Trial Evaluation Protocol: Strengthening Families, Protecting Children - Family Safeguarding Model - Difference-in-differences analysis

Intervention Developer	Hertfordshire County Council
Evaluator	What Works for Children's Social Care
Principal Investigator	Michael Sanders
Protocol Author(s)	Eva Schoenwald, Patrick Sholl, Vicky Clayton, Michael Sanders
Type of Trial	Matched difference-in-differences analysis
Age or Status of Participants	Children aged 0 - 12, that have been referred to Children's Social Care (further restrictions apply depending on outcome measure)
Number of Participating Local Authorities	5
Number of Children and Families	approx. 100,000, of which half are in a treatment local authority.
Primary Outcome(s)	Likelihood of becoming looked after within 18 months of referral
Secondary Outcome(s)	Likelihood of returning to statutory services following a closed CPP; Child Protection Plan (CPP) plan duration; days on CPP; likelihood of repeat referrals for domestic violence; mental health, or substance misuse; 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" and high rates and/or rising numbers of looked after children over the last three years

Table of contents

Background and Problem Statement

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 adopt and adapt 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 Works for Children's Social Care (WWCSC) is conducting a four-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.
- Given the challenges the COVID-19 pandemic poses to evaluating a stepped-wedge RCT, a **difference-in-differences analysis** will be conducted to provide an additional approach to analysing the programmes' effects and to increase the robustness of the impact evaluation estimates.

This document sets out the protocol for the difference-in-differences evaluation of Family Safeguarding.[1]

Family Safeguarding

The Family Safeguarding Model was developed in Hertfordshire with support from the Department for Education's Innovation Programme. Its delivery in Hertfordshire was evaluated by academics at Cardiff University and the University of Bedfordshire.[2]

The intervention supports a whole-system change to a local authority's child protection approach for children up to adolescence, focusing on supporting the needs of both children and adults in order that children can safely remain within their families. This involves:
- Establishing multi-disciplinary teams where specialist adult practitioners in domestic abuse, mental health and substance misuse are co-located with social workers under a unified management structure. This enables a multi-disciplinary whole family response through direct assessment and support from specialist adult practitioners

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[1] The protocol for the IPE and RCT can be found under https://whatworks-csc.org.uk/wp-content/uploads/WWCSC_Family-Safeguarding_TP_Final_V1.pdf.

[2] Forrester, D., Lynch, A., Bostock, L., Newlands, F., Preston, B. & Cary, A. (2017) Family Safeguarding Hertfordshire: Evaluation Report. Department for Education: London

as well as multi-professional group case discussions and sharing of knowledge and skills across disciplines.

- Use of Motivational Interviewing (MI) as a framework for practice for all staff. Staff undergo training and ongoing skills development workshops and follow a structured solution-focussed intervention programme with families which aims to work collaboratively with families and increase engagement.
- Using an electronic assessment workbook which provides a single data tool for all professionals and links to the work programme. This increases ease of information sharing between professionals and reduces social worker time spent recording and sharing information.

Social workers trained in Motivational Interviewing may see wider benefits to their practice. However, the full Family Safeguarding package, i.e. the involvement of adult specialist practitioners, is primarily expected to support cases where the primary referral reasons relate to child abuse or neglect from within the family. The model developers also recommend that this Family Safeguarding work is facilitated where cases with contextual safeguarding or other non-family based risk factors are held by separate teams (sometimes called Adolescent Teams). Although this is the preferred approach, local variation in system structure may mean that this preferred structure is not always possible. However even in teams with a wider remit the core Family Safeguarding work would still take place primarily where safeguarding risks come from within the family.

Although terminology will vary in each authority, throughout this protocol, 'children's safeguarding teams' is used to refer to social work teams undertaking safeguarding and child protection functions before the introduction of the family safeguarding model, while 'family safeguarding teams' is used to refer to these such teams after the introduction of the family safeguarding model.

Context

The difference-in-differences (DiD), IPE and RCT parts of the evaluation will be undertaken in the local authorities funded by the Department for Education to introduce Family Safeguarding as part of the Strengthening Families, Protecting Children programme, with the exception of the Trailblazer who is participating in a separate pilot evaluation. These local authorities are due to launch Family Safeguarding at a minimum of four-month intervals beginning in September 2020.[3] In the order they will be rolled out, these local authorities are Walsall, Lancashire, Telford & Wrekin, Wandsworth and Swindon.[4]

All authorities had a judgement of 'requires improvement to be good' at the point at which they applied for the programme. However, at the point of rollout to the first local authority, Children's Services in three of these authorities (Walsall, Lancashire and Wandsworth) have an Ofsted judgement of 'requires improvement to be good', while Swindon received a judgement of 'good' in July 2019 and Telford & Wrekin received a judgement of 'outstanding' in January 2020. These authorities were selected by the Department for Education to participate in the programme due to having high rates of children looked after compared to

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[3] Originally, the intervals were due to be six-months. Delays in implementation due to the ongoing COVID-19 pandemic have meant that some of the timings have shifted and local authorities are implementing the model in less than 6 month intervals for some wedges.

[4] Note that the order and length of intervals might be at risk of changing due to the implications of COVID-19 for the programme implementation.

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.

York Consulting has conducted a process evaluation of Family Safeguarding in four additional local authorities, namely Peterborough, Luton, Bracknell Forest and West Berkshire.[5]

In the DiD analysis, we will use the five local authorities named above as our treatment group, and produce a comparison group from other local authorities that follow similar trends over time to these treatment local authorities.

The ongoing impact evaluation of Family Safeguarding by What Works for Children's Social Care uses a Stepped Wedge Cluster Randomised Controlled Trial design to estimate the impacts of Family Safeguarding on children and families. While this design was chosen because of its robustness, the COVID-19 pandemic affects local authorities differently, making them less comparable in an RCT set-up without additional comparator local authorities.

Furthermore, there have been a number of changes to the timings of implementation, and it is likely that more may follow. Specifically, so far there has been a shortening of the gaps between go live dates. Smaller gaps between the go-live dates mean that we have less data points in each stage of the implementation, which makes it harder to clearly attribute changes that occur over time to the implementation of the model.

In addition to this, it seems possible that there may be changes to the order in which local authorities go live. Changes to the order threaten the randomised nature of the design, which can lead to significant differences between local authorities that implemented the model first compared to those that implement at a later stage. This can make it harder to estimate a causal effect of the model.

The difference-in-differences analysis can provide a second lense through which to analyse the programme, which rests on different assumptions and is thus not reliant on the order of implementation. Additionally, the difference-in-differences analysis will aim to take the differential effect of COVID-19 on different local authorities into account, by choosing comparator local authorities for each of the five local authorities implementing the Family Safeguarding model, where the trends in outcomes before implementation of the model are most similar to the Family Safeguarding local authority. This approach will thus choose comparator local authorities that have had similar developments in their outcomes for children and families before and during the pandemic, to make the groups as comparable as possible. For local authorities that have implemented the model before or during the pandemic, this matching approach will only provide limited improvements since the main effects of COVID-19 might only occur in the period that is not part of the matching dataset.

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[5] The process and impact evaluation conducted by York Consulting includes a series of case studies and interviews and a cost-benefit analysis using a Fiscal Return on Investment model. See: https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/932367/Hertfordshire_Family_Safeguarding.pdf.

Impact Evaluation

Aims

Family Safeguarding was first implemented in Hertfordshire and demonstrated promising results. However, the original evaluation was conducted using a pre-post design. The current evaluation uses a triangulation of results from a stepped wedge cluster randomised controlled trial approach and a difference-in-differences analysis to provide a more robust evaluation of the impacts of Family Safeguarding 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 Safeguarding 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 impact evaluation, is whether it achieves one of its primary goals - namely reducing the number of children looked after. The population of interest are children who have been referred to children's social care and are under the age of 13 at the time of referral. This is the primary age group that the Family Safeguarding teams in the developer local authority Hertfordshire work with and is designed to impact. While Safeguarding teams can in theory work with adolescents as well, safeguarding concerns among adolescents are more likely to become contextual which in some local authorities are specifically addressed by adolescent teams. We assess the following primary research question of interest:

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

Given the multifaceted nature of the model, we also expect to see changes in other important, but secondary outcomes, such as a reduction in the likelihood of children returning to statutory services. For some of these secondary outcomes our population of interest is either expanded, or further restricted, as detailed in the difference-in-differences Design Table below. 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 Safeguarding on the time spent on child protection plans?
3. What is the impact of Family Safeguarding on the likelihood of children being re-referred for parental substance misuse, domestic violence or parental mental health issues?
4. What is the impact of Family Safeguarding on the unauthorised school absence rates of children referred to children's social care?

Design

The design of the analysis is a difference-in-differences (DiD) design. The unit of analysis is at the individual level to optimise the power to detect an effect within the constraints of the project.

In a DiD design, we are comparing the change over time in outcomes in the local authorities implementing Family Safeguarding ("treatment group") with the change in outcomes in

comparator local authorities ("comparison group"). Each local authority implementing Family Safeguarding will be matched to comparator local authorities that have not implemented Family Safeguarding. More information on the matching procedure is detailed below.

This analysis is intended to complement the stepped wedge RCT analysis conducted by WWCSC. The pandemic has affected the Family Safeguarding local authorities to different degrees (e.g. in the form of delays to implementation, moving to remote working, etc.) Since an RCT relies on the assumption of treatment being random, this threatens the robustness of the RCT analysis for several reasons. Firstly, changes to the order of implementations can threaten the randomised element of the stepped wedge design. Secondly, shortening the gaps between local authorities going operationally live reduces our chance of being able to detect the impact of the programme as all five local authorities serve as the comparison group to each other. Thirdly, Covid may impact local authorities in different ways which causes concern for the design due to the small number of sites involved as it can change trends in outcomes over time and affect the degree of comparability between the five local authorities. a In order to counteract these risk, a difference-in-differences approach will help by comparing local authorities with similar trends in outcomes before *and* during the pandemic (before the implementation of the Family Safeguarding model in the selected authorities), thus making the comparator groups as similar as possible to the five local authorities implementing Family Safeguarding.

Table 1: Outcome variables definition and measurement

Trial type and number of arms		Difference-in-differences
Unit of analysis		Individual (child/young person)
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 aged 0-12 that have been referred within the trial period.
Secondary outcome 1	variable	CPP plan duration
	measure	Discrete variable equal to the number of days that the child has been on a single CPP. Plan length is recorded up to 12 months from the start of the CPP and censored for larger values.
	sample	Children aged 0-12 that have been referred within the trial period and that also started a CPP within 6 months of the initial referral start date.
Secondary outcome 2	variable	Days on CPPs
	measure	Discrete variable equal to the number of days that the child has been on CPPs over a period of 18 months from initial referral.
	sample	Children aged 0-12 that have been referred within the trial period.
Secondary outcome 3	variable	Repeat referrals for parental substance misuse, parental mental health or domestic violence

	measure	Coded 1 if the child has been re-referred within 18 months of an initial referral where the factors identified at the end of the assessment following the first referral included either parental substance misuse, parental mental health, or domestic violence. As measured 18 months after first referral. Coded 0 if not re-referred within 18 months.
	sample	Children aged 0-12 that have been referred within the trial period. The factors identified at assessment must include parental substance misuse, domestic violence, or parental mental health.
Secondary outcome 4	variable	Unauthorised school absence rates (exploratory)
	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 for the three terms that start after the initial referral date.
	sample	Children aged 0-12 that have been referred within the trial period.

We will use administrative, secondary data for the analysis. The administrative data will be requested from the ONS' National Pupil Database (NPD) via the Secure Research Service (SRS).

Matching

Local Authority Level Matching
We match treatment local authorities to control local authorities that most closely resemble them in the prior trends in outcome variables before the implementation of Family Safeguarding. We will then analyse individual-level data from the treatment and control local authorities.

Exclusion Criteria
We select control local authorities from all English local authorities with children's social care services excluding:
- Local authorities also using Family Safeguarding (or due to start using Family Safeguarding as part of the Innovation Programme).
- Local authorities which are likely to experience "contamination" from the local authorities implementing Family Safeguarding as they are partners in the Partners in Practice programme[6]
- Local authorities for which we have fewer than two years of data prior to the implementation of their matched local authorities' implementation of Family Safeguarding.

Matching on parallel trends
The identifying assumption in a DiD analysis is that there are parallel trends in outcomes between the treatment and comparator local authorities that would have continued if not for the implementation of the programme. While this assumption cannot be definitively proven, we can increase the likelihood by choosing as comparator local authorities the authorities whose trends in outcome variables match the treatment local authorities' as closely as possible prior to the introduction of Family Safeguarding. We match on local authority level

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[6] Innovation Programme. Partners in Practice. https://innovationcsc.co.uk/partners-in-practice/.

trends in primary and secondary outcomes for the two years prior to the intervention. Since we have access to very granular data via the ONS, we will match based on quarterly outcomes. for the four years prior to the intervention, excluding cases that entered the sample population in the final 18 months prior to implementation[7]. Local authorities will be matched based on the actual outcomes measures that will be used for the full analysis, aggregated to the local authority - quarter level. Quarterly outcomes will provide a higher quality of matching that also more accurately depicts the impact of COVID-19 on the individual local authorities.

We set out to find matches for each local authority for each outcome variable separately. Specifically, we match on the shortest normalised distance between the data for every treatment local authority and its potential comparators. The data is the change in the outcome variable between one quarter and the next (the outcome variable is standardised to take into account changes in the way that it is measured over the quarters) for all quarters in the two years prior to the introduction of Family Safeguarding in the specific treatment local authority. For each treatment local authority, the lowest scoring pairs whose trends are also convincing when inspected visually will be first preference for matching.[8] We will also test the robustness of the parallel trend assumption using placebo tests which are described in more detail below. Matching will be done with replacement, such that a single comparator could be used as a match for multiple treatment local authorities.

Once identified, the local authority pairs will be subject to further qualitative analysis to assess whether the matched authorities are likely to fulfill the common shocks assumption. According to this assumption, any event that occurs following the programme's implementation should affect each local authority equally (in other words, the parallel trends would continue to hold and deviations from parallel trends can be interpreted as a treatment effect). To test this assumption, we identify shocks that we expect to have repercussions in many local authorities (e.g. substantial serious case reviews that lead to reactions/changes in the entire sector) and assess whether the outcomes[9] in the local authority pairs appear to respond similarly. Secondly, we will seek to identify shocks that are potentially more idiosyncratic and thus threaten the validity of the parallel trends and common shock assumptions. Finally, we will run sensitivity analysis using only data from when Family Safeguarding was already implemented where we control for the common shock and its interaction with the presence of Family Safeguarding. A significant coefficient of the interaction effect will indicate a potential violation of the common shock assumption. These shocks will have to be large enough and relevant enough to our outcome measures that we can assume they will affect the outcomes for a particular local authority. Examples of such shocks include:
- Introduction of a new (whole-system) practice model
- Serious case reviews with repercussions for the local authority
- Local safeguarding children board reports with consequences for the local authority.

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[7] This is done to avoid confounding pre-intervention trends with potential early treatment effects, as we observe some children over a period of up to 18 months. We run sensitivity analyses on our main analysis to gauge the extent of the under-estimation of treatment.

[8] We visually inspect the pairs starting with the lowest scoring pair. If we find more than one parallel trend convincing, we will include more than one comparator LA. If none of the pairs are deemed adequate visual matches, then we will exclude the treatment LA from the analysis.

[9] We will also seek to look at outcomes affected by a common shock that do not form part of the Family Safeguarding research questions. This will enable us to disentangle the effects of a common shock from the effects of the intervention.

The local authorities identified as the closest match for each outcome for the treatment local authority and where our additional analysis suggests that it is likely that the parallel trends and common shocks assumptions hold will then be considered the control local authorities for the respective outcome. We will analyse their pseudo-anonymised individual-level administrative data of the outcomes accessed via the Office for National Statistics (ONS) from 2017/18 (which is two years before any treatment local authority they are matched to started Family Safeguarding) up until data from the 2023/24 year. If matches are not identified from the 10 closest neighbours, we accept that we cannot conduct the analysis for that treatment local authority and will exclude this local authority for the analysis.

Individual-level Matching

After matching at local authority level, we also match at an individual (child) level within the local authority matched pairs using coarsened exact matching (CEM[10]). We do so in order to decrease the imbalance on covariates between the treated and control individuals, allowing the identification of a better causal estimate.

We chose CEM to match at an individual level because it allows analysts to specify ex ante the maximum acceptable imbalance. It also has a number of other desirable properties, for example, it removes the need for an additional process to restrict data to an area of common support, meets the congruence principle, is robust to measurement error, and is computationally fast (important given that the dataset will be large relative to computing power available). CEM works by first temporarily coarsening the control variables based on the user's selection so that the continuous variables are cut into categories (e.g. age as an integer coarsened to 0-5, 5-10, 10-15, 15-18 years) and categorical variables are combined (e.g. school year coarsened to primary school, senior school). All individuals are then assigned to strata with the same coarsened control variables. Strata which do not have at least one treatment and control individual are dropped.

We match on individual-level control variables within the same financial year (if this results in dropping too many observations, we will match on variables within the same time period - pre-treatment or post treatment). For the purpose of CEM, we specify the coarseness of the variables as:

- Gender (included as a binary indicators: 0=Not recorded/unborn, 1= male, 2=female, 3=indeterminate)
- Age of children at the time of referral (0-3, 4-12)
- Ethnicity (major group[11])
- Disability (included as a binary indicator: 0=No, 1= Yes)
- Free school meal eligibility in the last six years or pupil premium eligibility if child is in reception, year 1 or year 2[12] (included as a binary indicator: 0=No, 1= Yes)
- Is child an Unaccompanied Asylum Seeker[13] (included as a binary indicators, 0=No, 1= Yes)

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[10] Iacus, S., M., King, G. & Porro, G. (2018, April 12). CEM: Software for Coarsened Exact Matching. CRAN. https://cran.r-project.org/web/packages/cem/vignettes/cem.pdf
[11] The major ethnic groups are: White; Mixed or Multiple ethnic groups; Black, African, Carribean or Black British; Asian or Asian British; Other ethnic group
[12] as all infant school children in government-funded schools are FSM eligible
[13] UN High Commissioner for Refugees (UNHCR), UNHCR's Engagement with Displaced Youth, March 2013, available at: https://www.refworld.org/docid/5142d52d2.html [accessed 14 June 2019] p28

- Whether or not the child has previously been made the subject of a CP (0, 1)

We make sure that individuals from each trial local authority are only matched to individuals from their specific comparator local authority (that has been identified as having parallel trends) in the CEM procedure. We only match individuals from the same financial year. If this means that a considerable share of treatment group observations have to be dismissed, we reserve the option to widen this criteria to match only individuals from the same period (before the implementation of Family Safeguarding and after the implementation of Family Safeguarding). Note that the coarseness is only for matching purposes and we describe our operationalisation of covariates for inclusion in the regression below. We report the proportion matched and the multivariate imbalance score which measures imbalance with respect to the joint distribution, including all interactions, of the covariates (Iacus, King and Porro, 2011)[14]. We then estimate the difference-in-differences regression weighted by the weights that equalise the number of treated and control individuals within each CEM stratum.

Sample size / MDES calculations

		Values
MDES (Cohen's d)		0.08
MDES (percentage point difference)		0.019
Baseline measures		0.06
Intracluster correlations (ICCs)	Local authority	0.00722
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 per year)		3,400
Average cluster size (children per local authority across all time periods)		10,200
Number of years		3
Sample Size (children)	Total	51,000

While power calculations ex ante for a DiD analysis have their shortcomings especially in terms of the precision of the calculations, the calculations above highlight a potential risk of being underpowered for this analysis.[15] The main analysis will use a feasible GLS estimation, an approach which has been shown to increase power, to mitigate this risk. The results of the MDES calculations will be taken into account in the triangulation of the results and discussed accordingly in the final report.

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[14] Iacus, Stefano M., Gary King and Giuseppe Porro. 2011. "Multivariate Matching Methods that are Monotonic Imbalance Bounding." Journal of the American Statistical Association 106:345–361. http://gking.harvard.edu/files/abs/cem-math-abs.shtml.
[15] Please note that the power calculations are based on the commonly used approach for clustered difference-in-differences designs, but does not fully take into account the staggered roll-out of the programme. This means that the MDES might potentially be underestimated.

Outcome measures

For the trial we will evaluate one primary outcome measure and five secondary outcome measures. Individual-level data will be collected from the ONS' National Pupil Database (NPD) via the Secure Research Service (SRS). Local authorities will not be involved in the data collection for the DiD analysis. Below we give an explanation and rationale of the outcomes outlined in the Design Table. In the instance of any unintentional inconsistencies, the above table definitions should take precedent in the analysis.

This analysis does not consider the research question on care proceedings that the stepped wedge RCT evaluates. This is due to a lack of data on care proceedings in the NPD dataset. We further exclude the measure of repeated statutory services as the required timeframe to observe this outcome will be too long to meet reporting deadlines. This outcome measure will only be reported on in the stepped wedge RCT.

Primary outcome measure
Whether or not the child has become looked after
To answer research question 1, we will analyse whether children (aged 0 - 12 who are referred to Children's Social Care within the trial period) are more or less likely to become looked after within 18 months of starting the referral where Family Safeguarding had been implemented, compared to when it had not been. The outcome measure is a binary variable, indicating whether or not a child that is in our sample (defined above) has become looked after at any point within 18 months of their first referral in the trial period.[16]

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

CPP plan duration
To answer research question 3, we use two different models. First, we will use a discrete variable measuring the number of days that the child has been on a single CPP, over a period of 24 months from the start of the CPP and censored for larger values.[17] Our sample will include any child aged 0-12 who has had a referral within the trial period that led to a child protection plan (CPP) within 12 months of the referral start date.

Days on CPPs
Second, we will use a discrete variable measuring the number of days spent on CPPs in days. In this case we will use our full analytical sample, i.e. all children that have been referred within the trial period. Thus any children who are not on any CPPs for the time frame of 36 months since the initial referral will be coded 0. We will record days spent on any CPP within that period i.e. multiple CPPs will be considered if applicable. Again, values over 36 months will be censored.

The potential reasons for changes in the time children spend on child protection plans are numerous. Spending longer on a CPP could be an indicator of children's social care

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[16] 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 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.
[17] that is, any values over 24 months will be coded as 24 months.

providing additional support to a child and family so as to meet their needs. However, it might also be a sign of a child and family not having their needs adequately addressed in a timely manner. Thus it is hard to unambiguously interpret changes in this measure as either good or bad. Results will require careful interpretation in combination with the other outcome measures, and findings from the accompanying implementation and process evaluation.

Repeat referrals for domestic violence, mental health or substance misuse
To answer research question 4, the outcome measure is a binary variable of whether or not a child that has previously been referred for parental substance misuse, domestic violence, or parental mental health has been re-referred to children's social care within 18 months of the initial referral start date. Our sample will be restricted to children aged 0-12 at the time of referral who have been referred within the trial period and whose initial assessment identified parental substance misuse, domestic violence, or parental mental health as factors identified at the end of assessment.[18] Since these factors are only identified at assessment, our sample is restricted to children whose referral has progressed to an assessment and where one of the factors identified at assessment includes one of the three factors defined above.[19]

Since a key element of Family Safeguarding is the inclusion of specialist adult practitioners with domestic abuse, mental health and substance misuse expertise, this analysis will allow us to evaluate one of the main components of the Family Safeguarding programme more closely.

Unauthorised school absence rates
To answer research question 5, the outcome measure is a continuous variable measuring the percentage of sessions missed by a child within our analytical sample due to unauthorised absence, out of all sessions the child was expected to attend. We will measure the school attendance rate of three consecutive school terms, beginning with the closest school term beginning after the start of the period in which the child entered our sample. This will involve having up to three measurements per young person.

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 outcomes outside of children's social care. 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 Safeguarding.

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 CPPs 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

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[18] Note that this does not include cases that lead directly to a Multi Agency Risk Assessment Conference (MARAC).
[19] Note that the cases do not need to be open after assessment.

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.

Analysis plan

Primary Analysis:

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

$$Y_{iat} = \beta_0 + \beta_1 FS_a + \gamma X_{iat} + \rho Z_{at} + \theta Year_t + \delta LA_a + \varepsilon_{iat}$$

Where

- Y_{iat} is a binary indicator that equals 1 if child i in local authority a entered care within 24 months of their first referral in time t in the trial period, and 0 otherwise.[20]
- FS_{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 Safeguarding (and 0 if before, or the local authority does not implement Family Safeguarding).[21]
- 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
- $Year_t$ are year dummy variables to capture time trends common to all authorities for each financial year.
- LA_a are LA fixed effects to capture average time invariant differences between local authorities
- ε_{iat} are the heteroskedasticity-robust standard errors at time t for individual i, clustered at the level of the local authority (the level at which assignment takes place).

The unit of analysis is at the individual level to optimise the power to detect an effect within the constraints of the project. To account for serial correlation in our data and to increase power, we will use feasible GLS estimates employing a random effects model that accounts for cluster-robust estimates[22].

We will judge the statistical significance of the treatment effects applying a significance level of 5%. Due to the small number of clusters, we will employ a robust inference technique and bias corrections suggested by Brewer et al. (2013) that produce correctly sized tests even with few groups.Our sensitivity analysis will consider different evaluation approaches that are discussed in detail below.

DiD involves multiple observations of the control and treatment groups over time. This can create serial correlation where errors in one period transfer to another. Serial correlation can be a particularly important issue in the DiD context because of the inclusion of fairly long time series. Dependent variables are typically highly positively serially correlated, and the treatment variable changes little within a local authority over time. Serial correlation usually underestimates the standard errors, meaning that we have too much confidence in the result,

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[20] Population as described above.

[21] Children can only occur once in our evaluation, i.e. that we consider the first referral.

[22] See Cameron & Miller (2015): A Practitioner's Guide to Cluster-Robust Inference. We use a linear regression if the baseline rate of our outcome is between 5 and 95%. If the baseline rate is outside of that range, we employ a logistic regression instead, as this model typically fairs better for binary outcomes with extreme baseline rates. We will consider conditional logit models to overcome the incidental parameters problem.

and we get a higher rate of false positives. We check for serial correlation using the Breusch-Godfrey test.[23] Serial correlation is partially taken care of when clustering at the local authority level.[24] To counteract individual-level serial correlation, we also try individual level fixed effects and random effects models with standard errors clustered at the local authority level if the share of individuals that occur repeatedly in our sample is larger than 15 percent. We choose between the fixed effects and random effects model using a cluster-robust Hausman test. We then consider whether the chosen model produces a statistically different and practically different estimate of the treatment effect to the treatment effect estimated from the main regression (where we take practically different to be 20% above or below).[25]

Covariates

In order to increase the precision of our estimates, we include the following individual-level covariates, gathered at the point of referral[26] and local authority covariates (where they are available) gathered from the most recent time point preceding the point of referral.

Vector of individual level covariates of the child or young person
- Gender (included as a binary indicators: 0=Not recorded/unborn, 1= male, 2=female, 3=indeterminate, 4=Missing)
- Ethnicity[27]
- Age at referral
- Disabled status[28] (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)[29]
- Is child an Unaccompanied Asylum Seeker[30] (included as a binary indicators, 0=No, 1= Yes)
- Number of previous child protection plans

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[23] We consider there to be serial correlation if the p-value of the Breusch-Godfrey test is below 0.05.
[24] Clustering at a group level (not group-year level) seems to account well for serial correlation when there are a sufficient number of groups (p.17). Bertrand, M., Duflo, E. & Mullainathan, S. 2004. "How Much Should We Trust Differences-in-Differences Estimates?," The Quarterly Journal of Economics, MIT Press, vol. 119(1), pages 249-275, February https://www.nber.org/papers/w8841.pdf
[25] If the sub-population of individuals that repeatedly occur in one sample is less than 15% of all observations, and/or the descriptive statistics of this sub-group suggest it is very different from the full sample, we will refrain from running multilevel models due to concerns regarding comparability.
[26] For time varying individual-level covariates, we use the latest entry at or before the start of the referral (e.g. age at referral). For time invariant covariates, we conduct checks to see whether the covariate unexpectedly changes over time, which would suggest data quality issues. For the variables that cover disabled status, free school meal eligibility and unaccompanied asylum seeker, we choose the maximum value, i.e. if there is any indication that the child fulfills one of these statuses, we accept the child as being in this category. For gender and ethnicity, we convert the classification for any child where there is more than one category over time (e.g. child recorded as male in one referral, as female in another referral) as missing. If there are two different values over time but one of them is coded as "Missing", the other value will be used for all entries).
[27] In the categories defined in the DfE's CIN census.
[28] 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.
[29] We use Pupil Premium Eligibility for the first three years as every child is eligible for free school meals during this period.
[30] UN High Commissioner for Refugees (UNHCR). (2013, March). *UNHCR's Engagement with Displaced Youth*. https://www.refworld.org/docid/5142d52d2.html, p28.

- 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).[31]

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[32]
- Proportion of children / young people eligible for Free School Meals (continuous variable based on all children in our sample)
- Proportion of children / young people white British (continuous variable based on all children in our sample)
- Presence of other Innovation Programmes - if the authority used programmes additional to Family Safeguarding that had similar aims or that induced whole system change (e.g. Signs of Safety) (coded as binary variables)

The data will be sourced from a variety of data sources (Characteristics of Children in Need Tables, LAIT, Ofsted reports, aggregate measures of individual-level data requested from LAs).

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 treatment 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 multiple imputation where data is missing experimentally at random.

Note that for local authority level covariates, we use linear interpolation rather than multiple imputation. The local authority level covariates differ from the individual level data in that they (a) only differ across local authorities and (b) are also time series data. It is difficult to imagine that the individual-level covariates will predict local authority level covariates such as e.g. staff turnover appropriately. We further have a short time series (making it harder to use any time series techniques) and the data is often missing at the beginning of our time series. For these reasons, we choose to use linear interpolation and last observation carried forward/next observation carried backwards for missing start/end values.

Secondary Analysis

For all binary secondary outcomes, namely return to statutory services, progression to care proceedings, and repeat referrals for parental substance misuse, domestic violence or parental mental health as defined in the Design Table above, we will choose between a logistic regression and a linear regression using the same decision criteria as for the primary outcome.

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[31] This covariate will not be included for the primary outcome and outcomes on re-referrals since the existence of a need code in the records is an indicator of the outcome of a child's referral and which is not known at the date of the referral.

[32] 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.

For the secondary outcomes CP plan duration, days on CPPs, and unauthorised school absence rates, we will use a linear probability model. For these outcomes we will employ the same specification as for the primary outcome. In the case of unauthorised school absence rates where we will measure children 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 to reduce the risk of finding significant results by chance.

Sensitivity Analysis

Definition of treatment and comparison group

We adopt a conservative approach in our primary analysis and define any child as part of the comparison group whose local authority had not implemented Family Safeguarding at the start date of the first referral within the trial period. This will most likely underestimate the treatment effect, since children in the comparison group might have been in contact with Family Safeguarding at a later stage of the referral.

To analyse the magnitude of the treatment effect further, we run additional regressions using different treatment and comparison 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.

Differential time effects

We do not consider time effects such as embedding periods in our primary analysis. This is because we only have a limited window of observing post implementation outcomes for the local authorities that implement Family Safeguarding as one of the last wedges to go live. It may be that Family Safeguarding 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 Safeguarding has been implemented in the local authority. The regression specification is:

$$Y_{iat} = \beta_0 + \sum_{m=1}^{M} (FS_{iat} \cdot T_m)\, \beta_m + \gamma X_{iat} + \rho Z_{at} + \theta Year_t + \delta LA_a + \varepsilon_{iat}$$

where T_m is a binary indicator that equals one if the observation is from a local authority that has been implementing Family Safeguarding for m periods (with s being the first period after implementation), and otherwise 0. The coefficients on the interaction effect will shed light on whether authorities experience increasing treatment effects the longer they run Family Safeguarding.

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 Safeguarding.

Decomposition

Since the go-live date of the Family Safeguarding model differs by local authority, the "treatment timing" is staggered. In such staggered settings, the treatment estimate has a risk of bias if there is a heterogeneity in treatment effects over time. Heterogeneous treatment effects over time are likely in our setting, since local authorities will still increase implementation and get used to new ways of working after the go-live date. This can potentially lead to a larger effect of Family Safeguarding on outcomes the longer the model has been implemented. To account for this risk of bias, we will run an additional sensitivity analysis using a decomposition put forward by Goodman-Bacon (2018) and will consider approaches such as the one put forward by Callaway and Sant'Anna (2020) if the decomposition suggests a significant risk of bias.

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 if the data exhibits a disproportionately high number of zero counts[33].

Since we expect the number of censored data points in our time spent on CPP outcome measure to be reasonably small[34], 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.

Triangulation of results

Since we will conduct an analysis exploiting the stepped wedge design of the implementation as well as a DiD analysis, results will have to be triangulated to reach a conclusion of the impact evaluation of Family Safeguarding. In the case that both evaluations align it will provide robust evidence of the potential impact of Family Safeguarding. In such a case, we will reach an average estimate of the impact of Family Safeguarding by pooling the two treatment effects to arrive at a single coherent estimate.

If however, the results diverge, care will have to be taken to draw adequate conclusions. We are conducting two types of analysis simultaneously and both have methodological challenges which will be affected by the roll-out of the programme and the ability to find suitable matches. If the assumptions underlying each quantitative method only hold for one of the approaches, we will rely primarily on these results to assess the models' impact. If the assumptions hold for both approaches, we will try to identify what accounts for the observed

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[33] This will be determined by comparing the number of zero values that are in the dataset to the number of zero counts you would expect to observe using the traditional model's probability distribution.

[34] We consider time spent on CP plans up to a length of 18 months. Based on official statistics, the share of CP plans that lasted longer than 2 years is on average at approximately 3% across England. (Children in need statistics). Consequently we expect the share of censored data points to be at a relatively higher level.

differences in results and will take these considerations into account when drawing conclusions.

Cost Benefit Analysis

Overall approach

We will evaluate the financial benefits and direct costs to local authorities of implementing the programme. We recognise that there may be other (social) benefits of the programme (e.g. to children who did not come into care) but this will not be the focus of our analysis. We will look at the costs and benefits over the entire observation period and will consider benefits based on our impact evaluation and actual costs, excluding any prerequisites. To quantify the benefits of the Family Safeguarding programme, we will consider the cost savings for a local authority through fewer children coming into care. This will be based on a triangulation of literature and best practices. We will report a benefit cost ratio and the net present social value of the programme.

Benefits

Our main analysis focuses on the effects of Family Safeguarding on children's social care outcomes. We will triangulate the found treatment effect for the primary outcome from the DiD and stepped-wedge RCT analysis as detailed in the previous section.

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 (the primary outcome). This will be informed by the coefficient of our primary analysis and average cost estimates per looked after child. Monetised benefits will be calculated as follows:

Total un-monetised benefit per LA = average treatment effect[35] x average number of children in the sample per year per local authority

Total monetised benefit/LA= $\sum_{i=0}^{5}$ $Total\ unmonetised\ benefit * £benefit/person * discount\ factor_i$

The discount factor will deflate benefits to their corresponding value in the base year. The benefit per person will be determined by triangulating existing research on the savings associated with a child not going into care. This will be based on the weighted average cost of a child going into care by placement type.

We will only compute benefits of the programme if the point estimate of the corresponding regression is statistically significant. Note that this will focus on the savings realised by the (average) number of cases where children that were involved with statutory services did or did not not go on to become looked after due to Family Safeguarding. We will also gauge cost savings in other areas of children's social care measured by the secondary outcomes in our main analysis should the effect estimates be statistically significant.

There are a range of benefits that we will not monetise but that we will take into consideration when discussing the cost effectiveness of the intervention. These include effects on staff workload and wellbeing, outcomes for the wider family network and improved relationships. These benefits will be discussed taking into consideration the findings of the implementation and process evaluation in particular.

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[35] This is the treatment effect coefficient of the main regression in the primary analysis.

Cost components

To estimate the actual costs of the programme, we will share an online survey with designated leads at all participating local authorities. We will measure the categories below, and where possible identify whether these are prerequisites, start-up (one-off) costs or recurring costs. Where possible we will also identify whether there is overlapping use or prolonged life use of any goods. We will seek to establish actual rather than intended costs, by collecting this data from people involved in the study. However, where this data is not forthcoming, we will need to rely on the forecast or anticipated costs.

The cost data will include:

1. Personnel cost for the implementation of the programme, i.e. how much local authority staff time is used for delivery of the programme that required backfilling positions or hiring additional staff, and for which staff roles - time required * average salary for this staff role
2. Training costs (both personnel costs[36] and any fees/license costs incurred)
3. Programme costs, e.g. fees and costs for programme components
4. Facilities, equipment and materials e.g. resources, printed materials, office supplies, computers, software, premises costs
5. Potential unintended consequences (e.g. an increase in the number of children on child protection plans, based on the findings of the full analysis) as identified in the logic model
6. Other programme inputs or hidden costs

Similar to the monetised benefits, costs will be deflated to the value in the base year. Personnel costs will be estimated by multiplying the number of hours by a typical hourly wage for an employee at the local authority in that role. The final cost estimate will be the sum of all costs listed above, discounted with respect to when they were incurred, averaged across all five local authorities.

We will seek to establish an overall cost of the programme and put the overall cost in context to the provided funding. We will seek to establish actual rather than intended costs, by collecting data directly from people involved in the study. However, where this data is not forthcoming, we will need to rely on the forecast or anticipated costs. This will be based on total cost to local authorities if they were to implement the intervention independently of funding and evaluation.

Timeline

Activity	Timeframe
First LA implements Family Safeguarding	September 2020
Final LA implements Family Safeguarding	April 2022[37]
Observation period for the final participants from the population sample ends	March 2024

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[36] E.g. hiring a trainer or hiring agency staff to cover the staff on training.
[37] Estimated date, subject to changes due to the implications of COVID-19

Data collected via ONS[38]	March 2025
Analysis (DiD) and triangulation of results between all three strands of analysis	2025/26
Reporting	2026

Ethics & Participation

We maximise the benefit of the evaluation by providing an additional lens to understand the impact, in particular getting closer to a causal estimate of the impact of Family Safeguarding, which is informative for local authority decision-making as to whether or not to invest in Family Safeguarding. We believe the risk of harm is very low. 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 analysis does not involve innovative technology, denial of service, large-scale profiling, biometric data, genetic data, data matching, invisible processing, tracking or targeting of individuals for marketing purposes. The outputs will be presented as summary statistics and will be checked for statistical disclosure.

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 mitigate the risk of a data breach by using the ONS' secure research service (SRS). Data will be stored on the ONS' systems. Access to the data will be limited to the project team at WWCSC; all researchers have undergone rigorous data protection training. It is very unlikely that the data requested will enable re-identification because we only ask for the data necessary to undertake the analysis and this contains no "instant identifiers" (e.g. name, address etc) or "meaningful identifiers" (which would allow matching to other datasets with more information).

The trial protocol has undergone an ethics review by a member of WWCSC's Evaluation Advisory Board.

Registration

The trial will be pre-registered on OSF (Open Science Framework, https://osf.io/) run by the Centre for Open Science (https://cos.io/).

Data protection

The underlying data used to conduct this analysis consists of administrative data from local authorities in England, five of which are funded by the Department for Education (DfE) to introduce Family Safeguarding as part of the Strengthening Families, Protecting Children programme. This excludes the Trailblazer local authority. All processing activities carried out

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[38] The DfE's individual-level statistics on the CIN and CLA census become available to researchers with a one year lag, e.g. the statistics on children in need from the April 2020-March 2021 year will be available from March 2022 onwards.

to conduct this analysis will abide by the Data Protection Act 2018[39]. We have conducted a full data protection impact assessment (DPIA) and have published a privacy notice on our website[40].

The data subjects are children who have been referred to children's social care. The personal data processed includes demographic details, and quantitative measures of their children's social care status (e.g. whether or not they become looked after, or are on child protection plans). Special category data is included, specifically ethnicity, and disability status (which we count as health data).

The data is owned by the DfE and will be made available via the ONS' Secure Research Service (SRS)[41]. Data will not be stored on WWCSC systems but on the ONS systems, which are highly secure and controlled by the ONS. Access will only be granted to research team members being part of the wider project team, and who are ONS accredited, and have undergone training and assessment. When access is granted to the ONS accredited researcher the data requested will be transferred to a secure "research instance" within the SRS. This "research instance" of the SRS is the location the data will be housed and used for the duration of the evaluation.

Once the project evaluation has completed the evaluator will request for the data to be moved from the "research instance" in the SRS to the WWCSC data "archive instance" which is also housed in the SRS.

The legal basis for processing the data is legitimate interests - details of how we pass the three part test are contained in the privacy notice linked to above. We are processing special category data on the condition that it is necessary for archiving, scientific, historical research or statistical purposes. This is because processing this special category data will help ensure our research is as accurate and informative as possible

For the purposes of the evaluation DfE and WWCSC are the data controllers, and the ONS are data processors. Data about individuals requested via the ONS secure research service will be pseudo-anonymised. 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). The ONS explicitly de-identifies the data, and provides meaningless identifiers that allow tracking individuals over time and linking necessarily data sets provided, but only the ONS would be able to link back to meaningful identifiers (such as the Unique Pupil Number). It is extremely unlikely that any researcher accessing the data would be able to identify any individuals in the data, and no attempts to do so will be conducted.

Prior to any results being extracted from the ONS SRS environment, they undergo checking for statistical disclosure, first by WWCSC staff, then by ONS staff. Further checks will be conducted by WWCSC staff prior to publication of any reports.

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[39] 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/
[40] https://whatworks-csc.org.uk/research-project/family-safeguarding-model-trial-evaluation/
[41] Details of the SRS can be found here: https://www.ons.gov.uk/aboutus/whatwedo/statistics/requestingstatistics/approvedresearcherscheme

For the purposes of having the data held in the WWCSC data archive WWCSC are the data controllers and the ONS are Data Processors. WWCSC reviews any researcher requests for access to the data housed in the WWCSC data archive in the SRS. WWCSC maintains an Archive Access Approval Panel who reviews all access and research project requests for the further use of the data once in the data archive based on criteria in keeping with the essence of the DfE grant funding letter to WWCSC.

Upon completion of the evaluation the lawful basis WWCSC, as sole independent controller, shall rely on, for the purpose of archiving and any subsequent secondary analysis of the data, GDPR Article 6.1(e), and GDPR Article 9.2(j) and DPA18 Schedule 1 Part 1.4(a),(b)&(c) for special category data including data considered to be a protected characteristic under the UK Equality Act 2010.

Data archived within the WWCSC instance of the Office for National Statistics Secure Research Service ("ONS SRS") for the purposes of secondary research on the data within this evaluation shall be non-identifiable data and governed under the UK Digital Economy Act 2017 and the UK Statistics and Registration Service Act 2007.

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 is further outlined in its IT Usage and Data Protection policies.

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 being 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. All WWCSC researchers accessing the data will have undergone the ONS SRS's accreditation system, including receiving relevant training and assessment.

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). Data collection, analysis and reporting will be led by Eva Schoenwald (Senior researcher, WWCSC).