

EVALUATION PROTOCOL

A pragmatic cluster randomised controlled trial of the 'Fostering Connections' programme: Examining the impact of trauma-informed training and support for social workers on youth in care in family settings

Centre for Evidence and Implementation and Bryson Purdon Social Research

Principal investigator: Dr Ellie Ott

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Evaluation protocol

Evaluating institution: Centre for Evidence and Implementation and Bryson Purdon Social Research

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YEF trial protocol for efficacy and effectiveness studies

Project title	A pragmatic cluster randomised controlled trial of the ‘Fostering Connections’ programme: Examining the impact of trauma-informed training and support for social workers on youth in care in family settings
Developer (Institution)	National Children’s Bureau
Evaluators (Institutions)	Centre for Evidence and Implementation and Bryson Purdon Social Research
Principal investigator(s)	Dr Ellie Ott
Protocol author(s)	Dr Ellie Ott, Caroline Bryson, Dr Susan Purdon, Anne-Marie Baan, India Thompson
Trial design	Two-armed cluster randomised controlled trial with random allocation at the young person social worker level
Trial type	Efficacy
Evaluation setting	Family and children’s services settings
Target group	Young people in care in foster care, or similar family-based settings, aged 10-18-years-old

Number of participants	558 young people at baseline; 391 young people at follow-up
Primary outcome and data source	<p>Young person outcome:</p> <p>Externalising behaviour measured through the Strength and Difficulties Questionnaire (carer report version, externalising score)</p>
Secondary outcomes and data sources	<p>Young person outcomes:</p> <ol style="list-style-type: none"> 1. Internalising score of the Strength and Difficulties Questionnaire (carer report version, internalising score) 2. Prosocial subscale of the Strength and Difficulties Questionnaire (carer report version) 3. Involvement with criminal justice system measured through conviction or subject to youth caution (SSDA903) 4. Transition into residential care (SSDA903) 5. Placement stability measured through unplanned moves (SSDA903) 6. Missing from care (SSDA903) <p>Foster carer outcomes:</p> <ol style="list-style-type: none"> 7. Compassion satisfaction reported by foster carers, measured through the Professional Quality of Life (ProQOL) scale (self-report) 8. Burnout reported by foster carers, measured through the ProQOL scale (self-report) 9. Secondary traumatic stress reported by foster carers, measured through the ProQOL scale (self-report) 10. Attitudes to trauma-informed practice reported by foster carers, measured through a bespoke questionnaire (self-report) <p>Supervising social worker and young person social worker outcomes:</p>

	11. Attitudes to trauma-informed practice reported by social workers, measured through the Attitudes Related to Trauma-Informed Care (ARTIC) scale
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Protocol version history

Version	Date	Reason for revision
1.0 [<i>original</i>]	03/07/2023	
2.0	15/10/2023	Integration of feedback received from YEF's Grants and Evaluation Committee, and following consultation with selected local authorities on the evaluation design
3.0	16/01/2024	Integration of feedback received from peer reviewer and YEF staff
4.0	27/01/2025	Updated sections on randomisation and sample sizes

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Study rationale and background

Early experiences affect the architecture of the growing brain (Gilmore, Knickmeyer & Gao, 2018; Mustard, 2006; Tierney & Nelson, 2009). While positive experiences help build structures that support emotional regulation and executive functioning, adverse experiences can hinder this development, negatively impacting the ability to respond proportionately to triggers and develop positive social relationships (Furnivall & Grant, 2014; Streeck-Fisher & van der Kolk, 2000). Experiences of childhood trauma often overlap with the circumstances that contribute to a child entering the care system (including parental neglect, physical and emotional abuse, domestic violence, poverty) (Bywaters et al., 2022; Glaser, 2000).¹ As a result, a large proportion of care-experienced young people (YP) have experienced trauma (Salazar et al., 2013). Exposure to traumatic experiences during childhood, without the presence of trusted and supportive adults, places children at higher risk of poorer physical, social and mental health outcomes in later life (Center on the Developing Child, 2023a). Adverse childhood experiences are also linked to violent and non-violent criminal behaviour (Boswell, 1996; Wright, Liddle & Goodfellow, 2016).

Positive social support can help to mitigate the impacts of trauma and enhance resilience to stress (Ozbay et al., 2007; Kimberg & Wheeler, 2019; Centre on the Developing Child, 2023b). Professionals can develop the skills and knowledge to navigate and respond to experiences of trauma in a way that makes a child feel safe, secure and supported (Buckley, Lotty & Meldon, 2016). Strong social support and relationships can help children and YP overcome experiences of adversity (What Works for Children's Social Care, 2022). For YP in care, having the support of a trusted adult is vital to managing daily stress and interpersonal difficulties (Hiller et al., 2021).

Narey and Owers' Review of foster care found that problems with retention/recruitment of foster carers (FCs) are related to the need for more specialist support/training to understand and respond to the complex needs of YP (Narey & Owers, 2018). Close to a third of looked-after YP experience two or more placements in a single year, often because of breakdown in the relationship with their carers.² This can be a result of carers being unprepared to respond to children's specific behavioural difficulties and lack of knowledge/support in relation to the impact of trauma on the lives of YP (Rock et al., 2013; NICE, 2021). Limited understanding of trauma frameworks may lead to a young person's behaviours being misunderstood and labelled as oppositional, destructive, or defiant (Farley, McWey & Ledermann, 2022).

¹ The Children Act 1989 (<https://www.legislation.gov.uk/ukpga/1989/41/contents>) outlines the categories of abuse for entry into care (including neglect). Research has also showed the causal relationship between poverty and child maltreatment and entry into care (e.g., Bywaters et al., 2022).

² Reporting year 2022: Children looked after in England including adoptions. <https://explore-education-statistics.service.gov.uk/find-statistics/children-looked-after-in-england-including-adoptions/2022>

A literature review from the Rees Centre at the University of Oxford (Brown, Sebba & Luke, 2014) into the role of the supervising social worker (SSW) refers to research by Sheldon (2004) into difficulties in the working relationship between children's social workers and SSWs in areas of communication, clarity of roles, and expectations around what FCs can reasonably manage. FC satisfaction was found to increase when the children's social worker – referred to in this trial as young people's social worker (YPSW) -- and SSW worked well together.

Trauma-informed practice (TIP) has become a well-adopted approach among practitioners working with children, particularly in the care system (Asmussen, Masterman, McBride, & Molloy, 2022). TIP draws from neuroscience, psychology, and social work theory and is based in the shared understanding of trauma from professionals in these fields (Levenson, 2007). Training social workers in TIP enables them to understand and respond to behaviours of YP in the context of their traumatic experiences, without over-pathologising or re-traumatising (Wall, Higgins & Hunter, 2016).

Positive impacts of TIP training for staff working with YP have been identified, e.g., on violent incidents (Baetz et al., 2021) and behavioural misconduct and violence in juvenile detention facilities in the USA (Zettler, 2021). Evaluations have also shown a benefit of TIP training for social workers on the use of trauma-informed activities (Wilson & Nochajski, 2016).

While there is wide-ranging evidence on the acceptability of TIP training, there remain significant gaps in evidence of its impact on YP's outcomes, and how it is used across the care sector. For example, there is limited evidence for TIP's impact on the prevalence of YP's offending behaviours, behavioural regulation, and pro-social relationships and on its implementation in the context of social work and foster care (Gaffney, Jolliffe & White, 2021).

A recent report by the Early Intervention Foundation found that while 89% of local authorities (LAs) in England reported implementing trauma-informed activities, only 22% had a shared definition of what trauma-informed means in practice (Asmussen, Masterman, McBride, & Molloy, 2022). Better definitions of TIP are required to identify how it differs from 'practice as usual', and training as a standalone tool is unlikely to be enough to influence meaningful sustainable change.

The evaluation of Fostering Connections provides a critical opportunity to generate insights into these issues and the importance of joined-up social work practice, while also championing the needs of care-experienced YP. A rigorous but carefully designed evaluation can improve our understanding of effective TIP training and support, how it can be embedded into social work practice, and which outcomes it can influence for YP in care.

The trial is being run as a cluster RCT across eight LAs, with YPSW as the randomisation unit and individual YP as the unit of analysis. There will technically be four arms in the trial: YP in

families where both the SSW and YPSW have been randomised to the intervention (the intervention arm); YP in families where both the SSW and YPSW have been randomised to control (the control arm); YP where the SSW has been randomised to the intervention and the YPSW has been randomised to control; and YP where the SSW has been randomised to control and the YPSW has been randomised to the intervention. **The primary analysis will be restricted to the first two of these arms (both SSW and YPSW allocated to intervention v. both allocated to control), because the intervention to be tested is the training of the YPSW/SSWs in combination. For this reason the trial is described as two-armed.** The other two arms will be included in an exploratory analysis to gain some understanding of whether training one of the two professionals is of value if, and when, both cannot be trained, and to give an estimate of the added value of training and supporting both.

There are constraints on the number of training places per LA³, so YPSWs per LA have not typically been allocated to intervention and control group in the ratio 50:50. Instead, the percentage allocated to the intervention was set so that all available places were filled, with up to a maximum of 70% being allocated to the intervention. Overall, the percentage of YPSWs allocated to the intervention is 46%. Randomisation has been run separately per LA by the trial statistician, giving implicit stratification by LA. Since the trial statistician is undertaking the randomisation and will conduct the statistical analysis, the analysis will not be blind to allocation.

Prior to randomisation, each SSW was paired to the YPSW they work alongside most frequently. Each SSW was then assigned to the same group as their paired YPSW. This pairing leads to a trial where the four arms are not balanced (see randomisation section). The two primary arms ('both SSW and YPSW allocated to intervention', and 'both allocated to control') are balanced, and the two secondary arms are balanced. But there will be systematic differences between the two primary and the two secondary arms. For the reporting on the exploratory analysis across all four arms the trial these differences will be acknowledged.

A total sample of 1,477 YPs entered the trial, together with 422 YPSWs and 264 SSWs. Of the 1,477, 979 were assigned to one of the two primary arms: 458 to the group where both the YPSW and the SSW are assigned to the intervention group and 521 to the group where both the YPSW and the SSW are assigned to the control group. After non-response at baseline and follow-up we anticipate around 391 YP will be in the final analysis dataset.

³ Depending on the number of in-scope social workers in an LA, either one or two training groups were allocated to the LA, with the maximum number of places per group being 35. In some instances the allocation ratio may consider team capacity and available back cover.

Outcomes data are to be collected at baseline and follow-up, for YP, FC, SSWs and YPSWs. An implementation process evaluation (IPE) will assess the appropriateness, feasibility, acceptability and fidelity of the intervention, and explore mechanisms of change.

Intervention⁴

The National Children’s Bureau (NCB) and Leap Confronting Conflict (Leap CC) have partnered to deliver the ‘Fostering Connections’ intervention aimed at enabling YP aged 10-18 years old in family-based foster care or supported lodgings to have reduced emotional and behavioural difficulties, including through the strengthening of meaningful relationships with trusted adults. To do so, the intervention seeks to improve professional relationships and communication between YPSWs and SSWs, improve support for FCs (including kinship/connected carers and host families of YP aged 16 and 17 in supported lodgings), and increase understanding of trauma and implementation of trauma informed practice from the adults supporting the YP in care (FCs, SSWs and YPSWs). For brevity, the term foster carers (FC) is used in the protocol to include both foster carers and supported lodgings with family environments and a similar support structure. This includes formal kinship care (also known as connected care), which is a type of foster care which involves placing a child into the care of a relative or someone with a significant connection with the child or young person. Supported Lodging involves placing a young person in care or ‘care leaver’ (usually a between the age of 16-21) in the home of an approved host family, for a temporary period. Young people under 18 continue to have a Young Person Social Worker, and supported lodgings are included in this trial if the host family has a support worker who is a Supervising Social Worker or akin to a supervising social worker.

The trial is being run in eight LAs/Children’s Trusts across England. The trial has particularly targeted LAs where the Multi-Agency Safeguarding Partnerships (MASP) have strategic priorities on preventing/reducing youth violence and/or safeguarding for adolescents.

YPSWs and SSWs are provided with an e-learning module (around 45 min), 7 days of in-person training and 3 online reflective practice⁵ (RP) sessions over five months, followed by 4 cross-

⁴ This section has been updated to reflect changes made to the intervention as communicated by the delivery team.

⁵ Reflective practice is intended to allow participants to enhance their skills, self-awareness, and deepen their practice. The RP process seeks to enhance participant’s learning and build their ability to take responsibility and be more empowered. Leap CC’s reflective practice sessions are experiential in nature and meeting a whole range of learning styles. They are informed by several models of reflections including Kolb’s Model of Experiential Learning (1984), Gibb’s Reflective Cycle (1998) and Rolfe, Freshwater & Jasper ‘What’ Model (2001).

LA virtual follow-up workshops after the end of the training and an online peer support network. Trained staff work with the young person and/or their FC across the 10 to 12-month period from start of the training to follow-up (including after the intervention ends).

A small group of TIP champions will be identified in each LA (up to 5 SSW and YPSW), who will act as a point of contact for advice and support for SSW and YPSW who participated in the intervention also beyond the life of the project. They are selected from among the SSWs and YPSWs in the intervention group, following delivery of the core intervention during the initial 5 months.

LA leaders (heads of service, team leaders) are engaged to support implementation of TIP and RP. The abovementioned e-learning module will be made available to the LA leaders in the intervention group on request to support understanding of the programme. An e-learning module will also be offered to FCs in the intervention group, and to FCs in the control group after follow-up data collection. The intervention will be delivered in three phases:

1a. Set up (0 - 5 months):

- Host webinar to introduce the programme to LAs and engagement meetings with senior leaders at prospective LAs to support recruitment
- Engage 10 cohorts across 8 LAs (and identify/engage participants for RCT)
- Identify participants based on the criteria:
 - SW of looked after YP aged 10-17 in family-based care
 - SSW of their FCs, including connected carers and/or supported lodging host families
- Weekly preparatory meetings with each LA to secure dates for the training, planning engagement work with social workers and managers to promote the training and arranging logistics.

1b. Development of resources and materials (0 - 5 months, concurrent with the set up):

- Co-develop recruitment/engagement/training materials with existing partner, i.e. Chickenshed theatre⁶, to embed the voice of care experienced YP and FCs through:
 - Video/audio
 - Case studies
 - Information about the Fostering Connections programme for YP and foster carers

⁶ Chickenshed is a London-based theatre companies who will support the co-development of training materials with care-experienced young people, with the aim of supporting learners to align their newly acquired theoretical knowledge with practical examples which reflect young people's lived-experiences.

- Develop/host e-learning for foster carers and social workers
 - Trainers tailor the training to the local context due to the style of training and depending on the issues which participants bring
2. Core training intervention (6 - 13 months, staggered):
- Participants will complete e-learning, 7 face-to-face training days, and 3 online RP sessions (2 hours each) delivered in combination for SSWs and YPSWs over 5 months. The core training intervention is delivered to each LA separately. Delivery modules and activities are tailored to the needs of each group, however the broad structure will cover topics like:
 - Induction
 - Values, identity, and boundaries
 - Working with challenging behaviour using TI principles and practices
 - Challenging behaviour and de-escalation
 - Option of pastoral calls with trainers for participants wanting to add more depth to their understanding of particular tools
3. Follow up to core training (11-16 months)
- Identify/establish small group of TIP champions in each LA
 - In-person workshop with LA senior leaders, project leads and TIP champions in the programme in each LA to co-develop policies and explore how to ensure the sustainability and ongoing implementation of TIP/RP for the intervention group.
 - Ongoing national peer support via online community of practice forum for all SSWs and YPSWs in the intervention group
 - Half-day online cross-LA learning and networking event for all SSWs and YPSWs in the intervention group and LA leaders
 - Cross-LA virtual follow up workshops for 4 months beyond core delivery in each wave for SSWs and YPSWs in the intervention group, guided by a Fostering Connections trainer to help consolidate the training and embed it into professional practice.

Set up (phase 1a) and the development of resources and materials (phase 1b) will be between August and December 2023. Intervention delivery (phases 2 and 3) will be during February 2024 – December 2024. Delivery will be staggered with intervention delivery in LAs starting between February and September 2024. NCB has introduced this staggered approach to reflect feedback from LAs that flexible start dates will allow them to accommodate the set-up requirements of the training programme and evaluation, alongside their other pre-existing commitments and priorities in relation to – among others – workforce development. Follow-

up time will be slightly shorter for child-outcomes for later waves given the use of administrative data, but this is not anticipated to have a significant effect on impact.

Box 1. Preliminary TIDieR framework

Brief name	Fostering Connections
Why	<p>The intervention seeks to improve the TIP support for YP in foster care (or similar family-based settings) as well as their FCs, through training and RP sessions for YPSW and SSWs. A key presumed causal mechanism is that TIP can help improve YP’s outcomes, by responding to the outcome of trauma (e.g., YPs negative behaviours), in a way that acknowledges trauma and its impact. Training YPSWs and SSWs together is expected to improve the support they provide to FCs and YPs including in relation to strengthening the attachment relationship between FCs and YP. LA leaders are engaged to ensure that social workers are supported to implement TIP/RP approaches in their practice.</p>
What	<p>Materials: FCs will have access to a 30 minute <i>‘Introduction to Trauma Informed Principles’</i> online module which provides a brief introduction into what trauma is and how to support young people. SWs will have access to a 30 minute <i>‘Fostering Connections Programme’</i> online module which gives a brief overview of Leap CC’s expertise and training style.</p> <p>In the training days, participants will be provided with workshop materials (including printed slide hand-outs and electronic copies circulated via email).</p> <p>Procedure:</p> <p>Core training (month 1-5)</p> <ul style="list-style-type: none"> • Month 1: Participants will complete the ‘Fostering Connections Programme’ online module • Month 2-6: Participants then take part in 7 face-to-face training days, and 3 online RP sessions (2 hours each) over 5 months. The training and RP is delivered to both SWs and YPSWs. <ul style="list-style-type: none"> • Induction • Values, identity, and boundaries • RP day • TIP days 1 & 2

	<ul style="list-style-type: none"> • RP day 2 • Working with challenging behaviour using TI principles and practices day 1 & 2 • Challenging behaviour and de-escalation • RP day 3 • Option of pastoral calls with trainers for participants wanting to add more depth to their understanding of particular tools <p>Follow up training and support (months 6 - 10):</p> <ul style="list-style-type: none"> • Month 6: Identify/establish small group of TIP champions • Month 6: In-person workshop with LA leaders and TIP champions in the programme to co-develop policies and actions that support ongoing implementation of TIP/RP • Ongoing national peer support via online community of practice forum and cross-LA learning and networking event for SSWs and YPSWs • Months 6-10: Monthly cross-LA virtual follow up workshops for 4 months beyond core delivery in each wave for SSWs and YPSWs
Who provided	<p>The core training is delivered by trainers of Leap Confronting Conflict who have over 30 years' experience in designing and delivering highly impactful training programmes to YP and the adults in their lives about successful conflict navigation.</p> <p>The follow up wraparound support (inc. the online community of practice forum, learning and networking and supporting event and cross-LA virtual follow up workshops) is led by the National Children's Bureau (NCB), the intervention developers, who have over 6 decades worth of experience of improving systems to keep children safe, supported and secure.</p>
How	<p>Participants will first complete an online module. The 7 training sessions are delivered in person in small groups (up to 35 participants) and the 3 RP sessions are online with smaller group sizes up to 12 participants. The Policy workshops with LA leaders will be held in person in the local area with the learning and networking event held online to ensure that social workers from across the country can easily attend. The peer-support forum and cross-LA follow-up workshops will be online. Delivery of support to YP and FCs by the YPSW and SSW could take a range of forms dependent on the YP's care plan.</p>
Where	<p>The training and RP sessions is expected take place in LA training facilities or other facilities that the LA typically hires.</p>

When and How Much	SSWs and YPSWs are provided with e-learning, 7 days of training and 3 online sessions of RP (2 hours each) over five months, followed by monthly follow-up workshops, a learning and networking event and an online peer support forum. Trained staff would be working with the young person and their FC throughout this period (months 1-10), including visits to the young person and/or foster family at a minimum of once every 6 weeks (but likely to be more frequent) as well as further meetings (reviews of care plans etc).
How well	Fidelity of the intervention will be assessed by the evaluation team as part of the IPE. Fidelity will be assessed based on coverage of the intended session content by trainers. Compliance will be assessed based on the attendance by YPSWs and SSWs of training and support sessions.

Intervention theory of change

The Fostering Connections approach is based on a modular curriculum delivered in two existing TIP programmes, Rise Up and Under Our Roof. Both programmes have been evaluated and reports are publicly available (King & Hahne, 2021; Lewis & Davis, 2021).

SSWs and YPSWs are key members of the team around the YP who can support FCs to care for YP who have experienced trauma. The intervention is expected to improve awareness of the impact of trauma on behaviour and, by increasing skills, knowledge and confidence in TIP and RP for SSWs and YPSWs, FCs will be supported to form and maintain positive, stable relationships with YP in their care. A key presumed causal mechanism is that by responding to the outcome of trauma such as negative behaviours, in a way that acknowledges trauma and its impact TIP can help to reduce this negative behaviour and prevent later crime and violence (Maynard et al., 2019). The specific casual pathway for YPSW and SSW, FCs, and YP is outlined below.

YPSW and SSW

The **causal pathway for YPSW and SSW** involves improved knowledge, understanding and awareness of trauma and its long-lasting impacts on people’s lives leading to change in practice. This practice shift relates to consistency in language and approaches used across teams enabling more effective collaboration, including information sharing regarding trauma histories, reflecting this in care plans and using it to improve the accuracy/relevance of referrals as well as matching, preparing and supporting FCs to support YP effectively. YPSWs and SSWs working together can support the attachment relationship between FCs and YP, through clarity of the two social worker roles, good communication, and empathy and challenge of the FC (Brown, Sebba & Luke, 2014). YPSW and SSW are expected to sustain

changed practices, and increasingly work in partnership (e.g., through a cross-team structure). Knowledge of trauma-informed principles is viewed as having the potential to improve job satisfaction of social workers in the short term (Asmussen et al., 2022), and can also help social workers to identify and understand secondary traumatic stress in staff (Lowenthal, 2020).

Fostering Connections includes work with LA leaders to ensure that social workers are supported to implement TIP/RP approaches in their practice, given that training as a standalone tool is unlikely to be enough to influence meaningful sustainable behaviour change.

Foster carers

The **causal pathway for FCs** involves a greater understanding of the impact of trauma on the behaviour of YP in their care, as a result of YPSW and SSW effectively sharing relevant information and supporting them through conversations and supervision. This enables FCs to better respond to the behaviour of YP in a trauma-informed way. Support from SSW through a TIP/RP approach and increased understanding of trauma enables FCs to strengthen their relationship with YP. Effective emotional support from their SSW is also expected to support improved professional quality of life of FCs.

Young people

The **causal pathway for YP** consists of strengthened relationships with adults, which is expected to contribute to fewer emotional and behavioural difficulties and improved mental health (What Works for Children's Social Care, 2022; Luthar, 2015). FC, SSW and YPSW reflecting on YP's behaviour differently and responding in a trauma-informed way reduces the risk of re-traumatization of YP. YP may also have better access to services facilitated by more effective information sharing between YPSW and SSW on the trauma history of the YP. YP in foster care having meaningful relationships with trusted adults, improved mental health, and reduced emotional and behavioural difficulties is hypothesised to reduce placement breakdown, isolation and the likelihood of involvement in youth violence.

The services received by YPSWs, SSWs, FCs and YP in the control group consist of 'practice as usual' i.e., the typical practice and provision by the YPSW and SSW. Practice as usual is defined in terms of the routine training and support on TIP/RP approaches that SWs and FCs receive (including any existing policy and protocols to support use of TIP/RP). Interested LAs were asked about their practice as usual. Based on the information collected to date, practice as usual varies significantly across LAs. Existing training offers are generally less intense than Fostering Connections, or tend to be focussed on particular cases rather than broader practitioner practice. Moreover, the Fostering Connections programme distinguishes itself not only in its intensity, but also in its focus on embedding TIP in practice, in training SSW and

YPSW together to facilitate joined-up TIP support to FCs and YPs, and in its consideration of the wider organisational context and necessary senior leadership support.

Impact evaluation

Research questions or study objectives

The primary question to be addressed by the trial will be:

Research Question 1 (RQ1): What is the impact of providing the training and support to both YPSWs and SSWs on the externalising behaviour of YP in care in family settings? This will be measured using the Strengths and Difficulties Questionnaire (SDQ), completed by FCs/supported lodgings providers at baseline and follow-up.

Secondary research questions focus on wider impacts on YP, as well as on impacts on SSWs, YPSWs and FCs. These ask questions about the impact of providing training and support to YPSWs and SSWs on:

Young people

RQ2: What is the impact of providing the training and support to both YPSWs and SSWs on the stability of foster care/supported lodging placements for YP, measured using SSDA903 data on reasons for moves (those categorised as ‘unplanned’) and transitions into residential care?

RQ3: What is the impact of providing the training and support to both YPSWs and SSWs on YP’s involvement with the criminal justice system, measured using youth cautions and convictions data in the SSDA903?

RQ4: What is the impact of providing the training and support to both YPSWs and SSWs on YP’s episodes missing from care⁷ as reported in the SSDA903?

RQ5: What is the impact of providing the training and support to both YPSWs and SSWs on the internalising and prosocial subscales of the SDQ, completed by FCs/supported lodgings providers at baseline and follow-up.

YPSWs and SSWs

RQ6: What is the impact of providing the training and support to both YPSWs and SSWs on their attitudes towards TIP, measured using the ARTIC scale?

⁷ Missing from care: a looked-after child who is not at their placement or the place they are expected to be (for example school) and their whereabouts is not known.

Foster carers

RQ7: What is the impact of providing the training and support to both YPSWs and SSWs on the compassion satisfaction, burnout, and secondary traumatic stress of FCs/caregivers in a family setting, measured using the ProQOL?

RQ8: What is the impact of providing the training and support to both YPSWs and SSWs on the FCs' attitudes towards TIP, measured using selected questions from the ARTIC scale.

All measures are described in detail in the 'Outcomes' section below.

The design of the trial is such that, in addition to measuring the impact of training and support provided to both SSWs and YPSWs, estimates will be produced of the impact of providing training to just the SSW or just the YPSW. As such, two secondary research questions to address within the trial are:

RQ9: What is the impact of providing the training and support to SSWs, but not to YPSWs, on the outcomes of YP, SSWs and FCs?

RQ10: What is the impact of providing the training and support to YPSWs, but not to SSWs, on the outcomes of YP, YPSWs and FC?

Design

Table 1: Trial design

Trial design, including number of arms		Two-armed cluster randomised controlled trial
Unit of randomisation		Young person social worker
Stratification variables (if applicable)		Local Authority
Primary outcome	variable	Young people externalising behaviour
	measure (instrument, scale, source)	Externalising score, Strengths and Difficulties Questionnaire (Goodman, Meltzer & Bailey, 1998), carer-reported version, fielded in online survey 10-12 months after baseline
	variable(s)	Young person: SDQ Internalising and Prosocial sub-scales; Involvement with criminal justice system; transition into

Secondary outcome(s)		residential care, placement stability; missing from care episodes SSW / YPSW: Attitudes to trauma-informed practice FCs: compassion satisfaction; burnout; secondary traumatic stress; attitudes to trauma-informed practice
	measure(s) (instrument, scale, source)	Young person: Internalising behaviour score (SDQ, carer-report version); Prosocial score (SDQ, carer-report version); Child conviction or subject to youth caution (SSDA903 2024-25); transition into residential care (SSDA903 2024-25); unplanned moves (SSDA903 2024-25); missing from care episodes (SSDA9035 2024-25) [See box 2] SSW / YPSW: ARTIC scale at 10-12 month after baseline FCs: Professional Quality of Life Scale (self-report), bespoke questionnaire (self-report) at 10-12 months after baseline
Baseline for primary outcome	variable	Young people externalising behaviour
	measure (instrument, scale, source)	Externalising score, Strengths and Difficulties Questionnaire, carer-report (Goodman et al, 1998), fielded in online survey as close as possible to randomisation of YPSW
Baseline for secondary outcome	variable	Young person: SDQ Internalising and Prosocial sub-scales; Involvement with criminal justice system; Transition into residential care, placement stability; missing from care episodes SSW / YPSW: Attitudes to trauma-informed practice FCs: compassion satisfaction; burnout; secondary traumatic stress; attitudes to trauma-informed practice
	measure (instrument, scale, source)	Young person: Internalising behaviour score (SDQ, carer-report version); Prosocial score (SDQ, carer-report version); Child conviction or subject to youth caution (SSDA903 2023-2024); transition into residential care (SSDA903 2023-2024); unplanned moves (SSDA903 2023-2024); missing from care episodes (SSDA903 2023-2024) SSW / YPSW: ARTIC Scale prior to randomisation FCs: Professional Quality of Life Scale (self-report); bespoke questionnaire (self-report) as close as possible to randomisation of YPSW

Randomisation

The trial is clustered, with the unit of randomisation being YPSWs. Each SSWs is assigned to the same group as the YPSW with which they work most frequently. The unit of analysis for the primary outcome is the YP. There are constraints on the number of training places per LA⁸, so YPSWs per LA have not typically been allocated to intervention and control group in the ratio 50:50. Instead, the percentage allocated to the intervention has been set so that all available places are filled, with up to a maximum of 70% being allocated to the intervention. In practice, for most LAs, the percentage allocated to the intervention is around 46%. Randomisation has been run separately per LA by the trial statistician, giving implicit stratification by LA. Since the trial statistician is undertaking the randomisation and will conduct the statistical analysis, the analysis will not be blind to allocation.

Prior to randomisation each SSW will be assigned to a unique YPSW and their allocation to group will follow that of the YPSW (see below).

A significant complication here is that the primary analysis aims to test whether delivering the intervention to both SSWs and YPSWs improves outcomes for YP, rather than simply testing whether delivering the intervention to one set of professionals has an impact. Yet, SSWs do not cluster within YPSWs (or vice versa), so straightforward randomisation of YPSW/SSW pairs is not feasible. Inevitably some YP in each LA will have a SSW who has been assigned to the intervention group and a YPSW who has been assigned to the control group, and vice versa. That is, when the randomisation is done, there will be YP in each of four arms with the first two (in bold) being the primary analysis arms:

:

Arm 1: $T_{SSW}T_{YPSW}$ (i.e. both SSW and YPSW assigned to the intervention);

Arm 2: $C_{SSW}C_{YPSW}$ (i.e. both SSW and YPSW assigned to the control group);

Arm 3: $T_{SSW}C_{YPSW}$ (i.e. SSW assigned to the intervention and YPSW assigned to the control group);

Arm 4: $C_{SSW}T_{YPSW}$ (i.e. SSW assigned to the control group and YPSW assigned to the intervention group).

As noted, our primary analysis will focus on YP within Arms 1 and 2 (that is, pure intervention and pure control). YP in Arms 3 and 4 will be excluded from the primary analysis. Arms 3 and

⁸ Depending on the number of in-scope social workers in an LA, either one or two training groups were allocated to the LA, with the maximum number of places per group being 35.

4 will however be included in an exploratory analysis, where the impact of just one of the two YPSWs/SSWs being assigned to the intervention is estimated.

In order to maximise the sample size of YP in Analysis Arms 1 and 2 each SSW was assigned to a unique YPSW prior to the start of the trial. This was done by assigning each SSW to the YPSW with whom they share the most eligible YP. To illustrate, if a SSW has 10 eligible YP, and for five of the 10 they work alongside YPSW-1, for three they work with YPSW-2, and for two they work with YPSW-3, then this SSW is assigned to YPSW-1.⁹ If YPSW-1 is then randomly allocated to the intervention group, this SSW will also be assigned to the intervention group (and vice versa). Note that two or more SSWs might be assigned to a single YPSW under this model. The aim in doing this assigning is to generate a set of YPSW/SSW ‘clusters’ that between them cover as many eligible YP as possible. The randomisation steps are described in detail in the Appendix.

The steps in the randomisation are summarised below:

Step 1	Assign each SSW to a unique YPSW (the one they work with for most YP)
Step 2	Randomly allocate YPSW to either intervention and control
Step 3	Assign each SSW to ‘intervention’ or ‘control’ with the allocation being the same as the allocation to group of their ‘unique YPSW’
Step 4	<p>Having determined the group status for every YPSW and SSW, establish which of four arms each YP now belongs to:</p> <p>Arm 1 : Both of the YP’s SSW and YPSW assigned to intervention;</p> <p>Arm 2 : Both of the YP’s SSW and YPSW assigned to control;</p> <p>Arm 3: The YP’s SSW assigned to intervention but their YPSW assigned to control</p> <p>Arm 4: The YP’s SSW assigned to control but their YPSW assigned to intervention.</p> <p>Only those in Arms 1 and 2 are used in the primary analysis.</p>

⁹ With assignment to an SSW being done randomly if there are two or more SSWs with which they share the same number of families.

This approach to randomisation does not give a four-arm RCT with balance across all four arms. Arms 1 and 2 will be balanced, and Arms 3 and 4 will be balanced, but the YP in Arms 3 and 4 will have different experiences to those in Arms 1 and 2 in the sense that the Arms 3 and 4 YP will be more likely to have a YPSW and SSW who work together infrequently. This does not affect the primary analysis, which compares just Arms 1 and 2, but in the reporting of the exploratory analysis that compares all four arms we will describe the potential for bias. The final report will include all the assumptions made for that analysis to be unbiased.

The randomisation was done at a single point in time per LA. The randomisation was carried out by the trial statistician within Excel using an anonymised list of eligible YP, YPSWs and SSWs. Each LA generated a list of their eligible YP (with a unique ID) and with an ID of the YPSW and SSW against each YP. Per LA, the SSWs were assigned to a unique YPSW following the rules set out above. A separate list of YPSWs was then created with a count of the number of eligible YP per YPSW. The YPSWs were sorted by this count variable and a systematic random half per stratum assigned to the intervention. This gives implicit stratification by the count variable per LA.

Once randomisation for an LA was complete, two files were generated, one showing the assignment to intervention or control for all YPSWs and SSWs, and one showing the group assignment for all eligible YP.

Participants

The trial is being delivered in eight LAs.

Within the participating LAs, the intervention is being delivered to:

1. YPSWs whose caseload includes at least one young person aged 10 to 17 at the start of the trial;
2. SSWs working with at least one FC or supported lodgings provider¹⁰ with a young person aged 10 to 17 in their care at the start of the trial.

All eligible YPSWs and SSWs within the participating LAs enter the trial with no process of opt out or opt in.

Any YPSWs or SSWs who enter the service after the start of the trial, or take on an eligible FC or young person after the trial begins, is out of scope, as the intervention has a single start point within each LA.

Although the intervention is being delivered to YPSWs and SSWs, the trial primarily focuses on the measurement of the impact of the intervention on eligible YP and FCs. For a YP or FC

¹⁰ For brevity, the term 'foster carer' is used in the protocol, but includes both foster carers and supported lodgings providers where there is a family-like environment and a similar support structure.

to be in scope for the trial, the young person needs to meet the basic age criteria, but also needs to have both a YPSW and SSW that is in-scope for the trial. If some YPSWs or SSWs are excluded from the trial (which might, for example, happen if Independent Fostering Agencies are excluded in some LAs) then the YP they are assigned to will not be included in the trial.

Our intention is to only include FCs and YP who are involved with the YPSWs and SSWs at the start of the trial.

Each YP has been assigned to one of the four randomisation arms. Some contamination during the trial is inevitable because some YP will have a change in their YPSW or SSW during the trial (with some 'control' YP being assigned to a trained SW during the trial and vice versa). This will be reviewed prior to follow-up data collection and sensitivity analysis will be conducted to establish the degree to which this dilutes the overall effect sizes (see Statistical Analysis Plan).

Baseline and follow-up data collection from SSWs, YPSWs and FCs is collected via an online survey. Consent is sought from YPSWs, SSWs and FCs prior to data collection. YPSWs, SSWs and FCs are given the choice to 'opt out' of being approached about any evaluation data collection activities. SWs and FCs are asked to provide consent or decline consent to participate in the baseline and follow-up survey through an integrated online consent form.

YPSWs, SSWs and FCs (excluding those who opted out) are sent a unique survey link by the evaluation team, and as such will not need to provide any identifying information via the online survey platform. The evaluation team will follow up via email - and phone numbers if provided – for reminders about the survey.

Sample size calculations

Our primary analysis (which uses externalising behaviour as measured through the SDQ as an outcome) will focus on the young people where both the YPSW and SSW have either been assigned to the intervention group or to the control group (Analysis Arms 1 and 2 as described in the randomisation section above). Arms 1 and 2 between them cover 66% of all eligible YP in the trial. Table 2 focuses on these two groups.

Under our original plans for the trial an MDES of 0.21 standard deviations would have been achieved. This was on the basis that we would recruit 10 LAs to the trial rather than the eight achieved. In addition, our initial predictions of response rate at baseline were higher than has proved achievable. However, the number of social workers has proved to be much larger than anticipated, which reduces the average cluster size. Nevertheless, the effect of all these changes is that the MDES has increased from the expected 0.21 standard deviations, to 0.24 standard deviations.

Our current assumptions are:

- Across the eight LAs, the total number of eligible YP is 1,477. Of these, 979 have been assigned to one of the two primary analysis arms: 458 to the group where both the YPSW and the SSW are assigned to the intervention group and 521 to the group where both the YPSW and the SSW are assigned to the control group.
- Of the YP in the primary analysis, baseline data has been collected for 57% of them, giving a trial ‘baseline population’ of 558. We do not expect there to be any major imbalances at baseline associated with FC non-response, because the decision taken by a FC on whether or not to take part should be independent of the randomisation. In most cases the baseline data was collected prior to randomisation, but where it was collected post-randomisation, the FC would have been very unlikely to be aware of the allocation of their SSW or their foster child’s YPSW.
- Of the 558 YP with baseline data, we assume 70% will be eligible (that is, the YP is still in foster care) and will complete at follow-up. This will give an analysis dataset of around 391: 183 YP in the intervention arm and 208 YP in the control arm.
- We assume that the correlation between the externalising SDQ score between baseline and follow-up will be around 0.6. The Creative Life Story Work (CLSW) trial, which compared baseline and follow-up SDQ scores on the SSSA903 for a similar population found a correlation of 0.53 (Taylor et al., 2022). With more standardisation on data collection in the Fostering Connections Trial we expect the correlation to be slightly higher at 0.6, but if the 0.53 is replicated our MDES increases from 0.24sd to 0.25sd.
- The Intraclass Correlation Coefficient (ICC) associated with the clustering of the trial within YPSWs is not known, and we do have data from which we can estimate it, but we have assumed it may be as high as 0.2. That is, we assume that between-YPSW variance in the SDQ externalising score accounts for quite a high percentage of total variance. This would be the case if social workers have a marked influence on SDQ scores. Given the hypothesis that the Fostering Connections training will affect SDQ scores, this seems the most reasonable assumption we can make. The average cluster size is expected to be around 3.7¹¹ for all those in the trial, but in the analysis dataset is expected to be considerably lower because of non-response. It could potentially be very close to 1, but is set at 1.5 in the calculations. Consequently the MDES is not very sensitive to the ICC assumption.

Table 2 sets out the assumptions for the primary outcome (externalising SDQ score). The assumptions do not all hold for the secondary outcomes, the major differences being:

¹¹ Calculated as the ratio of YP to SSWs, because there are fewer SSWs than YPSWs in the trial

- For SSDA903 outcomes, which are collected via administrative systems, the sample sizes will be somewhat larger because losses to the sample will be lower. The correlation between baseline and follow-up is not known but is likely to be low for the non-SDQ scores at least, and the ICC is not known, but overall we expect an MDES of around 0.20sd for these outcomes;
- There will be fewer FCs than YP in the analysis as FCs may care for more than one eligible YP, our best current assumption being that it will be around 139 in Group 1 and 163 in Group 2. We estimate an MDES of 0.27sd for their outcomes.
- There are 422 YPSWs and 264 SSWs in the trial (686 overall), divided into two groups, intervention and control, with around 316 in the intervention arm and 370 in the control arm per arm (139 and 163 respectively after non-response). For their outcomes we estimate an MDES of around 0.23sd.

Table 2: Sample size calculations

		PARAMETER
Minimum Detectable Effect Size (MDES)		0.24sd
Pre-test/ post-test correlations	level 1 (participant)	0.6
	level 2 (cluster)	0
Intracluster correlations (ICCs)	level 1 (participant)	0
	level 2 (cluster)	0.2
Alpha ¹²		0.05
Power		0.8
One-sided or two-sided?		Two
Average cluster size (if clustered)		1.5

¹² Please adjust as necessary for trials with multiple primary outcomes, 3-arm trials, etc., when a Bonferroni correction is used to account for family-wise errors.

		PARAMETER
Number of clusters ¹³	Intervention	198 YPSWs/124 SSWs at randomisation stage
	Control	224 YPSWs/140 SSWs at randomisation stage
	Total	422 YPSWs/264 SSWs
Number of participants	Intervention	183 (after non-response)
	Control	208 (after non-response)
	Total	391

Outcome measures

Outcomes data will be collected in relation to four groups (YP, FCs, YPSWs, and SSWs).

Baseline measures

Baselines measures consist of measurement of the primary and secondary outcomes as outlined below.

Outcomes data for the primary outcome and the secondary outcomes for SSWs, YPSW and FCs is collected at two time points: prior or close to randomisation of the YPSWs (baseline)¹⁴ and at a follow-up point (10 to 12 months after baseline).

Baseline data for the YP secondary outcomes (measured through SSDA903 data) covering the period April 2023 – March 2024 is collected throughout the year until 31 March 2024 and reported in July 2024.

Follow up data for YP secondary outcomes covering the period April 2024 – March 2025 will be collected in July 2025, and will hence cover approximately three months following the end of intervention delivery in LAs where training started in April 2024.

¹³ Please state how the data is clustered, if there is any clustering (e.g. by delivery practitioner or setting).

¹⁴ Baseline data from YPSWs and SSWs will be collected prior to randomisation. Baseline data from foster carers will be collected as close to randomisation of social workers as possible.

Data on training and support on TIP/RP approaches received by the control and intervention group will be collected from SSWs and YPSWs at baseline and follow-up to establish practice as usual, and inform the fidelity assessment that is part of the IPE.

Primary outcome

The primary outcome measure for the efficacy trial, is YP externalising behaviour measured through the externalising score - a subscore of the **Strengths and Difficulties Questionnaire (SDQ)** (Goodman, 2001). The externalising score will measure outcomes in relation to reduction in YP behavioural difficulties, as outlined in the theory of change. SDQ has been found to correlate with the level of offending in young offenders (van Domburgh et al., 2011).

The SDQ is a validated scale with an established evidence base which measures behaviours, emotions, and relationships across 25 items. The efficacy trial will adopt the parent/carer version (online), suitable for reporting on 4-17 year-olds. Carer and adolescents have been found to have fair agreement between scores, with adolescents self-reporting fewer behaviour challenges than carers (but no evidence of threshold effects for either) (Mohangi, Magagula & van der Westhuizen, 2020).

FCs/supported lodgings providers will be contacted by the evaluation team to complete the SDQ for each YP in their care that meets the eligibility criteria for the trial at baseline. SSWs will be expected to follow up with foster carers encouraging them to complete the survey.

Although the SDQ is collected as part of the administrative data SSSA903, we propose for SDQ data to be collected separately for the evaluation, to avoid challenges around standardising the point in time of collection and the risk of missing data.¹⁵ Initial conversations with LAs during the set-up phase confirmed this approach.

The SDQ questionnaire includes five subscales, each with five items, that measure: 1. Emotional symptoms; 2. Conduct problems; 3. Hyperactivity/inattention; 4. Peer problems; 5. Prosocial behaviour. FCs score from 0 to 2 on each item using a scale 'not true', 'somewhat true' or 'certainly true', thus producing a score for each subscale from 0 to 10, where a lower total score is a better outcome for items 1-4, and the reverse for item 5 (prosocial behaviour). The primary outcome in the analysis of the efficacy trial will be the overall 'externalising' score (from 0 to 20), generated by summing the scores of the conduct and hyperactivity subscales (internal consistent Cronbach's alpha 0.78).

¹⁵ The SDQ is collected annually through the SSSA903 and required for all children who have been in care for over 12 months and are aged between 4 years old and 16 years old (inclusive), unless the carer refuses to complete an SDQ. It is usually administered as part of a YP's health assessment shortly after coming into care and any point in the year. The evaluation of the Mockingbird programme found that SDQ data was often missing from SSSA903 submissions (Ott et al., 2020) but early indications on this trial are that it is more complete.

We plan to include data collection by FCs for all YP who are eligible at baseline, where FCs have not opt-outed of the trial. Follow-up data will be collected for all these YP in the trial. Given that the situations of YP, FCs, SSWs and YPSWs may have changed during the course of the year, the evaluation team will work with the LA to update the information that has been provided at baseline prior to the collection of follow-up data. If YP have changed placements, we will ask the LA to pass along the 'opt out' sheet and contact information for their new carer or key worker in residential or supported accommodation. If still FCs receive the survey for a young person who has moved in error, we will see this in the survey findings and FCs will be asked to pass the information and SDQ survey onto the young person's new carer/key worker in a residential or supported accommodation. FCs will be asked to complete a follow-up questionnaire to cover their own outcomes even if their eligible YP is no longer in their care.

Secondary outcomes

Secondary outcome measures will measure changes in outcomes for YP, FCs, and SSWs/YPSWs, in line with the programme's theory of change.

1. Young people

The first two secondary outcomes for YP will be the internalising and prosocial subscales of the SDQ.

The third and fourth secondary outcomes relate to YP in care experiencing more stable placements and a reduction in transition into residential care (RQ2). Placement stability will be measured through unplanned moves as recorded in the SSSDA903. There is a requirement to record the reason for placement change, using standardised codes. In the context of this evaluation, an unplanned move is defined as: 'Carer requests placement end due to child's behaviour', 'Carer(s) requests placement end other than due to child's behaviour', and 'child requests placement move.' Transition into residential care will be measured through SSSDA903 recording on transfer to residential care funded by social care services.

The fifth secondary outcome measure for YP is about involvement with the criminal justice system, measured through information on SSSDA903 in relation to convictions (RQ3). SSSDA903 includes a question on whether the child was convicted or subject to a youth caution (including a youth conditional caution) under the Crime and Disorder Act 1998 during the year for an offence committed while being looked after. This question on child conviction must be completed for all children aged 10 or over who had been looked-after continuously for at least 12 months.

The six secondary measure is a proxy for youth involvement in violence and exploitation of youth (including victimisation). Missing episodes is found to be a key indicator and consequence of criminal exploitation, including of YP in care (Missing People & ECPAT UK,

2022). There is a growing body of evidence linking child disappearance to an increased risk of involvement in crime (Heerde, Hemphill & Scholes-Balog, 2014; Shalev, 2011) and criminal exploitation (National Crime Agency, 2017; The Children’s Society, 2018). This item will be handled sensitively to avoid the criminalisation of children in care. Because of standardised and mandated reporting of children in care who are missing as well as vulnerabilities, missing reports for children in care are greater than their peers. We anticipate using instances coded as ‘M - Missing from care: a looked-after child who is not at their placement or the place they are expected to be (for example school) and their whereabouts is not known’ and ‘A - Away from placement without authorisation: a looked-after child whose whereabouts is known but who is not at their placement or place they are expected to be and the carer has concerns or the incident has been notified to the local authority or the police.’

Using administrative data as secondary measures for YP will minimise the data collection burden on participants and will track YP easier even if they change carers. LAs are required to submit a SSDA903 return for every child who is looked after during the course of the year ending 31 March (deadline for submission is 28 June of each year) (Department for Education, 2023). SSDA903 submissions follow clear guidance, are embedded in routine practice, and are subject to validation checks. The evaluation team has positive experience using SSDA903 data on placement breakdown and missing for evaluation purposes (Ott et al., 2020).

The evaluation team will collect SSDA903 data from LAs at baseline and follow-up. The evaluation team will collect SSDA903 data for all included YP for the year March 2023 - March 2024 (submitted June 2024), and for March 2024-2025 (submitted June 2025). During launch meetings, we will be discussing timelines for submission with LAs, and explore the earliest feasible date for receiving the collated data. SSDA903 data will be matched to the trial young people via a unique identifier.

Box 2. SSDA903 data collected for YP

Outcome	Measure	SSDA903 Question, items and definitions
SSDA903 SDQ	Total Difficulties score	Collected annually for all children (and young people) who have been looked-after continuously for at least 12 months on 31 March per year and who were aged between 4 years old and 16 years old (inclusive) on the date of the last assessment (the date of the assessment will be up to local authorities to decide but it will occur at least once in an annual cycle). Therefore, children aged between 4 years and 16 years 364 days at the time of their last assessment will be included

Placement stability	Unplanned moves	For episodes that cease due to a change in placement, there is a requirement in SSDA903 to record the reason for placement change using a set of codes. In this evaluation codes indicating unplanned move are: 'Carer requests placement end due to child's behaviour', 'Carer(s) requests placement end other than due to child's behaviour', and 'child requests placement move'
Transition into residential care	Transition into residential care	SSDA903 asks for details on data and reason for episode ceased. One of the items relates to: Transferred to residential care funded by adult social care services (Item E7)
Involvement with the criminal justice system	Conviction or subject to youth caution	SSDA903 includes a binary question on child conviction: 0 - Child has not been convicted or subject to a youth caution (including youth conditional caution) during the year 1 - Child has been convicted or subject to a youth caution (including youth conditional caution) during the year Includes offences committed before 1 April (start of reporting year) if the charge was not brought until some point during the twelve months ending 31 March (end of reporting year) and the offence was committed while the child was looked-after. Does not count offences committed while the child was not looked-after. A breach of an order is not regarded as a new offence unless it relates to a Criminal Behaviour Order (CBO) under the Anti-Social Behaviour, Crime and Policing Act 2014. In the case of a breach of a Criminal Behaviour Order (CBO), a new offence is deemed to have occurred.
Missing from care episodes	Missing from care	Episode where a child was 'missing' or 'away from placement without authorisation' during the year should be recorded on the SSDA903 regardless of duration according to the following definition: - <i>Missing from care</i> : a looked-after child who is not at their placement or the place they are expected to be (for example school) and their whereabouts is not known - <i>Away from placement without authorisation</i> : a looked-after child whose whereabouts is known but who is not at their placement or place they are expected to be and the

		carer has concerns or the incident has been notified to the local authority or the police.
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2. Foster Carers

Secondary outcome measures for FCs relate to their professional quality of life (RQ7) and understanding of the impact of trauma on the behaviour of YP in their care (RQ8).

We will use the Professional Quality of Life scale (ProQOL) (Stamm, 2010) which has been used with UK FCs in other studies (Ottaway & Selwyn, 2016; Hannah & Woolgar, 2018; Teculeasa, 2022), and is reported to have good construct validity (according to Stamm, 2010). It is a 30-item self-report measure of the positive and negative aspects of helping professions. The ProQOL has three discrete scales: compassion satisfaction (10 items), burnout (10 items), and secondary traumatic stress (10 items). The measures have good psychometric properties from a range of populations including FCs and social workers (Hannah & Woolgar, 2018).¹⁶ The questionnaire may be freely used as long as the author is credited and no (substantive) changes are made. The ProQOL will be scored using the method outlined in the ProQOL Manual (Stamm, 2010), and separate scores will be reported for each of the scales.

FCs' attitudes to TIP will be measured through a bespoke measure at baseline and follow-up, drawing on questions of the ARTIC scale (described in detail below). This decision is informed by the fact that no suitable validated measure could be identified on the basis of a review of similar trauma-informed training programmes. Common parenting scales were also considered. For instance, the Resource Parents Knowledge and Beliefs Survey (RPKBS) (Sullivan et al., 2015) has been used in several studies as a measure in determining knowledge of trauma-informed parenting, tolerance for behaviours, and confidence in parenting among foster parents (Bartlett & Rushovich, 2018; Lotty et al., 2020; Murray et al., 2019; Sullivan et al., 2015). However, based on a review of the questionnaire with the Fostering Connections programme team, it is assessed to have only partial face validity. Based on further consultation with the programme delivery team, the evaluation team has selected the most relevant items from the ARTIC scale (see further detail below). A composite score will be calculated based on a select number of items. We will pilot surveys prior to implementation.

3. SSWs and YPSWs

¹⁶ Compassion satisfaction $\alpha = .88$, $n = 1130$; burnout $\alpha = .75$, $n = 976$; compassion fatigue $\alpha = .81$, $n = 1135$; inter-scale correlations: 2% shared variance [$r = -.23$; $co-\sigma = 5\%$; $n = 1187$] with secondary traumatic stress; 5% shared variance [$r = -.14$; $co-\sigma = 2\%$; $n = 1187$] with burnout (Stamm 2010).

Secondary outcome measures for SWs relate to their attitudes to TIP (RQ5).

Use of trauma-informed approaches by YPSWs and SSWs will be measured through the 'Attitudes Related to Trauma-Informed Care' (Baker et al., 2016). The ARTIC scale is based on the theory that professionals' attitudes are an important driver of their behaviour, and that a change in staff beliefs could lead to meaningful practice change. No other potential validated measures with satisfactory face validity could be identified by the evaluation team. The ARTIC-45 consists of 45 questions with 7 core subscales: Underlying causes of problem behaviour and symptoms, Responses to problem behaviour and symptoms, On-the-job behaviour, Self-efficacy at work, Reactions to the work, Personal support of trauma-informed care, System-wide support of trauma-informed care (Cronbach's alphas = .93).

Compliance

Compliance to the intervention will be assessed quantitatively in relation to YPSW and SSW receiving the training and support that is part of Fostering Connections. As part of the IPE (see below), we will collect programme monitoring data from delivery partners in relation to attendance of the training and support sessions and analyse correlation with outcomes. Trauma-trainers will also be asked by the delivery team to complete a training delivery record post-session to monitor coverage of intended content.

The baseline and follow-up survey will collect data on training and support on TIP/RP approaches received by the control group in order to establish whether there is extensive contamination between the groups.

Analysis

The outcomes data for the trial is collected on YP, FCs, YPSWs and SSWs. The details of the analysis vary dependent on the unit of data collection. In this section we outline the analysis that will be done across each of these different units, subject to our current assumptions about the trial design. The analysis plans are set out in full in the trial Statistical Analysis Plan.

1. Outcomes for YP

Outcomes for YP will be from two sources: the main SDQ outcomes which will be collected via FCs as bespoke data collection for the trial; and SSSA903 outcomes (including the SSSA903 SDQ total difficulties score).

The main analysis of the YP data will be on an intention-to-treat basis. Estimates of impact per outcome will be regression-based¹⁷, with the equivalent baseline outcome being entered

¹⁷ Linear regression for continuous outcomes and logistic regression for binary outcomes.

as a covariate. Local Authority will be entered as a fixed effect. For SSDA903 outcomes, for YP just entering foster care there will be no baseline data applicable. Baseline SSDA903 outcomes will be coded as categorical: present; absent; not applicable.

The analysis will be based on aggregated data from across all areas. Standard errors will take into account the clustering of the trial data within YPSWs and SSWs. Impacts will be presented as Hedges' G effect sizes. For the YP secondary outcomes the tests will be corrected for multiple comparisons using Hochberg's step-up procedure¹⁸.

As described in earlier sections, the primary analysis will focus on the YP where either both the SSW and YPSW have been assigned to the intervention group or both have been assigned to the control group (Arms 1 and 2 as described in the randomisation section). YP from Arms 3 and 4 (where the SSW and YPSW have been assigned to different groups) will be excluded from this analysis.

An exploratory analysis for each of the primary and secondary YP outcomes will include Arms 3 and 4. For this analysis the trial will be assumed to follow a factorial design. The regressions for this analysis will generate three effect estimates: the effect of the intervention being delivered to SSWs; the effect of the intervention being delivered to YPSWs; and the additive effect of the intervention being delivered jointly to SSWs and YPSWs (that is, the interaction effect). Differences between the effect sizes will be tested for significance. Given that Arms 3 and 4 are not balanced with Arms 1 and 2 (see randomisation section), this analysis will be presented as having potential biases.

The primary outcome analysis will be subject to a range of sensitivity tests, with the full range of these being detailed in the Statistical Analysis Plan. However, a key one will be a test of whether contamination in the trial has led to a dilution of effect sizes, where by contamination is meant YP who are in the intervention group (Group 1) at randomisation, but are subsequently assigned to a YPSW or SSW who has been randomised to the control group, and vice versa. To test this we will exclude the contaminated cases and run a second regression model without them. This regression model will be run with and without a fuller range of covariates to control for any observable imbalance between the randomisation groups after the exclusions.

Given that some YPSWs and SSWs who are randomised to the intervention may not take up or complete the training, we will undertake CACE analyses that assumes the impact of non-participation on YP is zero or close to zero. Sensitivity analyses to test the impact of differing assumptions about missing data will also be included.

¹⁸ This is the approach recommended by What Works for Children's Social Care.

2. Outcomes for FCs

There are two secondary outcomes collected from FCs: the ProQOL score and TIP attitude score.

The analysis of this data will also be regression-based and will follow the same overall plan as for YP, and with similar sensitivity checks. For this analysis a foster-carer level dataset will be created. As with YP, standard errors will take into account the clustering of the trial data within YPSW/SSWs.¹⁹ Impacts will be presented as Hedges' *g* effect sizes. For the FC outcomes the tests will be corrected for multiple comparisons using Hochberg's step-up procedure.

3. Outcomes for YPSWs and SSWs

The outcome for YPSWs and SSWs in the trial will also be analysed via regression-models, and on an intention-to-treat basis. The regression will be specified broadly as for YP and FCs, but without clustering effects for YPSWs. (There will be some clustering of SSWs within YPSWs and this will be accounted for.)

A joint YPSW/SSW analysis will be undertaken but, subject to some evidence of impact, separate models will be run for YPSWs and SSWs.

As with the other analyses, sensitivity checks will be carried out and assuming some social workers in the intervention group do not take up the training, a complier average causal effect (CACE) analysis will be undertaken.

Sub-group analysis

The trial is relatively small, with an expected sample size of just over 500 YP per arm with complete baseline and follow-up data on the primary outcome. There are no prior expectations of large differential impacts across sub-groups, and the sample size is too small for modest differences across groups to be identified. For these reasons, very little sub-group analysis is planned. The exception is that the primary YP outcome will be presented split by ethnic group (generated via interaction effects in the regression model) to facilitate future meta-analysis.

Longitudinal follow-ups

Not applicable

¹⁹ A complication is that a single Foster Carer may have YP with different YPSWs. So the appropriate cluster for the analysis is likely to be the SSW, although this will be clarified once the randomisation design per LA is more concrete.

Implementation and process evaluation

Research questions

The objective of the implementation and process evaluation (IPE) is to provide a comprehensive analysis of the feasibility and understanding of the programme implementation. The IPE will focus on appropriateness, acceptability, feasibility, and fidelity/adaptation of the programme, which are considered lead implementation outcomes (Proctor et al., 2011). The Consolidated Framework for Implementation Research (CFIR) will inform the analysis of implementation determinants (i.e., barriers and enablers) – key to assessing feasibility – relating to the intervention, delivery settings, target families, and wider contexts (Damschroder et al., 2022).

The IPE will also explore causal mechanisms. As outlined in the theory of change, the relationships between the SSW and YPSW, the SW team and the FC, the FC and the YP, and between the YP and the YPSW are central to the assumed causal mechanisms underpinning the programme.

The research questions are:

RQ11: What are the perceived impacts of the Fostering Connections programme on SSWs, YPSWs, FCs and YP?

RQ12: Are there any unintended consequences or other negative effects of the Fostering Connections programme?

RQ13: What factors contribute to observed outcomes?

RQ14: Can Fostering Connections be delivered as intended, with fidelity including to dosage and reach, and what adaptations are necessary and/or made?

RQ15: Is Fostering Connections and its content and principles viewed as feasible, appropriate and acceptable by SSWs, YPSWs, FCs, and YP?

RQ16: Is the implementation support system sufficient and what strategies are required for quality implementation and embedding in practice?

RQ17: How do structural equity factors affect the need for the programme, acceptability, appropriateness and perceived impacts?

Research methods

The IPE will involve the following data collection, also summarised in Table 3.:

Programme administrative monitoring data: Attendance data will be collected by the delivery team to monitor social worker attendance in the training and support sessions. Data will be used to determine reach and compliance, and to assess correlation with outcomes.

Post training feedback survey: The evaluation team will work with the Fostering Connections team in the design of a short post-training feedback questionnaire which will be administered by the delivery team with YPSWs and SSWs attending the training. Such a survey would be built into delivery (i.e., administered at the end of a training session) and explore training uptake, participant responsiveness and acceptability. The survey will incorporate a validated psychometrically tested pragmatic measure of acceptability, notably the Acceptability of Intervention Measure (Weiner et al., 2017).

Training delivery record: Records will be completed by trauma-trainers post-session to monitor coverage of the intended content, adaptations, and to rate participant engagement (administered by the delivery team). Analysis of this data will inform assessment of dosage, responsiveness and adaptation.

Observation: The evaluation team will observe training, workshops and learning event (n=approximately 10 sessions overall) and review the e-learning resources. Sessions will be sampled for spread across LAs and type of session. Observations will inform assessment of participant engagement and responsiveness, and quality of delivery and adaptation, as well as strengthen the evaluation team's understanding of the programme. During observations, researchers will complete a structured pro forma with written notes.

Baseline and follow-up surveys with YPSWs, SSWs and FCs: The survey questionnaire for YPSW/SSW in the intervention group will cover feedback on the Fostering Connections programme and an assessment of appropriateness, acceptability and feasibility. Feasibility of TIP in general is covered by all groups through the use of the ARTIC (or sub-ARTIC questions for foster carers). This data will be collected from the intervention group only.

The FC survey will include selected questions to measure perceived change in their relationship with the YP in their care.²⁰ FCs will also be asked to provide feedback on the support received from their SSW, and their relationship with the SW team (SSW and YPSW).

Qualitative interviews with YPSWs, SSWs, TIP champions and FCs: These interviews will explore implementation strategies, key implementation barriers and enablers, feasibility, perceived impacts and mechanisms of change, and potential unintended or negative effects.

Interviews with YPSWs, SSWs and FCs will also explore changes in relationships and support. Interviews will therefore be at mid-point (after the end of the core training) and towards the

²⁰ We will draw on measures used in other studies that explored the child-foster carer relationship (e.g., Teculeasa et al, 2022; Golding & Picken, 2004; Pianta & Steinberg, 1992).

end of delivery (n= 36 at each time point). YPSWS and SSWs (inc. TIP champions) will be purposively selected from the intervention group, for diversity across local authority, employed by LA versus Independent Fostering Agency, training attendance, and (if possible) years of experience (n ~ 19 YPSW, 19 SSW, 10 TIP champions). FCs will be sampled for diversity in their ethnicity (and, if possible, the ethnicity of the YP in their care), and training attendance by their SSW (n ~ 24). Data on relevant sampling characteristics will be taken from the baseline survey and attendance data.

Qualitative interviews with strategic managers: Interviews with team leaders and / or heads of service will be used to explore fit of Fostering Connections within LA systems, and the feasibility of embedding the programme in social work practice in the longer-term. Perceptions of impact and potential unintended effect will also be explored. These will be 1-2 interviews per LA at mid-point and towards the end of delivery. (n=approx. 14 at each time point). If appropriate and feasible, some of these interviews may be replaced by the evaluation team attending and /or analysing recordings of workshop with LA senior leaders – given they cover broadly the same topic, and to reduce burden on LA staff.

Qualitative interviews with young persons: Interviews will explore perceived impacts, mechanisms of change, and relationships with FCs and YPSWs. These will be organized towards the end of the delivery period (n=20). We will recruit YP through their YPSW. We will ask YPSWs across LAs to invite YPs to indicate interest in being interviewed. We will focus on YP whose YPSW and FC's SSW is in the training group. We will aim for a diverse sample in terms of age, gender, ethnicity, FC/supported lodging.

Focus groups with the core Fostering Connections team: These discussions will be used to understand implementation strategies, feasibility and mechanisms of change, and will be scheduled after training, mid-way through delivery, and towards the end of delivery. (n=3 in total).

Topic guides for interviews and focus group discussions (FGD) will be informed by a review of findings from the co-development stage (e.g., survey and FGD with YP and FC) and training content, and shared with the Fostering Connections team and with a small number of SWs and FCs who are not part of the trial prior to their finalisation. Topic guides will be reviewed regularly with necessary amendments made to ensure appropriateness and relevance. Interviews and FGDs will be conducted by phone or online video platform and recorded on encrypted recording devices. In making the interviews accessible in particular to YP, considerations will include interview duration, vocabulary, and incorporating engaging visual cues.

Other data instruments (attendance sheet, training delivery record) will be agreed with the Fostering Connections team. The survey with social workers and FCs will be piloted, with the

research team and then with SWs and FCs who are not part of the trial including a semi-structured template to gain their feedback on the instruments and the language used.

Analysis

Data from each element of the IPE will be analysed separately, then triangulated and integrated, identifying areas of difference and reinforcement, and using different data sources to substantiate and explain findings. Unique identifiers will be assigned to support linking of data (e.g., correlation of training attendance and outcomes).

Qualitative data will be digitally recorded and transcribed verbatim. Thematic analysis (Braun & Clarke, 2006) as operationalised in the Framework Approach will be used to structure, explore and interpret qualitative data from interviews, records and observation. Themes will be developed both deductively (e.g. reflecting the theory of change, CFIR) and inductively (including unexpected, unintended and negative consequences) (Spencer et al., 2013; Gale et al., 2013). Our analysis will explore YP and SSW/YPSW descriptions of the impact of, and their perceptions of, the causal mechanisms leading to change. All other numerical programme monitoring data will be analysed with descriptive statistics to inform dosage, reach and fidelity of the programme.

Quantitative data from the survey with FCs will be integrated to answer RQ13 in relation to mechanisms of change (e.g., changes in relationships).

We will use well documented dimensions of implementation science to understand how the programme was implemented, the barriers and facilitators to implementing Fostering Connections as intended, and the perceived feasibility, acceptability, and appropriateness of the programme.

Table 3: IPE methods overview

Research methods	Data collection methods	Participants/ data sources (type, number)	Data analysis methods	Research questions addressed	Implementation/ logic model relevance
Programme monitoring data	Entered by Fostering Connections team and YPSWs and SSWs	Covers all trainers, and YPSWs and SSWs in the intervention group	Descriptive quantitative analysis Correlation with survey	14	Fidelity (, reach) and compliance

			data on outcomes		
Post-training feedback survey	Administered by the delivery team	YPSWs and SSWs attending training	Descriptive quantitative analysis, coding of open-ended questions	14, 15	Fidelity (responsiveness, acceptability)
Training delivery records	Entered by trainers	All trainers for all sessions	Descriptive quantitative analysis	14	Fidelity (dosage, responsiveness, adaptation)
Observation data	Observation of sessions/events	10 sessions, spread across LA and type of event	Qualitative thematic analysis	14	Fidelity (responsiveness, adaptation), acceptability
SSW/ YPSW and FC follow up survey	Online survey (as part of outcome survey)	SSWs, YPSWs, FCs across allocated and control trial arms	Descriptive quantitative analysis	13, 15, 16	Feasibility, acceptability, appropriateness, fidelity, mechanisms of change
In-depth interviews with YPSWs, SSWs, TIP champions and FCs	Qualitative interview	72 interviews in total purposively selected (36 at mid-point and 36 towards end of delivery)	Qualitative thematic analysis	11, 12, 13, 15, 16, 17	Implementation, feasibility, acceptability, appropriateness, perceived impacts and mechanisms of change
In-depth interviews with strategic managers	Qualitative interview	28 interviews in total, 1-2 per local authority (14 at mid-point, 14 towards the end of delivery)	Qualitative thematic analysis	11, 12, 13, 14, 15	Feasibility, implementation, perceived impacts
In-depth interviews with YP	Qualitative interviews	Purposively selected sample of YP (n=20) towards end of delivery	Qualitative thematic analysis	11, 12, 13, 15	Perceived impacts, acceptability

Focus group with Fostering Connections team	Focus group discussion	All Fostering Connections staff. 3 timepoints (after training, mid-way and towards end of delivery)	Qualitative thematic analysis	13, 14, 16, 17	Implementation strategies, fidelity, feasibility, mechanisms of change
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Cost data reporting and collecting

Our approach to the cost data collection, analysis and reporting will be informed by the YEF guidance on Cost Reporting (Youth Endowment Fund, 2022). The approach will be guided by the following principles and assumptions:

- Estimates account for the costs of delivering the programme, and not the evaluation or programme development.
- Estimates are derived using a 'bottom-up' approach.
- Estimates are informed from the perspectives of all organisations and individuals involved in delivering the programmes, in this case, Leap CC, NCB, LA leaders, YPSW, and SSW.
- Estimates will include some set-up costs, as certain activities, including preparatory meetings and welcome events with LA stakeholders, will be required every time the programme is delivered in a new setting.

Set-up Costs

These will include the one-off costs needed to set up the programme. While the set-up period involves a few activities (described in earlier sections), we anticipate the following activities to be repeated every time the programme is delivered in a new setting:

1. Welcome event for Heads of Services / team managers across the LAs and engagement meetings with social workers
2. Preparatory meetings with each LA (online)

Senior leaders across LAs will participate in a welcome event. For these staff, costs will be estimated based on the designations of invitees (and where possible paygrades) and sector level staff cost assumptions for salary costs and on-costs, or information in the public domain. Staff costs will be estimated only for the duration of the events and based on planned number

of attendances at each event (as opposed to actual attendances). These events will be supported by NCB members, and estimated time spent by NCB staff will be based on the project budget submitted for this study. In addition to staff costs, we expect that these events will be run using existing buildings and facilities of the LA, and access will not come at an additional cost. Depending on the duration and format of these events, we will consider if it is appropriate to include any other costs.

We will ask NCB to record the planned attendance numbers, the designations of attendees (where possible including their pay scale grade), the duration of the events and other materials provided for each of these events.

Recurring costs

These will include the following activities:

1. Initial e-learning and 10 day face-to-face training of SSWs and YPSWs and 3 online RP sessions, and optional pastoral calls
2. Workshops to co-develop policies supporting TIP and RP (senior LA leaders, project leads and TIP champions)
3. Cross-LA follow up workshops
4. Online peer support forum
5. Half-day online cross-LA networking event
6. Identifying TIP champions in each of the 10 LAs

During intervention delivery, 10 day training will be provided to SSWs and YPSWs (7 training days and 3 online RP sessions). Up to 35 SSWs and YPSWs per cohort will participate in the training with training delivered by Leap CC trainers across the 8 LAs. Additionally, these SSWs and YPSWs will complete an independent online learning module. For these staff (SSWs, YPSWs), costs will – where possible - be estimated based on the actual local costs taken as the pay scale grade in 2024 and will include employee costs such as national insurance and superannuation. In cases where it is proving challenging to get this information from LAs, we will use sector level staff cost assumptions for salary costs and on-costs, or information in the public domain. Costs will be estimated assuming full compliance, that is, we will not base costs estimates on the actual attendance by SSWs and YPSWs. Similarly, costs for wage and non-wage for Leap CC trainers will be obtained directly from Leap CC. We will work with Leap CC to obtain data on these costs for their staff in an anonymised way, where no staff member is named alongside the wages and time spent. Alternatively, if Leap CC would find this disclosure, we would rely on the estimated wages and non-wage costs using ONS data on similar sector and professional qualifications.

Building (training venue) and material costs (printed materials, catering) for the training will be estimated. These costs will be collected from Leap CC after the training has been delivered. Where the training venue is provided by the LA free-of-charge, a decision will be made on whether to estimate the rent based on local market rates within each LA. This decision will be based on whether the venue was provided free-of-charge across all 8 LAs, as that would guide our expectation for a scenario where the programme is either scaled up or replicated in other LAs.

Costs involved in the workshop to co-develop policies supporting TIP and RP within each LA will be calculated in the same way as outlined above for the engagement and welcome events. We estimate that all trained SSWs and YPSWs will participate in the workshops. For these staff (SSWs, YPSWs), costs will be based on the actual local costs taken as the pay scale grade in 2024 or using sector level cost assumptions. Costs will be estimated assuming full compliance, that is, we will not base costs estimates on the actual attendance by SWs and YPSWs. Similarly, costs for wage and non-wage for NCB staff will be obtained directly from NCB where non-disclosive or rely on ONS estimated costs for similar sector and qualifications.

The online peer-support forum and online cross LA learning and networking event will also be managed by NCB staff. For these activities, estimated time spent by NCB staff will be based on the project budget submitted for this study. Costs associated with the time spent will be obtained as wage and non-wage costs for NCB staff directly from NCB where non-disclosive or rely on ONS estimated costs for similar sector and qualifications. We will not estimate the costs for time spent by SSWs and YPSWs on this forum as this may vary greatly from one social worker to another and will be add to their workload if attempting to collect daily. For the networking event, we will estimate the wage and non-wage costs for trained SSWs and YPSWs for the duration of the events, assuming full compliance. TIP champions will be selected from the trained SSWs and YPSWs.

We have not included the visits to FCs and YP by SSWs and YPSWs as recurring costs as these are part of their usual role and responsibility, and the programme does not impose an expectation of additional visits. It is assumed that trauma-informed principles and RP can be integrated in usual practice without additional time requirements. This assumption will be verified based on IPE findings.

Analysis and Reporting

By collecting these costs, we aim to construct a comprehensive picture to give an overall cost of delivering the programme. We will endeavour to also report the average cost per young person in the intervention group, the average cost per social worker in the intervention group (SSWs and YPSWs combined), and the average cost per LA. The intervention group refers to primary group of interest, where both SSWs and YPSWs have been randomised to receive the intervention. In line with YEF guidance, the number of young persons, YPSWs and SSWs will

be based on full compliance, and not take into account reduced numbers due to attrition during the study.

As per YEF guidance, within these categories, we will indicate how the total costs break down to set-up and recurring costs. We will follow YEF's cost guidance when calculating the full cost of delivery, including adjusting costs to constant prices using GDP deflators with 2023 acting as the base year. We will account for uncertainty in the costings provided and document all assumptions made in the final calculations. We do not intend to complete sensitivity analyses. Within our approach described above, we have assumed that no durable equipment will be purchased for the delivery of the programme. Hence, the analysis will not estimate the equipment costs. Our estimates will not include travel costs for SSWs and YPSWs to conduct visits as this expected to be part of their usual responsibility.

Diversity, equity and inclusion

We specialise in working with communities facing adversity and promoting equity, diversity, and inclusion in evaluation and implementation. We recognise the inequalities in child welfare intervention and resources (The Child Welfare Inequalities Project Team, Bywaters & Featherstone, 2020) and the relationship between poverty and abuse and neglect (Bywaters et al., 2022). We recognise the structural inequality and structural racism from which racial disparities and trauma often stem.

Our evaluation team includes members with varying lived experience (including 8 years as a FC, often caring for ethnic minority teenagers), and we plan to use a diverse Evaluation Advisory Group. The Advisory Group's role is to provide guidance and expert insights for the evaluation, for instance in relation to key emerging issues that may be pertinent to the programme, to provide feedback on data collection tools and interpretation of findings, and contribute to the development of recommendations. The group will have representation from practitioners, experts on the topic, and people with lived experience. We will seek additional advice from organisations that represent and support children from the priority equity groups if deemed appropriate. Additionally, the programme is informed by those with lived experience.

We will use the CEI Equity in Evaluation Framework (CEI, n.d.) to surface equity issues and embed an equity, diversity and inclusion (EDI) perspective in in the evaluation. The Framework was informed by Child Trends (Andrew, Parekh & Peckoo, 2019) and writing on equity in implementation. It consists of 23 questions to prompt discussion and consideration of EDI issues, to support reflection and planning of appropriate action. Specifically, these relate to the programme, evaluation processes, our conduct of the evaluation, and how to use evaluation learning to address inequity.

We will work collaboratively to promote diversity and address equity and inclusion (including reaching those with protected characteristics), and feedback ongoing findings to address issues of inequity (e.g., in reach to FC and YP or use of TIPs).

The evaluation team will collect demographic data (sex, age, ethnicity) on care experienced YP through analysis of SSDA903 data at baseline and 12-month follow-up. The SSDA903 follows UK Government guidance on reporting ethnicity, and records date of birth and gender. As noted, no sub-group analysis of outcome data is planned on the basis that there are no prior expectations of large differential impacts across sub-groups, and the sample size being too small for modest differences across groups to be identified.

The evaluation design includes in-depth interviews with a sample of YP. Their voices and perspectives, and the acceptability of TIP to them, are important. We will assess safeguarding risks prior to the start of data collection, and create a project-specific safeguarding protocol. We have also carefully designed the evaluation not to be overly burdensome to YP facing adversity.

Interviews as part of the IPE will explore how structural equity factors affect the need for the programme, acceptability, appropriateness and perceived impacts. Children from Black, Mixed and Other ethnic groups continue to be over-represented in the numbers of children in care.²¹ FCs will be selected purposely for diversity in ethnicity and gender, and we will aim for a diverse sample of YP. The evaluation team is experienced in trauma-informed interviews with FCs, SWs, and YP and will be reflexive of how their own identities, lived experience, and power can affect interviews.

During set-up we will jointly with the LAs establish the estimated population of FCs who may face language barriers to participating in the research, and make necessary arrangements. The evaluation budget therefore includes a line to cover potential translation and interpretation costs, and we have experience doing interviews with interpreters for YP in care.

In the analysis, we will use an equity in implementation science lens to examine the factors at different levels that influence take-up, reach and impact (Baumann & Cabassa, 2020). We will also reflect on our own positionality and biases, stay close to participants' language, and come together to offer different perspectives. We will consider in costs analyses how certain costs may allow for greater equity and present accompanying narratives.

In disseminating findings, we will consider equity including who to reach and influence for the findings to address inequity and any risks in dissemination (e.g., distortion of messages) and how to mitigate. Following publication of the evaluation report by YEF, we plan a public-facing

²¹ Reporting year 2022: Children looked after in England including adoptions. <https://explore-education-statistics.service.gov.uk/find-statistics/children-looked-after-in-england-including-adoptions/2022>

and a LA-facing summary of findings in addition to the report as a first step to addressing power in dissemination of findings.

CEI organises monthly development sessions, which on a regular basis focus on topics related to equity and diversity, and are used as a platform to exchange experience on research with vulnerable and marginalised groups. Additionally, we do internal bespoke training for projects and team members, including on topics such as culturally sensitive interviewing. A specific training session will be organized to cover specificities of interviewing the study population of this evaluation.

Ethics and registration

We have sought formal ethics appraisal from the Social Research Association Ethics Service, and received a favourable opinion on our ethics application. We consider ethics as an ongoing process, discussed at regular team meetings.

Data protection

CEI has given this project an internal Data Protection Identifier (DPID) as part of our robust approach to identify risks posed to the people whose data is being used within the Fostering Connections project. CEI screened this project with our Data Protection Officer (DPO) whom we employ to oversee all data usage activities.

From the risk screening, our DPO identified a requirement for a Data Protection Impact Assessment (DPIA) to be conducted for Fostering Connections, which we have done. Risks that have been identified include the use of administrative information about YP who are in the care system and the matching with further information about YP from FCs within questionnaires we are requesting them to complete. This has allowed CEI to make sure it collects the minimum amount of information and reduce the ability for anyone to be able to identify YP within the information we are collecting.

All CEI employees have to take data protection training to understand the risks involved and are briefed on identified best practices by our DPO from conducting the DPIA. CEI writes and distributes data privacy notices for any people we are collecting data about and we have written a data sharing agreement in accordance with the UK regulator's code of conduct for data sharing which we will have in place with the LAs who are sharing data with us about YP.

We will maintain data protection by design in the way we set up the Fostering Connections project by conducting checks of system settings to keep data at the highest level of security

available and configured to only allow specific named researchers access to only the data they need to access.

For this project CEI will use personal data under UK GDPR Article 6.1(f) “legitimate interests” as the lawful basis for all processing activities involved in the running and delivery of the Fostering Connections project. These processing activities or uses of personal data will include using data to request informed ethical consent for their participation in the evaluation, to participate in interviews and surveys, to transcribe audio recorded in the interviews, to send a survey, to identify a person’s data to be able to respond to any data subject rights requests, and to organise and review the data in the analysis of information for the project. This is not an exhaustive list and the Data Privacy Notice we produce for each collection of data clearly indicates the uses of data which are relevant to each participant and the associated lawful basis for processing.

A note on consent: Ethical practices within research require informed consent (“Ethical Informed Consent”) to be gathered for a person’s participation in the evaluation as a research participant (when interviewed or completing a survey). Ethical Informed Consent is not equivalent to consent as a lawful basis under GDPR (“GDPR Consent”).

For the avoidance of doubt, Ethical Informed Consent is regarded as a supplementary data protection safeguard for the use of personal data under GDPR, which includes for the collection and storage of personal data, and is not equivalent to GDPR Consent because, to be compliant with the rules around the capture of GDPR Consent within the GDPR, should GDPR Consent be withdrawn by a person, CEI must immediately stop using that personal data and delete the personal data. This is a problem for a research project like Fostering Connections because once analysis of personal data is being conducted it would mean any findings derived from the personal data are not legally admissible in the research outputs. This means the analysis would need to begin again.

Ethical Informed Consent will be sought from YPSWs, SSWs and FCs prior to taking part in an interview or survey. Should a data subject withdraw their Ethical Informed Consent before any analysis has begun CEI will delete that personal data and not include it in the project with the goal of meeting data privacy legislative obligations to the Data Subject. Consent or ascent will be sought from young people who are invited to take part in a qualitative interview.

A note on placing data into the YEF Data Archive: For the purpose of archiving data into the YEF Data Archive, consistent with YEF providing a service to the youth sector as required by its funder, the Home Office, archiving activities are conducted under the authority of the Home Office and are therefore processed under Article 6.1(e) of the GDPR: “Processing is necessary for the performance of a task carried out in the public interest” at the point the data is in the YEF Data Archive.

Data archived within the YEF Data Archive is held within an instance of the Office for National Statistics Secure Research Service (“ONS SRS”) for the purposes of secondary research and shall be governed under the UK Digital Economy Act 2017 and the UK Statistics and Registration Service Act 2007. Any activities to match data to the DfE National Pupil Database will also be processed under Article 6.1(e) of the GDPR and CEI is acting under instruction from YEF for these activities.

Any processing of special category personal data or protected characteristics as defined by the UK Equality Act 2010 shall be processed in accordance with UK GDPR Article 9.2(j) which states “processing is necessary for archiving purposes in the public interest, scientific or historical research purposes or statistical purposes”.

As an organisation, CEI is unable to utilise Article 6.1(e) of the UK GDPR “Processing is necessary for the performance of a task carried out in the public interest,” (also known as “Public Task”) for the uses of personal data because there needs to be a basis in law to do so. The Public Task lawful basis is usual for government bodies and higher education institutions to use because of laws such as the Local Government Act or similar although there is no such law governing CEI’s work in on this project.

CEI has been commissioned by the Youth Endowment Fund to work on this project based on its mission to support the use of the best evidence in policy and practice to improve the outcomes for children and YP. CEI’s legitimate interest in processing personal data is for societal benefit to support the use of better evidence to improve life for children, families, and communities. We believe good evidence and effective implementation have the power to solve our most pressing social problems.

CEI demonstrates GDPR compliance externally through its comprehensive website-based [Data Privacy Policy](#). Where a data subject interacts with CEI, where the processing of their personal data is different to that specified on our website, we produce a relevant Data Privacy Notice in accordance with the information required of such a fair processing notices pertaining to either article 13 of the GDPR, if we are collecting personal data about a data subject directly from a data subject, or article 14 of the GDPR, if we are collecting personal data about a data subject from another party. These notices are provided at the point of collecting that personal data, or, where collection is indirect, if we have identified there is a disproportionate effort to provide a data subject with a privacy notice we will make a record of this with the reason why we believe this to be so.

Each Data Privacy Notice holds a copy of a data subject’s data protection rights and a contact email address for such requests to be made (dpo@theevidencequarter.com). Compliance for data processing is demonstrated for each project we conduct through our internal data protection review procedures. The first step in our data protection review procedures is for our project managers to complete a data protection risk screening (Privacy Impact

Assessment) form that is sent to our [Data Protection Officer](#) (DPO). The DPO will assess the requirement for a Data Protection Impact Assessment (DPIA) and assists us in the completion of the DPIA where required.

The DPIA outlines all purposes for processing personal data alongside the lawful basis for doing so, the retention periods for any data collected specifying points of minimisation throughout any project, and who the personal data will be transferred to which could include other controllers or processors and the technical, organisational and/or contractual measures which need to be in place to make such processing compliant.

There are a number of points of collection of personal data that are relevant to the Fostering Connections project and will be used by CEI. Understanding the points of collection of data is important to understand the data processing roles of the organisations processing/using the personal data.

Data being used in the project by CEI includes two time points of collecting: SSDA903 data about YP; surveys completed by YPSWs, SSWs and FCs (inc. SDQ data); interviews with YP, strategic managers, SWs, FCs, and focus groups with the Fostering Connections Team.

The interviews, focus group, and surveys: CEI will be the Data Controller for the personal data of all individuals that attend either an interview, complete a survey or attend a focus group. CEI uses third-party suppliers to support its work in conducting and often recording interviews and focus groups which will also be transcribed, as well as digital survey platform providers. Each of these suppliers is a Data Processor on behalf of CEI and CEI maintains up to date Data Processing Agreements with all suppliers in accordance with the requirements of Article 28 of the GDPR.

SSDA903 data about YP: The Local Authority collects SSDA903 data about young persons in their care on an annual basis as a standard practice. This data is collected despite the research and is a normal annual practice for a Local Authority. The LAs are the Data Controller for this data for their own purposes. A copy of selected SSDA903 data will be securely shared with CEI for the purpose of the research within the project and at that point CEI will be the Data Controller for their copy of the data. This will be pseudonymised data.

Strengths & Difficulties Questionnaire (SDQ) data: CEI will request FCs to complete a SDQ about YP on CEI's behalf. CEI will be the Data Controller for the SDQ data it collects specifically for the Fostering Connections project.

The YEF Data Archive: Upon conclusion of the analysis phase of the project YEF have instructed CEI its desire for this combined dataset to be archived in the YEF Data Archive. CEI will transfer the combined dataset to the ONS SRS on behalf of YEF. CEI will also send the list of UPNs of participants to the DfE for matching purposes in the archive. CEI is acting as a Data Processor on behalf of a Data Controller (YEF) for this transfer. Once the dataset has been

successfully transferred into the YEF archive, YEF will be the sole Data Controller for the copy of this data and CEI relinquishes all responsibility or controllership of the dataset residing in the YEF Data Archive.

CEI will retain copies of all personal data collected throughout the project for a further 2 years as a reasonable retention period should the data require reanalysis or repeat analysis sometimes desired of research datasets in this field of study. CEI will remain the Data Controller for this data until the data is securely deleted.

Stakeholders and interests²²

Developer and delivery team (key members):

- Caroline Coady (NCB, Assistant Director – Social Care): Intervention co-developer
- Georgia Macqueen Black (NCB, Social Care Programme Lead): Intervention co-developer
- Bianca Karpf (NCB, Social Care Programme Lead): NCB lead for delivery
- Alex Mckell (Leap CC, Head of Innovation): Intervention co-developer
- Denise Allen (Leap CC, Director of Delivery): Intervention co-developer and lead for delivery

Evaluation team:

- Dr Eleanor Ott (CEI, Associate Director): Principal investigator, responsible for delivery of all stages of the project to a high quality and on time, and leading the IPE.
- Anne-Marie Baan (CEI, Principal Advisor): Project Manager, responsible for day-to-day management across all elements, overseeing trial implementation and data collection-leading and undertaking the IPE.
- India Thompson (CEI, Senior Research Assistant): Researcher, supporting trial implementation, data collection and analysis and providing administrative and research support throughout.
- Dr Sweta Gupta (CEI, Principal Advisor): Analyst, responsible for providing expertise for the cost evaluation.
- Dr Susan Purdon (BPSR, Partner): Analyst, responsible for leading the design, analysis and reporting of the RCT.

²² This section has not been updated.

- Caroline Bryson (BPSR, Partner): Analyst, responsible for leading the design, analysis and reporting of the RCT.

Other stakeholder involvement:

- Advisory Group: Representation from practitioners, experts on the topic, and people with lived experience, responsible for providing guidance and expert insights for the evaluation.
- LAs: Partners in delivery, responsible for advising on the evaluation design and collecting / sharing of relevant data with the evaluation team.

No other sources of funding/support or conflict of interest.

Risks

The table below outlines the risks and proposed mitigations as included in the original protocol. The evaluation team provides quarterly updates on risks to YEF.

Risk	Mitigation
<p>Proposed randomisation approach is not feasible in LA to team structures and processes</p> <p>(Likelihood: medium; Impact: medium)</p>	<ul style="list-style-type: none"> ● We worked with LAs during the set-up phase to establish the best approach to randomisation. The approach and evaluation protocol have been amended accordingly. ● Processes will be established and agreed with LAs on handling movements of YPSWs and SSWs. Potential contamination will be monitored and sensitivity analysis will be conducted.
<p>LA support to data collection for the trial is low</p> <p>(Likelihood: medium; Impact: medium)</p>	<ul style="list-style-type: none"> ● Early engagement of LAs by NCB during the co-design phase. Ensure that LAs understand all that is being asked of them, and commit to completing all of the trial tasks. ● Financial incentive for LAs participating in the trial to cover time involved in data collection. ● Respective roles of NCB, Leap CC, and the evaluation team will be set out in detail and agreed. LAs will be asked to appoint focal points for the trial. ● The CEI team will be actively working throughout to support the trial, and follow-up actively with LA focal points.

	<ul style="list-style-type: none"> ● CEI has designed data collection to minimize burden on LAs and for CEI to do direct data collection.
<p>Low response rate in baseline and follow-up surveys (SWs, FC)</p> <p>(Likelihood: medium; Impact: medium)</p>	<ul style="list-style-type: none"> ● During the set-up period we explored with LAs optimal ways for distribution and introduction of the survey, including by embedding data collection within practice. ● Ensure that LAs understand all that is being asked of them and commit to managing primary data collection as per agreed roles and responsibilities. ● CEI will work actively to ensure as close to 100% data collection as is feasible, inc. by close engagement with LA focal points, and email and telephone chasing of surveys. For data collection from FCs, practitioners will use skills in trust building to encourage general participation, supported by clear, positive information sheets which will be piloted. ● Incentives offered to FCs ● Piloting of survey questionnaires
<p>Low participation in qualitative research by FCs, YPSWs, SSWs managers, and YP</p> <p>(Likelihood: low; Impact: medium)</p>	<ul style="list-style-type: none"> ● Clear, positive information will be provided to support recruitment ● Flexibility in interview scheduling times ● Interviews will be short and concise. Feasible, targeted instruments will be piloted and used. ● Incentives offered to FCs and YP to participate in data collection ● CEI staff are experienced in building engagement and approaching interviewees in an engaging, enabling way. ● Sample targets are realistic ● CEI will carry out telephone chasing calls and are highly skilled at encouraging participation by marginalised groups.

<p>Difficult to detect impact due to quality of data (e.g., SSDA903), high pre-existing knowledge of TIP, contamination between control and intervention group</p> <p>(Likelihood: medium; Impact: medium)</p>	<ul style="list-style-type: none"> • CEI and BPSR are highly experienced at supporting data quality. Baseline data will be scrutinised and additional training offered to LAs/SWs if necessary. • Use of validated measures where feasible, and data collection tools and flows will be piloted. • Assessment of practice as usual at baseline and follow-up. • Ensure that LAs understand the evaluation design and commit to adhering to the guidance provided. • Contamination will be assessed and sensitivity analysis conducted
<p>Evaluation staff absences (e.g., illness, periods of leave, staff turnover).</p> <p>(Likelihood: low; Impact: low)</p>	<ul style="list-style-type: none"> • CEI has a staff over 40+ plus associates and consultants and uses a cutting-edge work planning and scheduling system.
<p>Delays in securing positive ethics review.</p> <p>(Likelihood: low; Impact: medium)</p>	<ul style="list-style-type: none"> • We use an ethics review which offers rapid response and practical actionable advice. • The BPSR/CEI team is highly experienced and has submitted 50+ successful research ethics applications.

Timeline²³

Dates	Activity	Staff responsible/leading
Phase 1		

²³ Timing of data collection activities has been revised to reflect the delay in intervention delivery.

July – Sept 2023	Contracting, project initiation and ethical approval	CEI
Sept – Dec 2023	Design and set up	CEI & NCB
Phase 2		
Feb-July 2024	Baseline data collection (on outcomes for YPSWs, SSWs, FCs, and YP) and randomisation	CEI & BPSR
April 2024 – December 2024	Delivery of intervention (three waves February – March – April)	NCB & LEAP
July 2024 – December 2024	IPE data collection part 1	CEI
July 2024	Baseline data collection YP covering April 23-March 24 (SSDA903)	CEI & BPSR
Nov 2024 – June 2025	IPE data collection part 2	CEI
Feb – June 25	Follow-up data collection (on outcomes for YPSWs, SSWs, FCs, and YP)	CEI & BPSR
July 2025	Follow-up data collection YP covering April 24- March 25 (SSDA903)	CEI & BPSR
Phase 3		
Feb – Sept 2025	Analysis and reporting	CEI & BPSR
September 2025	Draft report	CEI & BPSR
November 2025	Final report	CEI & BPSR

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Appendix: Illustration of the random allocation steps

In this appendix the steps in the randomisation process for each LA are illustrated. For this illustration there are 46 YP, 14 YPSWs and 10 SSWs.

Table 1 shows the distribution of the 46 YP across the two teams of social workers. The rows represent the YPSWs and the columns the SSWs. The final columns and rows show the totals. The table is sorted on the total number of YP for each YPSW (high to low).

The cells in green (one per SSW) show the YPSW that each SSW is assigned to, this being the YPSW with whom they share the most eligible YP. The green cell is the cell with the maximum value in the column or, where there are ties for maximum, one is selected at random. This is the 'unique' YPSW for each SSW.

Table 1: The distribution of 46 YP across 14 YPSWs and 10 SSWs

	SSW001	SSW002	SSW003	SSW004	SSW005	SSW006	SSW007	SSW008	SSW009	SSW010	Total (sorted on total # YP)
YPSW14	1	1	1			2	1		1		7
YPSW13	3					1	1		1		6
YPSW02			1	1	1				2		5
YPSW12			2					1		2	5
YPSW01	1	1					1	1			4
YPSW04								1		3	4
YPSW06					1			1	1		3
YPSW11				3							3
YPSW09					1		1				2
YPSW10	1				1						2
YPSW05			1				1				2
YPSW03			1								1
YPSW07	1										1
YPSW08				1							1
Total	7	2	6	5	4	3	5	4	5	5	46

Table 2 repeats Table 1 but with a final column added showing the random allocation to intervention or control for the YPSWs.

Table 2: Random allocation of YPSWs to intervention or control

	SSW0 01	SSW0 02	SSW0 03	SSW0 04	SSW0 05	SSW0 06	SSW0 07	SSW0 08	SSW0 09	SSW0 10	Total (sorted on total # YP)	Random allocation of YPSW (1=interventi on; 0=control)
YPSW 14	1	1	1			2	1		1		7	1
YPSW 13	3					1	1		1		6	0
YPSW 02			1	1	1				2		5	1
YPSW 12			2					1		2	5	0
YPSW 01	1	1					1	1			4	1
YPSW 04								1		3	4	0
YPSW 06					1			1	1		3	1
YPSW 11				3							3	0
YPSW 09					1		1				2	0
YPSW 10	1				1						2	1
YPSW 05			1				1				2	0
YPSW 03			1								1	1
YPSW 07	1										1	0
YPSW 08				1							1	1
Total	7	2	6	5	4	3	5	4	5	5	46	

The final row of Table 3 now adds the assignment of each SSW to intervention or control, based on the random allocation of their unique YPSW.

Table 3: Assignment of SSW to match that of the allocation of their ‘unique YPSW’ (the green cells)

	SS W0 01	SS W0 02	SS W0 03	SS W0 04	SS W0 05	SS W0 06	SS W0 07	SS W0 08	SS W0 09	SS W0 10	Total (sorted on total # YP)	Random allocation of YPSW (1=intervention; 0=control)
YPSW14	1	1	1			2	1		1		7	1
YPSW13	3					1	1		1		6	0
YPSW02			1	1	1				2		5	1
YPSW12			2					1		2	5	0
YPSW01	1	1					1	1			4	1
YPSW04								1		3	4	0
YPSW06					1			1	1		3	1
YPSW11				3							3	0
YPSW09					1		1				2	0
YPSW10	1				1						2	1
YPSW05			1				1				2	0
YPSW03			1								1	1
YPSW07	1										1	0
YPSW08				1							1	1
Total	7	2	6	5	4	3	5	4	5	5	46	
Assignment of SSW to match that of the allocation of the YPSEW in the green cell	0	1	0	0	1	1	0	0	1	0		

Table 4 shows the arm that the young people in each cell are assigned to, based on the allocations of their YPSWs and SSWs. The colour coding is as follows:

Arm 1: Both YPSW and SSW assigned to the intervention

Arm 2: Both YPSW and SSW assigned to the control group

Arm 3: SSW assigned to the intervention and YPSW to the control group

Arm 4: SSW assigned to the control group and YPSW to the intervention

Table 4: Assignment of YP to each arm of the trial

	SS W0 01	SS W0 02	SS W0 03	SS W0 04	SS W0 05	SS W0 06	SS W0 07	SS W0 08	SS W0 09	SS W0 10	Total (so rte d on total # YP)	Rando m allocati on of YPSW (1=inter vention; 0=contr ol)
YPSW14	1	1	1			2	1		1		7	1
YPSW13	3					1	1		1		6	0
YPSW02			1	1	1				2		5	1
YPSW12			2					1		2	5	0
YPSW01	1	1					1	1			4	1
YPSW04								1		3	4	0
YPSW06					1			1	1		3	1
YPSW11				3							3	0
YPSW09						1		1			2	0
YPSW10	1				1						2	1
YPSW05			1					1			2	0
YPSW03			1								1	1
YPSW07	1										1	0
YPSW08				1							1	1
Total	7	2	6	5	4	3	5	4	5	5	46	
Assignment of SSW to match that of the allocation of the YPSEW in the green cell	0	1	0	0	1	1	0	0	1	0		

In this illustrative example:

- 7 of 14 YPSWs are allocated to the intervention
- 4 of 10 SSWs are allocated to the intervention
- 11 YP are allocated to Arm 1; 20 to Arm 2; 3 to Arm 3; and 12 to Arm 4.

Table 5 shows the allocation just for the YP in the primary analysis (Arms 1 and 2)

Table 5: Assignment of YP in the primary analysis

	SS W0 01	SS W0 02	SS W0 03	SS W0 04	SS W0 05	SS W0 06	SS W0 07	SS W0 08	SS W0 09	SS W0 10	Total (sorted on total # YP)	Random allocation of YPSW (1=inter vention; 0=contr ol)
YPSW14		1				2			1		7	1
YPSW13	3						1				6	0
YPSW02					1				2		5	1
YPSW12			2					1		2	5	0
YPSW01		1									4	1
YPSW04								1		3	4	0
YPSW06					1				1		3	1
YPSW11				3							3	0
YPSW09							1				2	0
YPSW10					1						2	1
YPSW05			1				1				2	0
YPSW03											1	1
YPSW07	1										1	0
YPSW08											1	1
Total	7	2	6	5	4	3	5	4	5	5	46	
Assignment of SSW to match that of the allocation of the YPSEW in the green cell	0	1	0	0	1	1	0	0	1	0		



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