What Works for Children's Social Care

TRIAL EVALUATION PROTOCOL: STRENGTHENING FAMILIES, PROTECTING CHILDREN - FAMILY VALUED MODEL

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Type of Trial	Stepped Wedge Cluster Randomised Controlled Trial (RCT) & Implementation and Process Evaluation (IPE)
Age or Status of Participants	RCT: Children and young people aged 0 - 17, that have been referred to Children's Social Care (further restrictions apply depending on outcome measure) IPE: • Staff across Children's Services, including Family Group Conference Services. • The children and families who are referred to / supported by Children's Services.
Number of Participating Local Authorities	5
Number of Children and Families	RCT: 78,000
Primary Outcome(s)	RCT: Likelihood of becoming looked after
Secondary Outcome(s)	RCT: Likelihood of returning to statutory services, following a closed CPP/CIN plan; rate of CPPs; days on CPP or CIN plans; likelihood of kinship care for CLA; likelihood of reunification with family for CLA; unauthorised school absence rates.
Contextual Factors	Local authorities had to apply to be part of the Innovation programme. Participation in the programme required an Ofsted rating of "requires improvement to be good" and high rates and/or rising numbers of looked after children over the last three years.

Contents

Background 6

Context 7

Impact Evaluation 7

- Aims 7
- Research questions 7
- Design 8
- Randomisation 10
- Participants 12
- Conditions 12
- Outcome measures 13
- Sample size / MDES calculations 16
- Analysis plan 18
- Data handling 25

Implementation and Process Evaluation 32

- Aims 32
- Research Questions 32
- Design 33
- Data Protection 41
- Timeline 42

Ethics 42

Risks 46

Registration 49

Personnel 49

Appendices 50

Background

Strengthening Families, Protecting Children

This evaluation is part of Strengthening Families, Protecting Children (SFPC), a five-year Department for Education funded programme supporting 18 local authorities to improve work with families and safely reduce the number of children entering care. SFPC will support selected local authorities to adapt and adopt one of three children's social care innovation programme projects in their own area.

The three projects are:

- Leeds Family Valued
- Family Safeguarding Hertfordshire
- North Yorkshire's No Wrong Door

What Work's for Children's Social Care (WWCSC) is conducting a three-part evaluation for each model:

- A pilot evaluation in one 'Trailblazer' local authority (LA). This local authority is the first in this evaluation to implement to model.
- This is followed by an impact evaluation of the model in five subsequent local authorities, with a stepped wedge cluster Randomised Controlled Trial (RCT) design.
- This is accompanied by an Implementation and Process Evaluation (IPE) across these same five local authorities, to understand the delivery during the rollout of the model.

This document sets out the protocol for the RCT and IPE parts of the evaluation of Family Valued.

Family Valued

Family Valued was developed in Leeds with support from the Department for Education's Innovation Programme. Its delivery in Leeds was evaluated by a consortium of academics and evaluators.[1]

The intervention supports a whole-scale shift to restorative practice, changing service-wide ways of working with children and families so that support is done 'with' them, not 'to' them. The programme involves:

- Introductory awareness raising, or deep dive training on restorative practice for all levels of staff in children's services and their partner agencies working with children, families and communities (such as health and education), including training for leadership and management.
- Review and reform of systems and structures in children's social care to ensure they optimise relationships with partners and restorative practice with families.

[1] Mason, P., Ferguson, H., Morris, K., Munton, T. Sen, R. (2017) Leeds Family Valued: Evaluation Report. Department for Education: London

- Offer of Family Group Conferences (FGCs) to families, as an alternative to child protection conferences, to reduce entry to care and support reunification.
- Newly-commissioned restorative services to address gaps in provision and act on the outcomes of FGCs.

A draft logic model setting out the contextual facilitators and barriers, interventions, mechanisms and outcomes for the family valued model is available in Appendix A. The logic model is based on programme theory and not on prior evidence of impact. The logic model will be subject to refinement following completion of the pilot evaluation in Autumn 2020.

Context

The IPE and RCT parts of this evaluation will be undertaken in the local authorities funded by the Department for Education to introduce Family Valued as part of the Strengthening Families, Protecting Children programme, with the exception of the Trailblazer who is participating in the pilot evaluation. These local authorities are due to launch Family Valued at six-month intervals beginning in April 2020. In the order they will be rolled out, these local authorities are Warwickshire, Newcastle, Coventry, Solihull and Sefton.

At the point of rollout to the first local authority, Children's Services in these authorities all had an Ofsted judgement of 'requires improvement to be good'. These authorities have all been selected by the Department for Education to participate in the programme due to having high rates of children looked after compared to their local authority statistical neighbour median over the last 3 years, and/or rising rates of children looked after in each of the last 3 years.

Impact Evaluation

Aims

Family Valued's delivery in Leeds was evaluated by a consortium of academics and evaluators.[2] However, the original evaluation was conducted using a pre-post design and counterfactuals not based on parallel trends in outcomes. The current evaluation uses a stepped wedge cluster RCT design to provide a more robust evaluation of the impacts of Family Valued when scaled to five other local authorities and provide an estimate of the impact on children and families on key outcomes.

Research questions

While the Family Valued model is a whole system reform that aims to affect multiple parties engaged with Children's Services, the key measure of the programme's success used in this evaluation is whether it achieves one of its primary goals - namely reducing the number of children looked after. Our population of interest are children (aged 0 - 17) who have been referred to children's social care. We thus assess the following primary research question of interest:

[2] Mason, P., Ferguson, H., Morris, K., Munton, T. Sen, R. (2017) Leeds Family Valued: Evaluation Report. Department for Education: London

1. What is the impact of Family Valued on the likelihood of children becoming looked after?

Given the multifaceted nature of the model, we also expect to see changes in other important outcomes, such as a reduction in the likelihood of children returning to statutory services. To provide a more thorough assessment of the model's impacts, we address the following secondary research questions:

2. What is the impact of Family Valued on the likelihood of children having their plan closed and then returning to statutory services?
3. What is the impact of Family Valued on the likelihood of children progressing to a CPP?
4. What is the impact of Family Valued on the time spent on CPP or CIN plans?
5. What is the impact of Family Valued on the likelihood of children looked after entering kinship care?
6. What is the impact of Family Valued on the likelihood of children looked after returning home?
7. What is the impact of Family Valued on the unauthorised school absence rates of children referred to children's social care?

Design

The overall study design is a cross-sectional stepped-wedge cluster randomised controlled trial, where the timing of implementation is staggered across local authorities. The point at which local authorities begin implementing the intervention is selected at random, constrained by their level of readiness to implement the model. In this way, all the local authorities in the sample will eventually implement the Family Valued model, but randomising the start date of the implementation of Family Valued will allow service users in the local authorities that have not yet implemented the programme to act as a control group against service in local authorities where Family Valued has already been implemented.

Given that, prior to the evaluation, there was already an existing need to stagger roll-outs over time, this means that nobody will be denied a service that they might otherwise have received. Local authorities implementing on different timescales will also allow us to take time-based effects into account, with every local authority also acting as a control group for itself over time.

RCT Design Table		
Trial type and number of arms		Stepped wedge cluster randomised controlled trial, two arms
Unit of randomisation		Local authority
Stratification variables		Low/High readiness to implement
Primary outcome	variable	Whether or not the child has become looked after

	measure	Coded 1 if the child has become looked after at any point within 18 months of the referral. Coded 0 if the child has not become looked after within this period.
	sample	Children/young people aged 0-17 that have been referred within the trial period.
Secondary outcome 1	variable	Whether or not the child has returned to statutory services following a CPP or CIN plan
	measure	Coded 1 if the child finishes a CPP or CIN plan and then returns to statutory services (i.e. begins a new CPP, CIN plan or becomes looked after) within 36 months of the initial referral start date, coded 0 if they have not re-entered statutory services within this time period.
	sample	Children/young people aged 0-17 that have been referred within the trial period
Secondary outcome 2	variable	Whether or not the child has started a CPP
	measure	Coded 1 if the child begins a CPP within 12 months of being referred, coded 0 if they have not entered a CPP
	sample	Children/young people aged 0-17 that have been referred within the trial period.
Secondary outcome 3	variable	Days on CPPs or CIN Plans
	measure	Discrete variable equal to the number of days that the child has been on CPPs or CIN plans over a period of 36 months from initial referral. Days spent on multiple referrals are counted if applicable.
	sample	Children/young people aged 0-17 that have been referred within the trial period.
Secondary outcome 4	variable	Whether or not the child has been in kinship care
	measure	Coded 1 if the child has spent (part of) its period of care in kinship care or has been taken into kinship care at the end of the episode, coded 0 if the episode of care includes no time spent in kinship care and, if the episode has ceased, it was not due to a Special Guardianship Order. Measured 18 months after the start of the period of care.
	sample	Children/young people aged 0-17 that started a referral within the trial period and became looked after within 18 months of the referral start date.
Secondary outcome 5	variable	Whether or not a child in care has been reunited with its family
	measure	Coded 1 if the looked after child returned home to live with their parents or other person with previous parental responsibility within 18 months of beginning the period of care. Coded 0 if the child did not return to live with their parents or other person with parental responsibility within 18 months.
	sample	Children/young people aged 0-17 that were referred within the trial period and who became looked after within 18 months of the referral start date.
Secondary outcome 6	variable	Unauthorised school absence rate

	measure	Continuous variable equal to the percentage of sessions missed due to unauthorised absence out of all the school sessions the child was expected to attend over three terms following the start of the referral.
	sample	Children/young people aged 0-17 that have been referred within the trial period.

We will use administrative, secondary data for the analysis. The administrative data will be provided by each local authority in the evaluation. Local authorities that are participating in the evaluation have committed to providing data. For details please see the Data Gathering section below.

Randomisation

The level of randomisation is at the local authority level. Due to the stepped-wedge evaluation design, we randomise the order in which local authorities implement the programme, in six month intervals, rather than which local authority implements the model. The randomisation will be stratified by the level of readiness of participating local authorities. Each local authority will be classified as either 'high readiness', for those that are in a position to implement the model sooner, and second for the 'low readiness' authorities, for those that will need longer to implement the model. Two local authorities were identified as 'high readiness', and three as 'low readiness'.

The two local authorities classified as 'high readiness' will be randomly assigned to implement the model either first or second. Those classified as 'low readiness' will also have the order randomised in which they will implement the model, following the high readiness local authorities (so they will be the third, fourth, and fifth local authorities to implement the model).

The division of local authorities into more or less ready tranches is meant to avoid implementation failure caused by choosing local authorities to receive the intervention that are not yet ready. The assessment of readiness was conducted by the developing local authority Leeds in collaboration with the Department for Education (DfE).

Our strata are thus very small strata (2-3), which normally would be avoided but was necessary in order to be able to implement the evaluation. However it is a notable constraint on our randomisation, that will affect the robustness of our results. This will be reflected in the evidence strength rating awarded to the final study.

To avoid potential contamination, local authorities whose implementation start date has not yet passed have to commit to business-as-usual practices to enable a treatment and control group comparison in each time period. However, they will be given permission to begin preparation to implement, so long as it would not influence the current practice in the local authority.

For the purposes of our evaluation, we will only consider children who have been in touch with children's social care between six-months before the first local authority's implementation date, and six-months after the last local authority's implementation date.

We define the implementation date as the date the Family Valued model is considered ‘Operationally Live’ in the local authority. The Operationally Live date has been set in advance by the Department for Education. When analysing the data, we may change the date we consider the intervention to have gone Operationally Live, if it becomes apparent that there have been significant changes in terms of the timings of the models core activities. This will only be done with the agreement with the Department for Education and consultation with the model developer. Any such changes will be detailed in the report. Specifically, the core activities are as detailed below.

Before the Operationally Live date:

- Awareness raising training (to LA staff and partners) is completed

From the Operationally Live date:

- In-depth restorative training begins (to LA staff and champions from partners).
- The LA implements an expanded FGC service and appoints staff in newly commissioned or restructured services (although the scene setting for this may have begun before Operationally Live).

It is worth noting that we count Operationally Live as a much earlier date in the implementation journey for Family Valued than we do for Family Safeguarding or No Wrong Door. This is in part because there’s a 6 month roll-out period, with no obvious cut-off point for when we would count things as treated. This should be considered when interpreting the findings.

The diagram below illustrates the randomisation and intended timings of the implementation of Family Valued across the local authorities. The trial period, as indicated in the diagram, takes place from 6 months prior to the first local authority implements the model (or goes Operationally Live), and continues until 6 months after the final local authority implements the model.

Participants

The children that we include in our sample are those who meet the following criteria.

- They are referred to the local authorities' children's services and, following assessment, their case remains open. This is because we want to evaluate only those children and young people with some ongoing contact with children's social care services that would be expected to be influenced by the treatment.
- Their original referral date falls within the trial period as defined above.
- Are aged 0 -17 at the point of referral.

This will provide our analytical sample, which will then be further restricted or extended for different outcomes, as described in the RCT Design Table above and the Outcome Measures section below.

Conditions

Children and young people that form part of the samples described above will be designated as part of the treatment and control groups according to whether Family Valued was implemented in their local authority at the time of their referral start date. We consider the start date of the referral that determined their affiliation to the sample population as the relevant point at which to determine treatment and control group allocation. Compared to other possible dates at a later stage in the referral, this constitutes a conservative approach. Some children in the control group might have been in contact with Family Valued teams at a later stage of their referral which can bias our estimate downwards. We revisit this approach in our sensitivity analysis.

We only consider referrals that started during or after our baseline period and do not allow children to enter our sample twice i.e. any additional referrals after the referral defining our baseline population will not be considered.

Condition	Description
Control	Children whose first referral in the trial period took place when the local authority was running their business as usual model.
Treatment	Children whose first referral in the trial period took place when the local authority was running the Family Valued model.

Outcome measures

For the trial we will evaluate one primary outcome measure and six secondary outcome measures. Individual level data will be collected directly from five of the local authorities participating in the Family Valued programme, as detailed above. Below we give an explanation and rationale of the outcomes outlined in the RCT Design Table above. In the instance of any unintentional inconsistencies, the above table definitions should take precedent in the analysis.

Primary outcome measure

Whether or not the child has become looked after

To answer research question 1, we will analyse whether children and young people (aged 0 - 17 who are referred to Children's Social Care within the trial period) are more or less likely to become looked after where Family Valued had been implemented, compared to when it had not been, within 18 months of starting the referral. The outcome measure is a binary variable, indicating whether or not a child that is in our analytical sample (defined above) has become looked after at any point within 18 months of their first referral in the trial period.[3]

Secondary outcome measures

In addition to the primary outcome, we will also seek to evaluate six secondary outcome measures.

Whether or not the child has returned to statutory services following a CPP or CIN plan

To answer research question 2, we use a binary outcome measure, indicating whether or not a child has returned to statutory services within 36 month of the initial referral start date, following the end of a CPP or CIN plan prior to this after the initial referral.[4] Our sample will include any child/young person aged 0-17 that has had a referral within the trial period.
We choose return to statutory services (rather than considering CPPs and CIN plans only) as our outcome measure to avoid classifying children whose needs have escalated to e.g. going

[3] Note that the episode of care does not have to result directly from the initial referral, e.g. a child who had a case that was previously designated as open closed but then returns to children's services and becomes looked after within 24 months of the initial referral date will be coded as 1.
[4] We use the referral start date as the starting point for the 24 months time frame (as opposed to imposing a time frame starting from the end of the first CPP/CIN plan) to reduce potential endogeneity through selection into the sample. Endogeneity can occur if social workers assess risk differently through Family Valued. This might lead to a change in the length of CP/CIN plans, which in turn can result in different children in treatment and control groups terminating a first CPP/CIN plan within the trial period.

into foster care as “non-returners”. We acknowledge that we do count as a non returner children that stay on a CIN plan or CPP, or escalate from the former to the latter. Thus care must be taken interpreting this outcome measure.

This outcome will seek to act as a measure for the quality of assessment and direct work with children and their families. Since an important component of the model is Family Group Conferencing (FGC), which can serve as an alternative to initial child protection conferences (ICPC), we consider CPP and CIN plans together. This ensures that the outcome measure reflects a reduced need for children’s social services rather than a shift from one type of service to another, in case the model leads to a change in thresholds required for CPPs.

Whether or not the child has started a CPP

To answer research question 3, we use a binary outcome measure indicating whether or not a child has begun a CPP within 12 months of being referred to children’s social care. Our sample will be the analytical sample, including any child/young person aged 0-17 that has had a referral within the trial period.

We include this outcome measure to capture the effects of Family Valued particularly around the early help and restorative practice elements of the model. These elements and the work together with partners should reduce the need for statutory services in the local authorities that implement Family Valued. While outcome measure 2 evaluates the effect of Family Valued on children and young people already in touch with the social care system, research question three investigates the effect on the rate of CPPs directly. Due to the potential for Family Valued to introduce a review of and potential change in assessment thresholds, a reduction in CPPs may not necessarily reflect a reduction in risk within families. Therefore, this measure will have to be evaluated considering the other results to shed light on the mechanisms behind the found effects.

Days on CPPs or CIN Plans

To answer research question 4, we use a discrete variable measuring the number of days that the individual has spent on CPPs or CIN plans over a period of 36 months from the start of the initial referral. Larger values will be censored at 36 months.

If, under the Family Valued model, families make changes and build confidence to overcome challenges more effectively, this should reduce the length of statutory interventions for children. We consider CPP and CIN plan length jointly to take into account changes in thresholds due to Family Group Conferencing as explained above. This also aims to measure on a broader scale whether children are subject to statutory interventions (not including care) for shorter periods of time.

Whether or not the child has been in kinship care

To answer research question 5, we use a binary outcome measure, indicating whether or not a child looked after has been under kinship care at any point of the episode or has been taken into kinship care at the end of the episode[5], as measured 18 months after the start of the period of being looked after. Our sample will include any child/young person aged 0-17

[5] typically under a Special Guardianship Order (SGO).

who has started a referral within the trial period and subsequently became looked after within 18 months of the referral start date.

This outcome evaluates whether Family Valued increases the likelihood of children to be cared for within their kinship network. Specifically, it is hypothesised that Family Group Conferencing may influence this outcome.

Whether or not a child in care has been reunited with its family

To answer research question 6, the outcome measure is a binary variable of whether or not a child looked after has returned to live with someone who previously had parental responsibility at the end of its episode of care, as measured 18 months after the start of the period of care. Our sample will be restricted to children/young people aged 0-17 who have been referred within the trial period and subsequently became looked after within 18 months of the referral start date. We will further exclude any young people from our sample who turn 18 before the episode of care ends. If the data is available, we will only code cases as returned if the child/young person leaves care to live with parents/family as part of the care plan.

One intended effect of Family Group Conferencing is that families feel greater ownership of plans and are thus more likely to make changes in their behaviour. If families succeed in making long-term changes that reduce the risk in families, this should increase reunification of children looked after with their families.

Unauthorised school absence rate

To answer research question 7, the outcome measure is a continuous variable measuring the percentage of sessions missed by a child within our analytical sample due to unauthorised absence. We will measure the unauthorised school absence rate of the three closest school terms beginning after the start of the period in which the child entered our sample.

Unauthorised school absence rates are a valuable addition to the children's social care outcome measures detailed above as they directly relate to children's opportunities and have important implications for children's long term outcomes. Since there exists no direct link in the logic model between the model and unauthorised school absence rates, this outcome is of an exploratory nature to see whether we can capture part of the potential wider benefits of Family Valued.

Care should be taken in the interpretation of the results of our analysis. Each result (pertaining to a specific outcome measure) will help create a picture of the changes that are taking place because of the intervention. However in isolation we should be wary of concluding strongly that one direction is good or bad. This is especially true in terms of our measures relating to research questions 2 and 3. For example, a reduction in the length of statutory interventions could be positive - indicating that children's social care interventions address the families needs more rapidly. However, it could also be negative - and indicative instead of cases being closed prematurely, with families having unmet needs which could lead them to return to statutory services shortly after closing the case. Thus we will evaluate each analysis in the context of the others that we conduct. We will also interpret the results

alongside the findings of the associated implementation and process evaluation, which may shed further light on the factors driving these outcome changes. We will also reflect any remaining ambiguity accordingly in our reports.

Sample size / MDES calculations

NB: These power calculations were conducted with the 'steppedwedge' package in Stata. We will conduct simulations to ensure the accuracy of these and update the trial protocol before any outcome data is collected. This could lead to changes in the minimum detectable effect size (MDES).

		Proportion of children who become CLA within 18 months of referral start date
MDES		0.007
Baseline measures		0.04
Intracluster correlation (ICC)	Local authority	0.01566
Alpha		0.05
Power		0.8
One-sided or two-sided?		Two-sided
Level of intervention clustering		Local authority
Number of clusters		5
Average cluster size (children per local authority across all time periods)		15,600
Average cluster cell size (children per local authority per time period)		2,600
Sample Size (children)	Total	78,000

We are powered to detect an effect size of 0.07, or a 0.7 percentage point decrease or increase in the proportion of children who become looked after within 18 months of referral start date.

Sample size and cluster size

The sample size was derived from the estimated baseline population for our primary outcome, which is the number of children who have been referred in a six month period.[6] We take the average across local authorities that form part of our sample to calculate the average cluster size. The sample size is derived from the average cluster size times the number of local authorities in the trial and the six periods of the stepped-wedge implementation.

[6] Using publicly available data, we used the number of referrals within a year. Figures were divided by 2 to derive an estimate of the cluster size for our 6-month intervals.

Baseline rates

Baseline rates were calculated by averaging the share of children who became looked after in a given year out of the number of referrals in that year across local authorities who are part of our sample. Data was sourced from the Local Authority Interactive Tool (LAIT).[7]

Intra-cluster correlation

We use the latest available historical data to estimate the intra-cluster correlation (ICC). Using a proxy for the sample size and baseline rates as above (taking into account the different sample sizes and baseline rates in each individual local authority), we can calculate the ICC using the loneway command in Stata. Since our outcome is binary, we do not need any additional individual-level information to calculate the ICC.

Analysis plan

Primary Analysis

We will assess the impact of Family Valued Model on the primary outcomes of interest Y_{iat} in the following GLMM regression framework:

$$logit(Y_{iat}) = \beta_0 + \alpha_{0a} + \beta_1 FV_{iat} + \sum_{i=1}^{5} \beta_{i+1} I(t=i) + \gamma X_{iat} + \rho Z_{at} + \varepsilon_{iat}$$

Where:

- Y_{iat} is a binary indicator that equals 1 if the child entered care within 24 months of their first referral in the trial period, and 0 otherwise.[8]
- α_{0a} is a (normally distributed) random intercept at the level of the cluster. This random effect estimates the stochastic variation of individual clusters around the conditional mean of the clusters.
- β_{i+1} are a series of indicator variables adjusting for time trends by introducing dummy variables for each time period after the baseline period t = 0.[9]
- FV_{iat} is a binary indicator that is equal to 1 if the child had its first referral during the trial period after the local authority implemented Family Valued (and 0 if before).[10]
- X_{iat} is a vector of individual and household level characteristics that may also influence the outcome, such as age of the child, gender, and household SES.
- Z_{at} is a vector of time-varying local authority characteristics, such as the number of children per local authority or the turnover rate of staff.
- ε_{iat} are the errors at time t for individual i.

The GLMM is an extension to GLM for analysing correlated data. The unit of analysis is at the individual level to optimise the power to detect an effect within the constraints of the project. We use a logistic regression within the GLMM framework to account for the binary nature of our outcome variable and because the baseline rate is low.

[7] https://www.gov.uk/government/publications/local-authority-interactive-tool-lait
[8] population as described above
[9] We consider the referral date to be the relevant date according to which the relevant time dummy is determined.
[10] Children can only occur once in our evaluation, i.e. that we consider the first referral

We will judge the statistical significance of the treatment effects applying a significance level of 5%. Due to the small number of clusters, we cannot cluster or bootstrap standard errors via any conventional method. However, we will consider whether or not applying a wild bootstrap with a correction for the small number of clusters is appropriate in this instance. Our sensitivity analysis will consider different evaluation approaches that are discussed in detail below.

There is a risk of non-compliance, e.g. local authorities may implement some or all aspects ahead of their agreed Operationally Live date, or fail to implement some elements. As stated above, we will adjust the date we consider Family Valued has been implemented, if it becomes apparent that there have been significant changes in terms of the timings of the models core activities. This will only be done with the agreement of the Department for Education and in consultation with the model developer. However, outside of this we will take an intention-to-treat approach, and will not, in our primary analysis consider other elements of non-compliance. However if the IPE shows that there have been substantial instances of non-compliance, we will consider running complier average causal effect (CACE) analysis to account for this. This would however be secondary analysis.

Covariates

In order to increase the precision of our estimates, we include the following individual level and local authority covariates (where they are available), gathered at the point of referral.

Vector of individual level covariates of the child or young person

- Gender (included as a binary indicators for male, female, or other/undetermined)
- Ethnicity[11]
- Age in months
- Academic year
- Disabled status[12] (included as a binary indicator: 0=No, 1= Yes)
- Eligibility for free school meals (included as a binary indicator: 0=No, 1=Yes, if pupil has ever been recorded as eligible for free school meals on Census day in any Spring Census up to the pupil's current year), Pupil Premium eligibility (for Reception, Year 1 and Year 2)[13]
- Is child an Unaccompanied Asylum Seeker[14] (included as a binary indicators, 0=No, 1= Yes)
- Number of previous child protection plans (where possible to collect)
- The main need for which child started to receive services for this referral (if applicable), as defined in the CIN census (included as a categorical variable: 0 = Not stated, 1 = Abuse or neglect, 2 = Child's disability/illness, 3 = Parental Disability/illness, 4 = Family in acute stress, 5 = Family dysfunction, 6 = Socially

[11] In the categories defined in the DfE's CIN census.

[12] Hughes K, Bellis MA, Jones L, Wood S, Bates G, Eckley L, McCoy E, Mikton C, Shakespeare T, Officer A. Prevalence and risk of violence against adults with disabilities: a systematic review and meta-analysis of observational studies. *Lancet* 2012.

[13] We use Pupil Premium Eligibility for the first three years as every child is eligible for free school meals during this period.

[14] UN High Commissioner for Refugees (UNHCR). (2013, March). *UNHCR's Engagement with Displaced Youth*. https://www.refworld.org/docid/5142d52d2.html, p28.

unacceptable, 7 = Low income, 8 = Absent parenting, 9 = Cases other than Children in Need).

In addition, we would have wanted to take into account families (e.g. through adding family fixed effects), however we are reasonably confident data will not be available, so we have refrained from including them.

Vector of time-varying local authority level covariates[15]

- Number of children in the local authority
- Numbers of assessments by Children's Social Care
- Average number of cases per children and family social worker (based on full-time equivalent (FTE) counts[16]) as defined in the Local Authority Interactive Tool (LAIT) (integer)
- Proportion of children / young people not seen in accordance with the timescales specified in the plan (continuous variable)
- Proportion of children / young people eligible for Free School Meals (continuous variable)
- Proportion of children / young people white British (continuous variable)
- Turnover rate of staff (percentage rate)
- Presence of other Innovation Programmes - if the authority used programmes additional to Family Valued that had similar aims or that induced whole system change (e.g. Signs of Safety) (coded as binary variables)

Handling missing data

In cases of missing data, we will consider the possible reasons for its missingness and undertake statistical analyses to determine whether there are any patterns relating to other recorded covariates or to the intervention variable. We will drop observations with missing outcome variables, and will drop covariates that are missing at a rate greater than 30%. For covariates with lower levels of missingness, we will conduct null imputation, by which we mean, for categorical variables, creating an additional dummy variable for the covariate indicating whether it is missing or not; for numeric variables, we will do the same, but also, in the numeric covariate, code it as another arbitrary number - e.g. 0 (that will be the same for all such missing values).

Secondary Analysis

For all binary secondary outcomes, namely return to statutory services, kinship care, and family reunification as defined in the RCT Design Table above, we will use the same regression specification as for the primary outcome.

For the secondary outcomes number of days on CPPs/CIN plans and unauthorised school absence rates, we will use a linear probability model. Due to the small number of clusters, we cannot cluster or bootstrap standard errors via any conventional method. However, as above, we will consider whether or not applying a wild bootstrap with a correction for the

[15] We will request monthly data on these covariates from the local authorities. In the case that obtaining this more granular data proves impossible, we will use yearly data as a proxy. We will use the most recently available measurement that took place prior to the referral date.
[16] as defined in the Local Authority Interactive Tool (LAIT) (integer)

small number of clusters is appropriate in this instance. In the case of unauthorised school absence rates where we will measure children/young people repeatedly at the end of three terms, we include individual random effects in the regression specification as well as indicator variables for the school term and a variable controlling for the time since the relevant referral. Other specifications remain as specified in the primary analysis.

Due to the high number of secondary outcomes, we will use Hochberg multiple comparison adjustments for the secondary outcomes to reduce the risk of finding significant results by chance.

Sensitivity Analysis

Definition of treatment and control group

We adopt a conservative approach in our primary analysis and define any child as part of the control group whose local authority had not implemented Family Valued at the start date of the first referral within the trial period. This will most likely underestimate the treatment effect, since children in the control group might have been in contact with Family Valued at a later stage of the referral.
To analyse the magnitude of the treatment effect further, we run additional regressions using different treatment and control group definitions. We will look at different treatment definitions including:

- Children whose spent at least half their time on any open referrals in the trial period when the local authority had implemented Family Safeguarding, i.e. if a child had 64 days of open referrals during the trial period, and had at least 32 of those days after the local authority had implemented Family Safeguarding, they would be coded 1, otherwise coded 0.
- Children who spent at least 4 weeks across any open referrals during the trial period under Family Safeguarding coded as 1, otherwise coded 0.

In a second step, we will re-run the analysis on care outcomes, namely outcome measures 5 and 6 measuring kinship care and reunification with family, classifying children as treatment and control group based on whether Family Valued was implemented in their local authority by the time they entered care. Family Valued can affect a child's involvement with children's services at various stages starting from a first referral up to episodes of care. Elements such as Family Group Conferencing can also be used both when a child is on a CIN plan or when a child has already become looked after. To shed some light on which stages of the model influence children looked after most, we will redefine the treatment and control conditions to see whether the found effect of Family Valued on care outcomes remains similar when defining the treatment group differently.

Non-parametric permutation test

To check the robustness of our results, we will seek to conduct a non-parametric permutation test for testing the null hypothesis of no treatment effect. The permutation test can provide an alternative to the GLMM models used in our primary analysis, as it remains valid in small samples and in the presence of correlation across different clusters regardless of the underlying data distribution. It is also robust to mis-specification of the models used to

construct the test statistics.[17] The permutation test generally works well with a small number of clusters but in the current research design of five clusters only, the evidence is more scarce. Hence, we include this test only as a sensitivity analysis to support the main analysis.

Differential time effects

We do not consider time effects such as embedding periods in our primary analysis. It may be that Family Valued needs some time to be fully embedded and functional. In that case the treatment will show differential time effects. In this sensitivity analysis, we thus include differential treatment effects depending on the time passed since Family Valued has been implemented in the local authority. The regression specification will be:

$$logit(Y_{iat}) = \beta_0 + \alpha_{0a} + \beta_1 FV_{iat} + \sum_{m=0}^{M} (FV_{iat} \cdot T_{t+m})\,\delta_m + \sum_{i=1}^{5} \beta_{i+1} I(t=i) + \gamma X_{iat} + \rho Z_{at} + \varepsilon_{iat}$$

Where T_{t+m} is a binary indicator that equals one if the observation is from a local authority that has been implementing Family Valued for m periods, and otherwise 0. The coefficients on the interaction effect will shed light on whether authorities experience increasing treatment effects the longer they run Family Valued.

We recognise that the estimation of differential time effects will likely be underpowered due to splitting the treatment effect into separate, time-dependent effects. Nevertheless, we consider this analysis as potentially providing a richer picture of the effects of Family Valued.

Regression specifications

In the event that the data distribution suggests a different model would be more suitable, we will run and report these models in addition. Specifically, this will include (but not be limited to) considering hurdle models when evaluating the impact on days on CPPs/CIN plans. Since we expect the number of censored data points in the time spent on CPP/CIN plans outcome measure to be reasonably small, we use a linear probability model in our main regression specification for research question 3. If the data turns out to be more heavily censored, we will consider employing a tobit model instead. Similarly, we will use a logit model to check the robustness of our regression on unauthorised school absence rates.

Definition of the implementation date

For our main analysis, we define the implementation date as the Operationally Live date, where awareness raising restorative practice training has been completed (see more detailed explanation above). Choosing this date as the date from which on we count children that have been referred as “treated” can potentially lead to the treatment effects differing between children that were in touch with children’s services shortly after the implementation compared to those that were involved with statutory services much later. This is because some components of Family Valued such as the in-depth restorative practice training and appointment of staff to new or restructured/expanded services might not have been implemented fully at the chosen implementation date. We chose this Operationally Live definition taking into account the large scope of Family Valued and the Department for Education’s definition of the Operationally Live date.

[17] Wang, R. & De Gruttola, V. (2017): The use of permutation tests for the analysis of parallel and stepped-wedge cluster-randomized trials

To explore whether this has an impact on our coefficients of interest, we will re-run our analysis defining the implementation date as the point after which both awareness training and deep dive training have been completed (and most new staff have been appointed). We will compare the results with the coefficients from our main analysis to check the robustness of our estimates.

Exploratory Analysis

High and low readiness of local authorities

Since the randomisation of the implementation date was stratified by the readiness of local authorities to implement Family Valued, we explore a potential difference in effects of the implementation of Family Valued between high and low readiness authorities.

$$logit(Y_{iat}) = \beta_0 + \alpha_{0a} + \beta_1 FV_{iat} + \beta_2 HR_a + \beta_3 HR_a * FV_{iat} + \sum_{i=1}^{5} \beta_{i+3} I(t = i) + \gamma X_{iat} + \rho Z_{at} + \varepsilon_{iat}$$

Where:

- HR_a is an indicator that is equal to 1 if the authority belongs to the high readiness group that first implements the programme, and 0 if they belong to the 'less ready' group.
- $HR_a * FV_{iat}$ is an interaction term that will allow for differential effects of the model on the local authorities in the high readiness tranche versus the low readiness tranche. β_3 will be zero if the intervention affects the likelihood of a child entering care in both groups of local authorities equally.

Cost benefit analysis

Our main analysis focuses on potential effects of Family Valued on children's social care outcomes. Given the opportunity for the model to not only improve outcomes but also realise significant cost savings for local authorities, we will investigate the implicit cost savings our estimates suggest.

The main focus of this analysis will be on any savings or costs realised through a change in the number of children that become looked after. This will be informed by the coefficient of our primary analysis and average cost estimates per looked after child. This will only estimate savings or costs for the direct effects we are measuring, i.e. the average number of cases where children that had an open case did or did not go on to become looked after through the model. Consequently, this analysis will only cover a part of the potential reduction in the rate of looked after children through Family Valued.

Data handling

Data gathering

Data will be collected directly from local authorities. We limit ourselves to asking for administrative data that has to be recorded for statutory returns so that our analysis will not need further data collection.

Data	Collection Point	Source
Individual-level administrative data on the sample populations (including treatment condition, and individual covariates)	In 12 month intervals, starting 12 months after the start of the trial period until the end of the trial period (six months after the last implementation date)	Directly from local authorities
Individual-level administrative data on outcome measures	In 18 month intervals, starting at the end of the trial period until 36 months after the trial period (42 months after the last implementation date)	Directly from local authorities
Local-authority level administrative data (summary statistics of the previous six months)[18]	In six month intervals), starting at the end of the baseline period (the date when the first local authority goes Operationally Live) until the end of the trial period (six months after the last implementation date)	Directly from local authorities

Data protection

The underlying data used to conduct this analysis consists of administrative data from local authorities funded by the Department for Education to introduce Family Valued as part of the Strengthening Families, Protecting Children programme, with the exception of the trailblazer. The data about individuals requested from each local authority will be pseudonymised. We will not request any 'instant identifiers' (that would allow us to point to an individual in the dataset) or 'meaningful identifiers' (which would allow identifying someone through linking the data to another dataset, beyond the local authorities administrative datasets). We will require 'meaningless identifiers' (data variables used within the local authorities dataset or datasets, but have no meaning beyond these datasets' boundaries) to track individuals over time.

[18] If the data is not available in monthly intervals, we will try and get as frequent intervals as possible, as a last resort, we will use yearly data that is publicly available online.

This section is structured according to the guidance given by the Information Commissioner's Office, which "covers the General Data Protection Regulation (GDPR) as it applies in the UK, tailored by the Data Protection Act 2018".[19]

Principles of the GDPR

Principle (a): Lawfulness, fairness and transparency

1. Lawfulness:

WWCSC will be a data controller in common with each local authority for each of their respective datasets. WWCSC decided to process the data and decided the purpose of its processing, what data should be collected and which individuals to collect data about. The data is collected by the local authorities for their own purposes. They determined that they would share the data with WWCSC for processing. The legal basis for WWCSC processing the data is legitimate interest.

Legitimate interest is a three part test:

1) Purpose test: are you pursuing a legitimate interest?

We are a charity, whose purpose is to improve the evidence base in children's social care. We consider the processing of the data to be in our legitimate interests because it will enable us to produce research in this area, which will benefit local authorities, in particular senior leaders who make decisions about practice models, as well as the Department for Education in future funding decisions.

2) Necessity test: is the processing necessary for that purpose?

The processing is necessary for the purpose because processing individual-level data allows us to conduct analysis which is better powered to detect the impact of Family Valued, and which allows us to better control for the circumstances of the individual which may affect the outcome. Both of these factors mean that we are more likely to be able to provide meaningful research which can be used to inform practice, with downstream effects for children involved in statutory social care.

3) Balancing test: do the individual's interests override the legitimate interest

We will publish a privacy notice on our website to give general notice of this processing, prior to it taking place. While the data is quite sensitive and on a population which includes vulnerable children, the data will be pseudonymised, with us being very unlikely to be able to identify any child or family. The data will be stored securely. We believe this processing falls within generally socially acceptable uses of this kind of data - it is scientific research in the public interest by a charity and for the benefit of a vulnerable group. Alongside the privacy notice, we will include a form which individuals can fill in to uphold their individual data rights. We therefore believe that the individuals' interests do not override our legitimate interest in this processing.

The legal basis for processing special category data is that it is necessary for archiving, scientific, historical research or statistical purposes (point (e) of section 10 of the DPA which

[19] Information Commissioner's Office, Guide to the General Data Protection Regulation (GDPR). https://ico.org.uk/for-organisations/guide-to-data-protection/guide-to-the-general-data-protection-regulation-gdpr/

refers to (j) (archiving, research and statistics) of Article 9(2) of the GDPR). The project meets condition (4) in Part 1 of Schedule 1:
(a) is necessary for archiving purposes, scientific or historical research purposes or statistical purposes,

This processing constitutes scientific research as it will be used to create evidence on pre-defined, specific hypotheses around what works to improve outcomes for children who have undergone statutory intervention, in order to increase the knowledge base in this area. The special category data we are using is data concerning ethnic group and health, specifically disability status. Not being able to assign ethnic group or disability status to our data would limit the scientific value of this research because they are likely moderators of social care outcomes. The likelihood of children to enter care also varies significantly by ethnic group and is thus important to control for when trying to gauge the impact of Family Valued on children's services.

(b) is carried out in accordance with Article 89(1) of the GDPR (as supplemented by section 19)

Organisational and Technical Arrangements
"Those safeguards shall ensure that technical and organisational measures are in place in particular in order to ensure respect for the principle of data minimisation. Those measures may include pseudonymisation provided that those purposes can be fulfilled in that manner." The data will be pseudonymised i.e. it can no longer be attributed to a specific data subject without the use of additional information. We are not requesting any 'instant identifiers' (e.g. name or address) or 'meaningful identifiers' (identifiers that allow linking to other datasets, beyond the local authorities').
Safeguards (DPA 2018 Section 19)
In the UK, the requirements of Article 89(1) GDPR will not be met unless the provisions of Section 19 DPA 2018 are also complied with. We have no reason to believe that the research will cause damage or distress (and certainly not substantial damage or distress) to the children or young people - the analysis requires no extra involvement of the children or young people. The data has already been collected in the course of day-to-day work with the child/young person and their family. The processing and presentation of evidence is unlikely to have distressing effects because we protect against identification of the individual and also against statistical disclosure (following the ONS standard rules outlined in the Approved Researcher training). The research is not being carried out for the purposes of measures or decisions with respect to a particular data subject but looks at the effect of Family Valued on the cohort as a whole.

(c) is in the public interest.
The work is intended to support work towards high standards of quality of social work practice which affects a substantial section of the public.

2. Fairness:

ICO's guidance says fairness means "you should only handle personal data in ways that people would reasonably expect and not use it in ways that have unjustified adverse effects

on them"[20]. This data is being used for statistical research to understand whether a practice model is working and contribute towards improvements in public services. We believe that "the reasonable person" would find the use of data in this way acceptable.

3. Transparency:

This will be covered below in the section on the right to be informed. We will ensure that privacy notices are written in clear and plain language. We will also ensure that notices have a Flesch-Kincaid grade level of 7 to ensure that either older children who are able to object by themselves can do so and that the notices are accessible to all parents.

Principle b): Purpose Limitation

This data will only be used to increase the evidence base about how Family Valued affects the outcomes of children / young people and their families involved in social care. They will not be used for any other purpose, other than usual statistical checks to ensure the accuracy of the data.

Principle c): Data Minimisation

We have only requested data that is adequate, relevant and limited to what is necessary to fulfil the purpose of this project i.e. to build the evidence base on Family Valued. Broadly speaking, we can classify the data requested into two groups, broadly individual-level and local authority level variables. The individual-level variables are sourced from local authority administrative datasets, and local authority level variables are sourced from public data e.g. the Local Authority Interactive Tool (LAIT).

Individual-level variables

- Outcome measures which are necessary to assess the impact of Family Valued on certain domains of interest;
- Other individual-level variables which we expect to influence the outcomes. Not being able to include these variables would limit the scientific value of this research because they are likely moderators of social care outcomes.

Local authority level variables

- Local authority level variables which we expect to influence the outcomes.

Principle d): Accuracy

The local authorities spend considerable time cleaning the administrative data so that it is suitable for data returns to the Department, and we are requesting only data that is in such returns (for example, the LAIT[21], CIN Census[22]). We will conduct usual checks on all variables used to validate data quality. Please see the "Handling missing data" for our approach to missing data in the administrative datasets.

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[20] Information Commissioner's Office. Principle (a): Lawfulness, fairness and transparency. https://ico.org.uk/for-organisations/guide-to-data-protection/guide-to-the-general-data-protection-regulation-gdpr/principles/lawfulness-fairness-and-transparency/

[21] HM Government. Local authority interactive tool (LAIT), https://www.gov.uk/government/publications/local-authority-interactive-tool-lait

[22] HM Government. *Statistics: children in need and child protection.* https://www.gov.uk/government/collections/statistics-children-in-need

Principle e): Storage limitation

WWCSC will transfer its data to an externally managed data archive (details are being finalised and this protocol will be updated accordingly) and keep this data indefinitely. This is permitted under GDPR, provided it is for: archiving purposes in the public interest; scientific or historical research purposes; or statistical purposes.[23] WWCSC will delete any copies of the data it holds outside of the archive once the data has been successfully transfered to the archive, and it has been two years after publication of the final analysis, to allow for follow up robustness checks.

Principle f): Integrity and confidentiality (Security)

See "Data security arrangements" below.

Principle g): Accountability principle

The Executive Director of WWCSC and Principal Investigator for this research (Dr. Michael Sanders) will be ultimately responsible for the conduct of the research. Other details are below in the accountability and governance section.

Individuals' rights under the GDPR

The right to be informed

WWCSC will publish a privacy notice on its website detailing how the processing will be done. As this data is indirectly collected and for "scientific or historical research purposes" as well as "statistical purposes", WWCSC is relying on an exemption to the requirement to individually inform participants as it would "prevent or seriously impair the achievement of the purposes for processing".

This is the case because:

- It would require WWCSC, a not-for-profit organisation, to expend considerable resources to mail a large number of individuals thus leaving less resources to undertake the processing;
- It would require re-identifying the individuals via their addresses, which is data the WWCSC does not have access to.

The right to access, rectification, erasure, restriction of processing and to object

Individuals have the right to access their individual data and supplementary information. The right of access allows individuals to be aware of and verify the lawfulness of the processing. Individuals are entitled to obtain:

- confirmation that their data is being processed;
- access to their individual data; and
- other supplementary information.

If an individual wishes to access this information, we cannot comply directly because we do not have identifiers in the dataset. We would point the individual towards the privacy notice and trial protocol to indicate the type of information that we hold on them for the purpose of this analysis. We would then collect the information necessary for their local authority to be able to identify them via the online form, and refer the case to the local authority where the

[23] For further details see the ICO's guidance on storage limitation. https://ico.org.uk/for-organisations/guide-to-data-protection/guide-to-the-general-data-protection-regulation-gdpr/principles/storage-limitation/

request can be handled using the local authority's own subject access request procedures. For individuals invoking their rights to rectification, erasure, restriction of processing and to object, we would then require the local authority to inform us of which rows of data to rectify or delete.

The right to data portability

The right to data portability allows individuals to obtain and reuse their individual data for their own purposes across different services. It allows them to move, copy or transfer individual data easily from one IT environment to another in a safe and secure way, without hindrance to usability. This is not particularly relevant in the context of statistical analysis as the value of processing the data is to the public and comes from the aggregation of the data, rather than from the processing of the individual's data, and so it is difficult to imagine the purpose of porting the data to an alternative system.

Individual's rights in relation to automated decision-making and profiling

Nothing in this analysis is related to either automated decision-making or profiling of any individuals.

Accountability and Governance

WWCSC takes and documents the appropriate technical and organisational measures in place to comply with GDPR. Data Protection is overseen by WWCSC's Operations Director with support from a designated member of the Senior Research Team. The approach of WWCSC to information security will be outlined in its IT Usage and Data Protection policies, which are in the process of being finalised as WWCSC becomes independent from Nesta.

Checks on staff

The data will only be accessed by WWCSC research team members. Research staff at WWCSC have undergone data protection training and have substantial experience in handling data, as well as be subject to Disclosure and Barring Service checks. The research team continues to review the training needs of the team to ensure WWCSC's approach remains up-to-date.

Data security arrangements

Data will be transferred securely using a secure platform such as Egress. Egress meets the FIPS 140-2 standard: https://www.egress.com/certifications.

Data will be stored on encrypted hard drives and processed on a non-networked laptop. When not in use, both these encrypted hard drive and non-networked laptop should be stored in the safe.

Data will also be transferred to an external data-archive. Precise details on what archiving service will be used is in the process of being determined, these details will be published once confirmed.

Implementation and Process Evaluation

Aims

The purpose of this implementation and process evaluation is to assess delivery during the rollout of Family Valued across five local authorities. The aim of this is to help understand and explain any identified intervention effects (or lack thereof) in the concurrent stepped-wedge randomized controlled trial, to identify elements of successful delivery, and to improve understanding of the model.

This will build on the findings from the published evaluation from Round 1[24] and ongoing evaluation from Round 2 of the Children's Social Care Innovation Programme, based in the local authority in which the model was developed, as well as WWCSC's ongoing pilot evaluation in Trailblazer local authority Darlington[25]. The design has also been informed by feedback from WWCSC's Young Advisors and Stakeholder Advisory Group, details of which are presented in the pilot evaluation protocol[26].

The research questions and methods for this implementation and process evaluation are set out below. Findings will be published in a final report at the end of the Family Valued Trial.

Research Questions

The implementation and process evaluation seeks to answer the following research questions:

1. **Fidelity and adaptation**
    a. To what extent does delivery in participating authorities adhere to the model?
    b. Are the key assumptions and facilitating factors in place?
2. **Programme differentiation**
    a. What does the existing service structure and practice look like in participating Authorities prior to the introduction of the model?
3. **Reach and acceptability**
    a. What is the number and characteristics of families reached by the intervention?
    b. What is the experience of staff and families who have been involved with the intervention?
4. **Mechanism**
    a. Does implementing the model lead to perceived changes in the interim and ultimate outcomes identified in the logic model?
    b. Is the level of effectiveness of the model perceived to differ for different groups?
    c. Are there any perceived unintended or negative consequences as a result of introducing the intervention?

[24] Ibid
[25] https://whatworks-csc.org.uk/research-project/family-valued-model-pilot-protocol/
[26] Ibid

Design

Planned indicators to answer each research question are presented in the table below. Indicators and thresholds have been developed based on the logic model, previous evaluation findings, and input from the model developers.

Indicators		Method and Time Point
1. Fidelity and adaptation **a. To what extent does delivery in participating authorities adhere to the model?**		
Within each authority:	*Suggested threshold for model adherence*	
- Number and proportion of staff and leaders within children's services frontline teams who have been trained in restorative practice (including which training was attended, their role and the teams they work for).	*70% have attended at least awareness raising training*	Admin data at 6, 12, 24m follow-up
- Number of staff from partner agencies[27] who have been trained in restorative practice (including which training was attended, their role and the service they work for).	*No threshold determined*	
- What is the number of FGC coordinators recruited and trained?	*The number of FGC coordinators has increased*	
- What is the number of new staff recruited to new or restructured restorative services?	*No threshold determined*	
- To what extent is practice in FGCs consistent with key FGC principles and the principles of restorative practice[28] ?	*N/A*	Observation / survey / interviews at 12m follow-up
- To what extent is practice with families following training consistent with the principles of restorative practice?	*N/A*	
- What is the structure and function of newly-commissioned or restructured restorative teams and services?	*N/A*	

[27] Including police, health, and education staff who are to attend awareness raising training

[28] i.e. an independent coordinator and following a three part structure, family led, mobilises support from the family network, enables safe and appropriate involvement of children and vulnerable family members

b. Are the key assumptions and facilitating factors in place?		
Within each authority:	*Suggested threshold for model success*	
- What is the vacancy rate in children's services? - What is the average caseload in children's services?	*20% or below* *17 or below*	Admin data at pre-implementation and 6, 12, 24m follow-up
- What proportion of staff perceive there is sufficient buy-in and support from leadership? - What proportion of staff feel they have enough time for direct work? - What proportion of staff feel they have enough time to take full advantage of the model?	*70%* *70%* *70%*	Survey at 6, 12, 24m follow-up
2. Programme differentiation **a. What does the existing service structure and practice look like in participating authorities prior to the introduction of the model?**		
Within each authority: - Description of the existing structure and practice model of children's services prior to introduction of the model - Description of the ways in which this existing structure and practice model is similar to and different to the new model - Whether any elements of the Family Valued model are rolled out early prior to the intended Operationally Live date		Interviews, focus groups, observation at pre-implementation, and review of LA documentation and publicly available information
3. Reach and acceptability **a. What is the number and characteristics of families reached by the intervention?**		
Within each authority: - Number and characteristics (i.e. demographics, CP/CIN status, primary referral reasons) of families who have accessed FGC or new / restructured services - Proportion of families referred who progressed to FGC (conversion rate) - Proportion of FGCs which resulted in an agreed plan.		Admin data at 12 and 24m follow-up

b. What is the experience of staff and families who have been involved with the intervention?	
- Staff self-reported experience of the model, including facilitators and challenges to delivery and drivers of or obstacles to family engagement. - Family self-reported experience of working with FGC service and staff trained in restorative practice, including drivers of or obstacles to engagement	Interviews / focus groups at 12m follow-up

- What proportion of staff: *(suggested threshold for model success: 70%)*
  - Feel satisfied with how the change process has been managed?
  - Feel satisfied in their jobs?
  - Intend to remain within the authority?
  - Feel prepared and supported by the information, training and support provided?
  - Feel confident to make changes to practice?
	Survey at 6, 12, 24m follow-up

4. Mechanism **a. Does implementing the model lead to perceived changes in the interim and ultimate outcomes identified in the logic model?**	

- To what extent the intervention is perceived to affect:
  - Approach to risk, decision making, care plans, partnership working and support for families?
  - Staff self-reported workload, stress and wellbeing?
  - Family engagement and outcomes, including relationships, wellbeing and risk/safety.
	Interview / focus group / survey at 12m follow-up

b. Is the level of effectiveness of the model perceived to differ for different groups?	
- To what extent are staff and family outcomes perceived to differ according to staff and family characteristics such as authority, area characteristics, staff experience, problem type or demographics such as age of child?	Interview and focus group at 12m follow-up
c. Are there any perceived unintended or negative consequences as a result of introducing the intervention?	
- Staff and family reported negative consequences	Interview / focus group / survey at 12m follow-up

Methods

Data collection

Data will be collected in four phases

- Pre-implementation phase (three months before training is complete & new posts are in place, i.e. the Operationally Live date)
- 6m Follow-up phase (six months after the Operationally Live date)

- 12m Follow-up phase (12 months after the Operationally Live date)
- 24m Follow-up phase (24 months after the Operationally Live date)

The Operationally Live date, set in advance in agreement with the Department for Education, is defined above in the Randomisation section.

Qualitative data (i.e. interviews, focus groups, observations) will be collected at pre-implementation to understand practice prior to the model being introduced, and at 12 months follow-up as this allows a reasonable period of time for the model to begin bedding in before this data is collected. Only this one follow-up point per LA will involve in-depth qualitative data collection to be minimally intrusive. Longer-term adherence and views of the model will be captured through the admin data and survey at 24 months follow-up.
Data will be collected through the following methods. Sample sizes are available in the data collection schedule below.

Admin Data

Administrative data about programme delivery and reach will be collected directly from each LA at the pre and follow-up time points. Admin data is expected to include the following:

Training and recruitment

- Number and proportion of staff and leaders within children's services frontline teams who have been trained in restorative practice (including which training was attended, their role and the teams they work for)
- Number of staff from partner agencies (across police, health and education) who have attended training in restorative practice (including which training was attended, their role and the service they work for)
- Number of FGC coordinators recruited and trained
- Number of new staff recruited to new or restructured restorative services

Service characteristics

- Vacancies in children's services teams
- Average caseloads in children's services teams

Case characteristics

- Number and characteristics (i.e. demographics, CP/CIN status, primary referral reasons) of families who have accessed FGC or new services
- Proportion of families referred who progressed to FGC (conversion rate)
- Proportion of FGCs which resulted in an agreed plan

Survey with staff
A short online survey collected from all staff who have been trained in restorative practice will be undertaken at the follow-up time points. This will aim to understand staff satisfaction and views on the model including perceived benefits of the model.

Interviews with staff
Semi-structured individual face to face or telephone interviews will be undertaken with senior leadership and management across children's services, with staff from family group

conference teams and new or restructured restorative services at the pre-implementation and 12m follow-up time points. These will be expected to last up to 60 minutes.

Focus groups with staff
Focus groups with from early help, safeguarding and children looked after services who are being trained in restorative practice, or part of newly commissioned or restructured services, will be undertaken at the pre-implementation and 12m follow-up time points. These will be expected to last up to 90 minutes.

Observations of practice
Observations of home visits with social workers and staff being trained in restorative practice will be undertaken at the pre-implementation and 12m follow-up time points. Observations of family group conferences will be undertaken at the 12m follow-up time point.

Interviews with families
Interviews with parents and young people from cases who have worked with family group conference services and staff trained in restorative practice will be undertaken at the 12m follow-up time point. Interviews will be expected to last up to 45 minutes.

Sample Recruitment and Selection Criteria
The research team will develop study information sheets, a privacy notice and consent forms to be used in the recruitment process. To ensure that data collected is theoretically comprehensive, participants will be sampled purposively, and stratified according to a range of characteristics set out below.

Interviews, focus groups and observations with leaders, managers and practitioners
Leaders, managers, staff in teams being trained in restorative practice, and Family Group Conference coordinators will be approached to take part in the study. The researcher will work with administrative and management staff in the LA to identify and contact staff. Information will be provided to staff by email and through team meetings. The researcher will only collect data that is necessary for the evaluation and will aim to reduce burden wherever possible through providing clear information and arranging data collection at times and locations that are convenient for staff and families. Interviews and observations will be stratified to include leaders, managers and practitioners across a range of professions, roles and experience, and from a range of teams.

Interviews and observations with families
Parents, carers and young people whose case is or has been open to teams trained in restorative practice, including Family Group Conference teams and newly commissioned or restructured services, will be recruited for observations and qualitative interviews. Social workers will be encouraged to approach all families where it is appropriate to do so, explain the study and ask if they would be interested in speaking to a researcher. If the family agree, the researcher will give further details, answer questions, and proceed with informed consent procedures.

For young people under 16 a parent or carer will provide consent in addition to the young person's own assent to participate. The researcher will ensure that family individual needs, such as learning disabilities, are taken into account through discussing with the social worker

in advance of any interview or observation. For families where literacy or language affect understanding of the written research materials, the researcher will be available to explain the materials verbally in plain english in person or over the phone, supported by the worker and checking for understanding. In addition to a verbal explanation of the research by the social worker and researcher, and the opportunity to ask questions, a tailored version of the information sheet, using accessible language, will be provided to families (and where relevant, children and young people). Where families prefer that observations of home visits are not recorded, written notes will be taken.

Within each LA we will seek to interview and observe practice with families assigned to a range of teams and lead social workers within those teams. Across the whole sample we will also seek to include ethnic minority and ESL families.

Data Collection Schedule

Method	Sample and size *per LA* at each time point	*Pre*	*6m Follow-up*	*12m Follow-up*	*24m Follow-up*
Admin Data	Across children's services	X	X	X	X
Survey with staff	All managers and all staff who have been trained in restorative practice		X	X	X
Interviews with staff	• Senior leadership (n = 2-3) and management (n = 2-3) within services trained in restorative practice, as well as newly commissioned or restructured services • Family group conference staff (n = 1-3 depending on size)	X		X	
Focus groups with staff	Staff from early help / safeguarding / children looked after services who are being trained in restorative practice or part of newly commissioned or restructured services (2 focus groups of 6 staff)	X		X	
Observations of practice with families	Home visits with social workers and staff being trained in restorative practice (n = 4)	X		X	
Observation of family group conferences	Family group conferences (n = 3)			X	
Interviews with families	Parents (n = 3) and young people (n = 3) working who have participated in family group conferences. Parents (n = 3) and young people (n = 3) who have worked with teams trained in restorative practice and / or newly commissioned or restructured services.			X	

Analysis

Preparation and analysis of qualitative data

Interviews and focus groups will be recorded, transcribed and pseudonymised prior to analysis.

Qualitative analysis of interview, focus group and observational data will use NVivo software and follow a thematic analysis approach. This will involve data familiarisation, checking accuracy of transcription, labelling the data with descriptive codes and developing themes which describe patterns across the data to answer the pre-specified research questions. Analysis will look for patterns, consistencies and inconsistencies across different informants, sites and time points that might be informative for the research questions.

The following steps will be taken to ensure rigor in the analysis and reporting of qualitative data:

- Confidence that the findings are an accurate reflection of participant experience will be ensured through presentation of examples of participant responses using quotes, and triangulation between different informants and data collection methods.
- The degree to which findings are transferable to other contexts will be considered through detailed description of contextual factors, and collection of data from a range of informants to gather a range of perspectives.
- Transparent reporting of the research and analysis process will ensure the study methods are clear and repeatable.
- When interpreting findings, consideration will be given to contrasting and inconsistent accounts, as well as findings from previous research using the intervention model.

Analysis and triangulation of quantitative and qualitative data

Research Question 1: Fidelity and Adaptation

Admin and survey based indicators of staffing and training, as well as assumptions and facilitating factors (specified in Table 1) will be presented descriptively for each local authority at each time point, to illustrate what is being delivered in each authority, as well as how this varies between authorities and how this changes over time. This will be supplemented using the suggested thresholds for each indicator to establish the extent to which each local authority is delivering each element of the model as intended.

These findings will be triangulated with a description of the structure and function of new and restructured restorative services, as well as qualitative assessments of the extent to which family group conferences and practice with families are being delivered in a way that is consistent with the principles of Family Valued and Restorative Practice.

Research Question 2: Programme Differentiation

Qualitative data from interviews, focus groups and observations at pre-implementation, and review of LA documentation and publicly available information will be used to provide a description of the existing structure and practice model of children's safeguarding services prior to the introduction of the model, a description of the ways in which this is similar to or different to the new model, and whether any elements of family valued are rolled out early prior to the intended Operationally Live date.

Research Question 3: Reach and Acceptability
Admin data indicators (specified in Table 1) of the number and characteristics of families reached by the intervention over the course of the evaluation period will be presented descriptively for each local authority.
Survey based indicators of staff satisfaction at each follow-up time point will be presented descriptively, supplemented by an assessment of whether these indicators have reached the suggested threshold for intervention success as specified in Table 1. These will be triangulated with qualitative findings in relation to how the model has been received by staff and families.

Research Question 4: Mechanisms
Qualitative data from interviews and focus groups, as well as survey data at 12 month follow-up will be used to assess staff and family perceived changes as a result of the model and any negative consequences.

Data Protection

What Works for Children's Social Care will act as data controller for the IPE. All directly collected data through surveys, interviews, observations and focus groups will be processed on the legal basis of consent. This includes provision of family contact information to the researcher, which will be provided only with family prior agreement to be contacted. Aside from contact information, all other administrative data collected for the IPE will be collected at the aggregate level and will therefore not contain any personally identifying information. All data will be handled in accordance with GDPR regulations. Data will be pseudonymised and depending on the type of data stored securely in encrypted files or locked rooms in secure buildings. Data will only be used for the purpose of the stated research aims and only be accessed by members of the research team. Third party transcription services may be used where a confidentiality and data sharing agreement is in place. Personally identifying data will be deleted five years after the end of the study (final publication of the full SFPC evaluation).

A privacy notice will be provided to all individuals taking part in direct data collection indicating the legal basis for processing data, what data is being collected and why, who is collecting the data, how data will be handled and stored and who to get in touch with for information or complaints.

Timeline

		-3m	0m	6m	12m	24m
	LA	IPE Baseline	Operationally Live	IPE 6m Follow-up	IPE 12m Follow-up	IPE 24m Follow-up
1	Warwickshire	Mar-20	Apr-20	Oct-20	Apr-21	Apr-22
2	Newcastle	Jul-20	Oct-20	Apr-21	Oct-21	Oct-22
3	Coventry	Jan-21	Apr-21	Oct-21	Apr-22	Apr-23
4	Solihull	Jul-21	Oct-21	Apr-22	Oct-22	Oct-23
5	Sefton	Jan-22	Apr-22	Oct-22	Apr-23	Apr-24

*This timetable is indicative only. Evaluation dates may be subject to change in line with changes to delivery timescales

Ethics

Research Ethical Approval

The Implementation and Process Evaluation component of this trial protocol underwent ethics review by a member of WWCSC's Evaluation Advisory Board, and recommendations were incorporated into the protocol.

WWCSC is currently reviewing its ethical review process and establishing a Research Ethics Committee, which will review the RCT component of this trial protocol, before any data will be shared by local authorities.

Ethical considerations

The project lead(s) will take ownership of ongoing monitoring of ethical issues throughout the research lifecycle. This will include regular contact with authorities during fieldwork periods, to allow ethical concerns to be raised and discussed, as well as regular review points within the research team, following the completion of each data collection phase for each wave. Should any unexpected ethical issues arise during the project, the research team will take advice from the WWCSC Research Ethics Committee.

Ethical Issue	IPE Mitigation	Impact Evaluation Mitigation
Confidentiality	Confidentiality will be ensured through removal of identifying information before analysis and ensuring no individual, family or team can be identified in the reporting of results. Participants will be notified of this, and that their answers will in no way affect their treatment, either by their employer in the case of staff, or children's services, in the case of families. Given numbers are quite small, care will be taken in reporting to ensure participants cannot be individually identified.	All data will be pseudonymised prior to being sent to WWCSC, and therefore very unlikely to be able to be identified by researchers at WWCSC. The outputs will be aggregate statistics and will be checked for statistical disclosure (e.g. mask cells with smaller than 10 observations). This will be explained on the privacy notice that will be available on WWCSC's website.
Risk of harm or distress	Data collection will be undertaken with potentially vulnerable populations on potentially sensitive topics. Because families will already be working with social work professionals, the likelihood of disclosure of any harm or risk of harm that has not already been disclosed to the social worker families will already be working with is low. Families will be made aware prior to participating that their responses will be pseudonymised and remain confidential with the exception that any disclosure of harm or risk of harm will need to be reported to the family's social worker for safeguarding purposes. All researchers collecting direct data will be subject to DBS checks, and trained in safeguarding procedures. If the sensitive nature of any content of the evaluation does lead to any participant becoming distressed the evaluator will assist them in seeking support through their social worker, or by signposting any other local support services as agreed with the individual LA, and remind them of the option to discontinue or withdraw. In the unlikely event that the data collected suggest that the intervention is causing harm,	The data used is administrative data which is collected / created in the course of day to day children's social work, and no further collection of data is required. The data is being used for statistical research to understand whether a practice model is working and contribute towards improvements in public services. We believe that "the reasonable person" would find the use of data in this way acceptable, and would not cause them any harm or distress. The low risk of harm mostly comes from the possibility of harm if the individual were identified (very unlikely) following a data breach (also very unlikely). We will mitigate the risk of a data breach through following detailed data handling procedures. What Works for Children's Social Care is in the process of updating its data handling policies and procedures - these will be detailed in this protocol before publication.

	this will be reported to those responsible for programme delivery. All efforts will be made to avoid any visits to family homes by lone researchers, using either phone interviews or travelling together with a social worker or another researcher for face to face visits. If there is an unplanned need for lone researchers to visit families, safety will be ensured through following a lone working policy. In accordance with the employer's lone working policy, researchers working alone will always carry a means of communication and ensure that colleagues are aware of their whereabouts and that they are working on their own. Researchers will check in and out with a colleague before and after any lone working visits. If there is any indication that the researcher's presence during observation of social worker practice adversely affects any family member or professional practice, then the researcher will discontinue the observation, and, if appropriate, follow relevant safeguarding procedures.	
Informed Consent	All participants will have the opportunity to ask questions, will be asked to give consent to participate and will be made aware that participation is optional. For young people under 16 a parent or carer will provide consent in addition to the young person's own assent to participate. Procedures for families affected by learning disability or difficulty understanding study information and written materials are set out in the sample recruitment section above.	Due to the nature and scale of the data collection, it is not possible for us to gain informed consent from research participants. However we will publish a privacy notice providing details of the study.
Right to Withdraw	All participants will be made aware they have the right to discontinue participation or withdraw at any time, including withdrawing their data at any point before aggregated analysis has been completed. Contact details will be	In our privacy notice we will provide mechanisms for individuals to withdraw from the study, should they wish

	provided so that participants can directly request this.	
Feedback for Participants	A short accessible summary of the final research report will be publically available for participants to access	A short accessible summary of the final research report will be publically available for participants to access

Risks

This section outlines the anticipated risks to evaluation success that may arise and steps that will be taken to mitigate against these.

Risk	Likelihood	Impact	Mitigation
Low engagement of LA staff and families in evaluation (IPE)	**Low**	**Medium**	The study is designed to collect only data that is necessary for the evaluation, and to minimise burden on the local authority and participants by ensuring that interview times and locations are flexible and convenient to participants and that any survey proforma or data template is clear and brief. Although there may be challenges engaging busy practitioners and families with complex circumstances, involvement of only a proportion of the overall number involved with the intervention is needed to reach recruitment targets. Therefore reaching targets is expected to be achievable. Given their smaller numbers overall, participation will be needed from a reasonable proportion of senior leaders. However, it is expected that these staff members will be easier to engage due to their investment in the programme. The evaluation aims to triangulate between a range of informant sources, therefore a lower response rate among one informant group will not have a major overall impact on the ability of the evaluation to achieve its aims.
Intervention not sufficiently embedded in time for the process evaluation (IPE)	**Medium**	**Medium**	Given the complexity of the model being delivered, it is likely to take some time for practice to change and be embedded. The process evaluation has allowed a reasonable amount of time for the intervention to begin to embed before follow-up data is collected. It is acknowledged that the longer term embedding and sustainability of the programme after the first two years is out of scope of this evaluation. Should there be delays with delivery, the evaluation dates will be delayed accordingly as well.
Delays caused by	**Medium**	**Medium**	WWCSC will work closely with colleagues at the local authority to anticipate where possible, and manage and

changes in leadership, Ofsted inspections, or other unexpected internal or external events (IPE)			minimise any disruption caused by these factors. Should there be delays with delivery, the evaluation dates will be delayed accordingly as well.
Unable to access admin data (IPE)	**Low**	**High**	Administrative data is a key component of the evaluation and important for answering a number of the research questions. WWCSC will work closely with the authority from the outset to establish a data sharing protocol and timeline that is acceptable to both organisations.
Bias in qualitative sampling and reporting from participants (IPE)	**Medium**	**Medium**	It is likely that the families and staff sampled are going to be biased towards being more positive about children's social care. We attempt to address this through our sampling methods, but also will be sure to acknowledge this in our reporting. In addition, a combination of social desirability bias, and concerns about what they say getting back to children's services may lead to families being more positive than reality. Steps will be taken in interviews to build rapport with families, reassure them of the researchers' independence, and explain clearly the confidential nature of the research to minimise this bias.
Allegiance Bias (IPE)	**Low**	**High**	Funding for the evaluation is provided by the Department for Education (DfE). WWCSC must work closely with the authorities who developed the intervention, the authorities introducing the intervention, and the funder of rollout (DfE), in order to deliver the project. This could result in a risk to the independence, or perceived independence of the evaluation. However, in mitigation of this risk, WWCSC are a separate and independent organisation, with their own separate governance processes - a board of trustees whose role includes oversight of the independence of the organisation. Further, WWCSC will act as a data controller for this evaluation. Therefore, the way in which the data is processed is determined by WWCSC and not any other organisation. In addition, the WWCSC evaluators come from a neutral standpoint, informed by the current state of the evidence. There is so far no evidence of impact of the model relative to a robust counterfactual, and the model is therefore in a position of equipoise. The publication of a protocol in advance of data collection will also ensure that the evaluators follow a pre-planned approach, providing full transparency of methods and rationale. In addition, as stated in the qualitative analysis methods, consideration will be

			given to contrasting and inconsistent accounts, and quotes and triangulation across informants and methods will be used to support findings that are reported. Finally, researchers will aim to reassure participants that identifying information will not be shared outside of the research organisation - providing families and staff an opportunity to speak more freely and openly than they might do otherwise.
Data is not available in required format (RCT)	**Medium**	**High**	We will send a draft data-collection template to local authorities far in advance, and consult with relevant data teams at local authorities to ensure they understand and are able to provide the data we need. If they are not able to do at initial consultation, we will support them to ensure that they can by the time outcome data is available.
Implementation date changes significantly (RCT)	**Medium**	**Low**	Changes to the implementation date, if not taken into account in the analysis, could significantly undermine the analysis. We have mitigated against this by allowing for some flexibility within the trial protocol. See the section on randomisation above for details. In order to be able to take any changes out in our analysis, we have been clear that it is important that all parties clearly communicate with us. In addition this should be picked up in the process evaluation. Significant changes could also delay outcome data, and so reporting.
Implementation happens over time, not allowing for precise definition (RCT)	**Medium**	**Medium**	If the key components of the model are delivered across a wide time-period, it could be difficult to determine when to classify children as treatment or control. In the Randomisation section we try to provide some clarity for how we will do this; in addition our sensitivity analysis should help somewhat. However it would remain that this could bias our treatment estimate.
Lack of fidelity or inconsistencies in implementation (RCT)	**Medium**	**Low**	This could obscure what it is we are evaluating. The IPE will explore how the model was delivered in the different local authorities, so will allow us to contextualise the findings.
Unanticipated changes in local authorities (RCT)	**Medium**	**Medium**	Such as changing in assessment thresholds, could bias our results. Our IPE should help us know whether this is the case. We also have determined in our analytical strategy that we would add dummy covariates for implementing other models during the trial period.

Registration

To safeguard against spurious findings, we will register the study with the Open Science Framework (OSF) before any outcome data are obtained.

Personnel

The evaluation is funded by the Department for Education and will be undertaken by What Works for Children's Social Care (WWCSC). The Principal Investigator is Michael Sanders (Executive Director, WWCSC).

Impact evaluation personnel

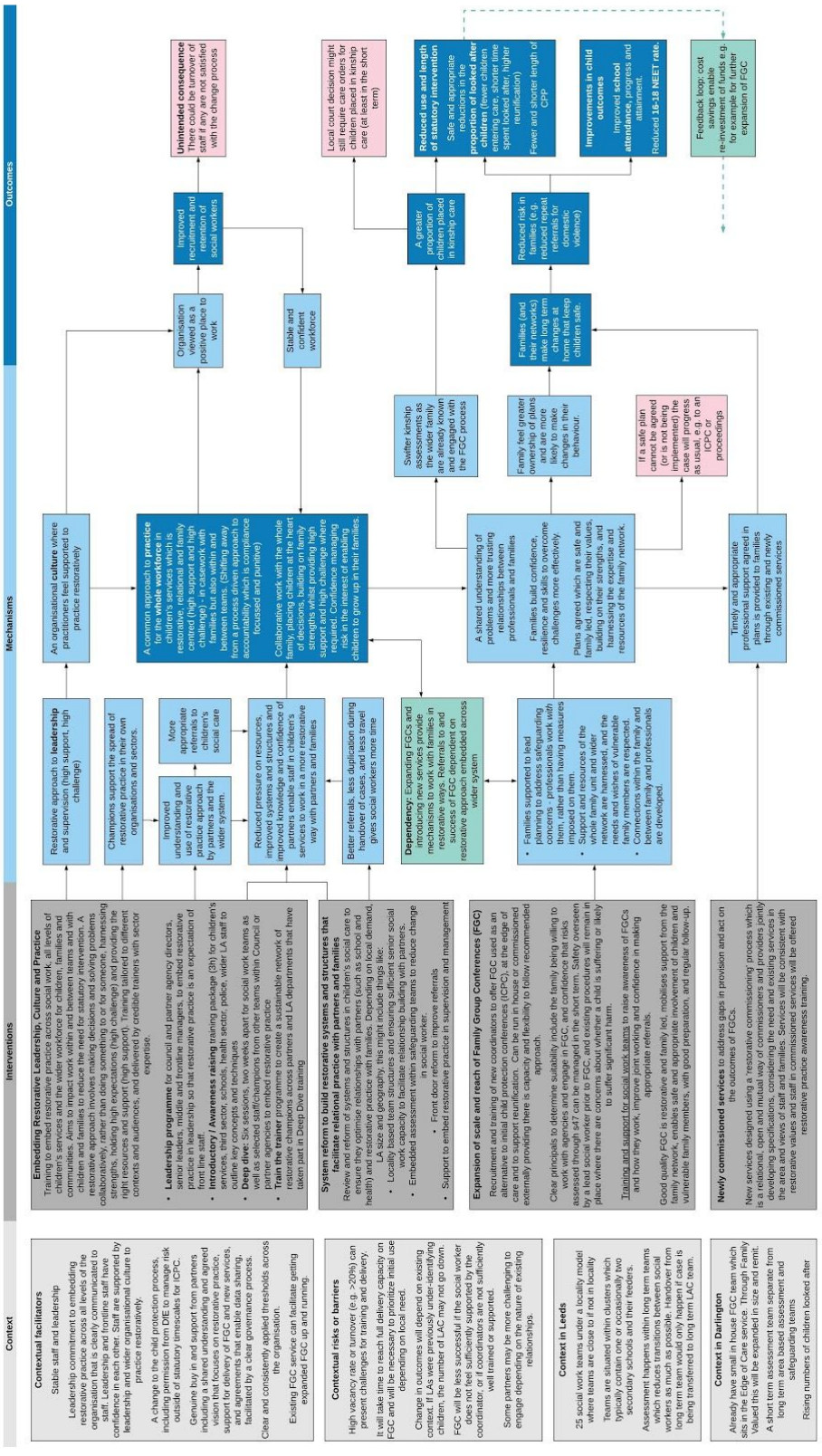
For the impact evaluation: data collection, analysis and reporting will be led by Eva Schoenwald (Researcher, WWCSC), and overseen by Patrick Sholl (Research and Programmes Manager, WWCSC). The work will be done in consultation with Dara Lee Luca (Economist at Mathematica Policy Research, and Adjunct Lecturer in Public Policy at Harvard Kennedy School).

Implementation and process evaluation personnel

IPE data collection, analysis and reporting will be led by Hannah Collyer (Senior Researcher, WWCSC - project lead for process evaluation), supported by Abby Hennessey (Research Assistant), Daniel Kearns (Research Assistant), and overseen by Louise Reid (Head of Programmes and Research, WWCSC).
There will be frequent communication and collaboration between the staff working on each component.

Appendices

Appendix A: Draft Logic Model



Appendix B: Power calculations Stata code

steppedwedge, binomial detectabledifference complete(1) vartotal(0) p1(0.04) m(2600) k(1) rho(0.01566) alpha(0.05) beta(0.8) steps(5)