 4) needs some supportive evidence and not just references to a couple of publications. However, the main missing element is whether or not the treatments for these risk factors are part of MST generally or the START programme specifically. [Major]

Authors’ reply: In view of the reviewer’s comments and our amendments described in response to comments 2.3 and 2.4 above, we have removed this passage of text.

2.6 The section entitled ‘The treatment of CD and the importance of MST’ returns us to CD, when it is not the main selection criterion for participants in this RCT. [Major]

Authors’ reply: We have refocused this section (now headed “MST as an intervention for antisocial behaviour” on antisocial behaviour, rather than the specific diagnosis of CD.
2.7 In the review of MST RCTs, more detail could be provided about the outcomes examined. For instance, where the authors say “On follow-up ranging from 12 to 17 months, based on five studies, the SMD was -0.41...” (p. 6), to what outcome measure are they referring? [Minor]

Authors’ reply: We have reviewed this section and made sure that details are provided for the outcomes examined. In the example highlighted by the reviewer, the outcome was missing and we have now added it.

2.8 On p. 9, reference is made to requiring adequate power to examine mediators and moderators of outcome, but only passing reference is made in the introduction as to what might be moderators, and no comment at all is made on what might be the mediators that will be useful to examine. I do think the mediators and moderators should be identified and a rationale for their investigation presented. [Major]

Authors’ reply: We now identify and discuss moderators and mediators within the Aims.

2.9 In the final paragraph on p.9, the authors state that the aim of MST in this RCT is to “reduce the incidence of out-of-home placements for young people at risk of being removed from their home because of antisocial behaviour, severe mental health problems, educational problems, or unmet needs”. The introduction has not mentioned MST for “severe mental health problems, educational problems, or unmet needs”. MST is introduced as being designed to address antisocial behaviour (p. 5). In the Aims section on p. 11, the aim is stated as “reducing out-of-home placement and youth offending in a high-risk sample of adolescents”. Clarity is required. [Major]

Authors’ reply: We have clarified that the aim of the trial was to investigate the effect of MST on out-of-home placements (the primary outcome of the study) in young people showing severe antisocial behaviour. We agree with the reviewer; unfortunately, we conflated the range of co-occurring problems and needs of this population with the specific purpose for which MST has been developed. In line with this, we are studying whether or not MST improves emotional well-being and attendance and behaviour at school as secondary outcomes.

2.10 The introduction ends by referring to the DH-sponsored MST initiative. I think information is needed about when the clinical initiative began and when the RCT began and how the RCT embedded itself in clinical practice. [Major]

Authors’ reply: We have included this information at the start of the Introduction (please refer to our response to point 2.1 above).

2.11 The introduction should end with the aims. [Major]

Authors’ reply: As recommended, we now present the aims at the end of the Introduction.
3.1 The heading should be Method. [Minor]

Authors’ reply: we have referred to the journal’s Instructions for Authors, which indicate that “Methods/Design” is the appropriate heading in a paper describing a trial protocol.

3.2 The design needs to be stated clearly under a subheading ‘Design’. Unit of randomisation? Is it MST plus MAU or MST only? [Minor]

Authors’ reply: As recommended, the paper now includes a “Trial design” section. We now state explicitly that the two conditions are MST alone and MAU, and that the unit of randomisation is the individual.

3.3 Ethical and research governance approvals need to be referenced under and ‘Ethics’ subheading (i.e., committees and reference numbers). [Minor]

Authors’ reply: We apologise for this oversight. Details on ethical approval for the study are now provided.

3.4 A section on Participants should clearly state the inclusion and exclusion criteria. At present they are confusing. For instance, on p. 12 ‘four alternative inclusion criteria’ are announced. Then a 7-item risk scale is presented, which appears to be one criterion, and then a list of the four criteria. This followed by separate lists of criteria for four services (children’s services, YOTs, CAHMS, and educational services). It seems to me that a description of the services and their clientele is required, followed by a crisper list of inclusion/exclusion criteria. [Major]

Authors’ reply: We agree that this section was difficult to follow in our original submission. The full set of inclusion criteria is necessarily complicated by the existence of different sets of criteria for the four different referral sources. We have sought to improve clarity by presenting the common inclusion criteria (which must be met by all participants, regardless of referral source) and indicators of risk status (for which every participant must meet at least three indicators) in the text. The source-specific inclusion criteria are now briefly summarised in Table 1. We believe that this makes the core inclusion criteria easier to follow.

3.5 The section on sample size calculation should be inserted. Does the information on which this is based come from the Brandon Centre study? If not, where does it come from? The authors said that the trial should be powered to examine the mediators and moderators (see p. 9). Is it? [Major]

Authors’ reply: We have provided more information about the basis for the sample size calculations. These were based on data derived from the major effectiveness trials of MST in the USA, which were aggregated in a meta-analysis for the 2013 NICE Clinical Guideline on conduct disorder. We have also clarified (now on p.10) the need to obtain sufficient data for examining moderators and mediators – the trial is not formally powered for this.
3.6 The Recruitment section should give some information as to how the required sample size will be achieved. That is, how many need to be approached to recruit willing and appropriate participants? Taking dropouts into account, how many extra are needed to reach the required sample size? [Major]

Authors’ reply: In the section on sample size we have directed the reader to Figure 1, which shows the participant flow and estimated numbers of participants at each stage of the trial. Estimations of the numbers of participants were based on data as described in 3.5 above.

3.7 More information needs to be in the ‘Randomisation and Blinding’ section. The unit of randomisation is the individual. Are they randomised to specialist MST therapists? Do they get treatment as usual as well as MST? Do others in the service know who is receiving MST? Is this likely to affect the individual’s treatment by non-MST staff in any way (positive or negative)? If MST staff are working in a service, is there contamination of the MAU group (i.e., do non MST staff use MST practices)? Do MST staff also provide MAU for other clients? Who does the qualitative interviews and how does this relate to either blinding or likely bias in responding (e.g., to the therapist)? [Major]

Authors’ reply: Participants in the trial are randomised to receive either MST alone, delivered by specialist MST therapists, or MAU. There has been no systematic study of whether the development of MST teams affects practice in contiguous MAU services. We do not have control over whether, or to what degree, other professional staff in services are aware of treatment allocation. While some of the knowledge associated with MST may be disseminated informally to staff involved in delivering MAU, it is highly unlikely that MAU staff will systematically apply MST principles and practices. Furthermore, it is worth emphasising that one unique and crucial aspect of the MST model of service delivery is the close supervision provided to MST therapists by licensed MST consultants, thereby ensuring that their treatment practices are applied faithfully. This supervisory framework of course would not be available to non-MST therapists.

We have also provided more detail in the “Randomisation and procedures to minimise bias” section to make explicit that the qualitative interviewers are carried out by researchers independent of the MST service providers.

3.8 Much of the description of the intervention in the Method section is too general; most of this information should be (and is) in the Introduction. This should be pared down to what the therapists actually do in START. [Major]

Authors’ reply: We have reviewed the description of the intervention and amended it so that what is contained in the Introduction describes the more general and theoretical aspects of MST, as well as reviewing the literature. The description in the methods focuses on the practical details of how the MST therapist works with the young person, family, and others involved (e.g. teachers, social workers).

3.9 Similarly, much of the description of what MAU might be is general and speculative. Some of the general information could be placed in the introduction. For the purpose of the protocol, what is needed here is simply information about how actual MAU will be logged. [Major]
Authors’ reply: We feel that it is important to retain the description of MAU here, as it describes the likely components of MAU that participants in the trial who are randomised to MAU may receive, and how MAU will be delivered in the context of the trial.

3.10 The Assessments section is too unclear and discursive. It would help to divide assessments into screening, the primary outcome, secondary outcomes, and process measures. This is partially how the subheadings appear, but primary outcomes cannot be plural, and secondary and process outcomes might usefully be separated, and types of measure should all fall under one of these headings. Research findings supporting the measures (apart from psychometric studies) should be in the introduction, not here. [Major]

Authors’ reply: As suggested by the reviewer, we have divided this section into primary outcome and secondary outcomes. Within the secondary outcomes section, we have grouped the measures by type (objective, self-report, interview) and included information on the health economic evaluation.

3.11 Health economic evaluation should be stated in the Aims, and the economic measures included in one of the assessment categories. [Minor]

Authors’ reply: Health economic analysis now appears within the description of secondary outcome measures. The Aims make reference to assessment of cost-effectiveness as a secondary outcome (“Thirdly, the trial will establish whether MST is cost-effective compared to MAU and results in reduced costs across mental and physical health, criminal justice and educational systems”) but we respectfully disagree with the reviewer’s suggestion that the Aims should mention the use of health economic analysis as a means of establishing this, just as we do not make reference in the Aims to the other methods used in relation to the primary and other secondary outcomes.

3.12 Table 1 is not informative – it looks like a researcher’s log. A procedural flow diagram showing what happens when would be an improvement. [Major]

Authors’ reply: We have replaced Table 1 with a new table (now Table 2), which is modelled on the template of recommended content for the schedule of enrolment, interventions, and assessments published in the SPIRIT guidelines. We believe that the new table shows more clearly which measures are administered at which time point.

3.13 Statistical analysis plan – is the trial powered for this logistic regression accounting for all of these covariates? The sample size section does not describe the logic underpinning this. [Major]

Authors’ reply: As detailed in the “Sample size and power calculation” section, the trial is sufficiently powered to detect a 10% difference in our primary outcome measure. If a genuine treatment effect exists and the covariates help explain the variation in the outcome data, then including these covariates will decrease the standard error of the treatment effect parameter estimate, and therefore increase the power.
4. DISCUSSION: Some of the broad aims, succinctly described here, would have been very useful in the Introduction. [Major]

Authors’ reply: We believe that the reorganisation of the paper, and the greater focus on accurately describing the population, specifying risks and moderators, and clarifying the aims of the study, have improved the clarity of the introduction.