

Evaluation of Family Drug and Alcohol Courts

Intervention Developer	Family Drug and Alcohol Courts in England
Delivery Organisations	Family Drug and Alcohol Courts in England
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Type of Trial	Quasi-experimental design: Coarsened Exact Matching
Age or Status of Participants	Parents in care proceedings; Children of Parents in care proceedings
Number of Participating Local Authorities	14 FDAC sites and 12 Local Authorities (originally planned 31 Local Authorities)
Number of Children and Families	600 children (300 intervention; 300 control): 430 families (215 intervention; 215 control)
Primary Outcome(s)	Reunification
Secondary Outcome(s)	Parental alcohol and drug misuse cessation, number of contested hearings, use of expert witnesses
Contextual Factors	COVID-19 Study redesigned to take account of Local Authority resourcing challenges and non-FDAC data availability

Study Plan version history

VERSION	DATE	REASON FOR REVISION
1.0	17 Dec 2020	Original version
2.0	1 Nov 2022	<ul style="list-style-type: none"> ● Added section about variations from original evaluation ● Updated number of FDAC sites and Local Authorities included in the study (Table 1) ● Updated Identification strategy section: changed severity of alcohol and drug misusing into type of misuse due to non-FDAC data availability ● Updated Outcome measure section: added an additional analysis of the number of different types of expert witness to deal with Research Question 5 ● Updated Matching section: updated Table 3 on matching variables; coarsening strategy for child, parent and case-level outcomes; and variables to be used by children-, parent- and case-level (details on changes can be tracked by comparing previous version of Table 3 “Intended variables for matching” in Appendix C) ● Updated Analysis plan section: changed model specification into a multilevel logistic regression to account for clustering effects; added a multilevel Poisson regression model to explore the difference between FDAC and non-FDAC cases in the number of different types of expert witness consulted during care proceedings. ● Updated Data source and availability section, in accordance with non-FDAC data availability ● Added new table listing misalignment between FDAC and non-FDAC data, along with derived variables to harmonise the data (Table 5) ● Updated Ethics & Participation section: regarding local authorities having to send out opt-out letters to parents receiving non-FDAC care proceeding ● Updated personnel list and timeline ● Expanded the date window for eligibility for study inclusion to maximise our sample size <p>Minor changes:</p> <ul style="list-style-type: none"> ● Used consistent terminology for child-, parent-, and case-levels of data ● Refer to the comparison group as non-FDAC care proceedings as usual, to match the language used with LAs ● Simplified the power analysis table to remove clusters with ICC = 0. (No change to calculation)

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Background and Problem Statement

In 2008 the Government launched its family drug policy strategy, which aimed to prevent intergenerational harm because of parental substance misuse (HM Government, 2008) and to improve outcomes for children affected by parental substance misuse (Harwin & Ryan, 2008). The first UK Family Drug and Alcohol Court (FDAC) was set up in London in 2008 as a three-year pilot funded by central government in the Central London Family Proceedings Court. The London FDAC was followed by sites in Milton Keynes and Buckinghamshire (2014) and East Sussex (2015).

The basic criterion for FDAC referral is that parental substance misuse (drugs or alcohol or both) is a key factor of the Local Authority's concerns about child(ren) within a care proceedings case. FDACs aim to improve outcomes for children and families by providing an alternative way of working with parents involved in care proceedings in relation to alcohol and drug use. The primary aim of FDAC is to ensure that a child can stay with parents or be reunified at the end of care proceedings if it is safe to do so. If reunification is not possible, then the aim is to find an alternative carer for the child swiftly, to give the child the best chance for permanency and stability. FDAC also aims to stop parents from misusing alcohol and drugs, to make the home environment safer, and to reduce the risk of future instances of care-proceedings.

FDACs use a 'problem-solving' court approach to justice, whereby courts use their authority to help address the complex social issues that bring people before them (Harwin & Ryan, 2008; Roberts et al., 2017). FDACs encourage parents to believe recovery and change are possible, along with aiming to provide a realistic understanding of the challenges they face. Specialist, designated judges provide parents with regular supervision and support through fortnightly court reviews. A specialist multidisciplinary team also works closely with the courts and parents to support families to change and overcome their alcohol and drug misuse problems and other difficulties.

Children and Families Act 2014

The Children and Families Act 2014 made several substantive changes to the implementation of care proceedings. The changes most relevant to FDAC relate to the use of experts in care proceedings and the introduction of new limits on the duration of care proceedings. S13 restricted the use of experts as these delayed cases. S14 introduced a 26-week limit on the length of care proceedings, though extensions can be granted in some circumstances.

The National Unit

In April 2015, the Department of Education's (DfE) Children's Social Care Innovation Programme supported the Tavistock and Portman NHS trust and adoption charity Coram to create the FDAC 'National Unit' to scale up the intervention. The National Unit supported nine FDAC sites and closed in September 2018. Further information about the National Unit's implementation is explored in Roberts et al. (2017).

Effectiveness

Early evidence about FDAC was promising. Harwin et al. (2011) found that FDAC parents were more likely to stop misusing alcohol and drugs, and more likely to be reunified with their children relative to a comparison group. This study also provided some evidence that FDAC could provide cost savings by using fewer experts relative to non-FDAC care proceedings as usual.

The 'After FDAC: outcomes 5 years later' study, found that a higher proportion of mothers in FDAC abstained from drugs or alcohol over the five-year follow-up, relative to comparison mothers (Harwin et al., 2016; Harwin et al., 2018). It also found that a significantly higher proportion of FDAC than comparison mothers who had been reunited with their children at the end of proceedings experienced no disruption to family stability at three-year follow-up. Whilst the study compared intervention outcomes to a comparison group, the comparison group was drawn from care proceedings as usual where alcohol or substance misuse was a factor in issuing care proceedings. The study did not construct a counterfactual using other factors, which may have made for a stronger comparison (e.g. by using matching). In addition, this study drew on a relatively small sample size (140 intervention cases; 100 comparison cases).

A study of the London FDAC found that a higher proportion of parents whose case was heard in FDAC had ceased misusing alcohol and drugs by the end of proceedings, and more FDAC than comparison families were reunited with their children. Additionally, proportionately fewer children in FDAC families experienced new neglect or abuse in the first year following reunification (Harwin et al., 2014).

Variation between sites

FDACs also deliver their support differently across sites. All sites deliver support during care proceedings, but some sites (such as Gloucestershire) also offer pre-proceedings or post-proceedings support. Some sites offer peer-mentoring, and overall staffing varies between sites. Additionally, some teams are embedded within Local Authorities (such as Gloucestershire) whilst other multidisciplinary teams are commissioned services delivered by external providers (such as London).

Variation also exists between sites when there is subjective decision making (such as deciding which cases to offer support out of a sample of cases that meet the inclusion criteria).

Rationale for further evaluation of FDAC

Prior evidence suggests that FDAC is a promising intervention for children in families with alcohol or drug misuse. Yet much of the prior evidence about FDAC's effectiveness comes from the London FDAC site. The evidence may also be further strengthened by assessing impact with a stronger counterfactual and with larger sample sizes (to detect smaller effects). Furthermore, these evaluations assessed the effectiveness of FDAC before the reforms introduced by the Children and Families Act 2014, which changed how care proceedings are administered. A separate feasibility study was therefore carried out to establish a research design to evaluate the impact of FDAC on outcomes for children and families across FDAC sites and in light of legislative reform. The design of the current study is the result of this feasibility study.

Variations from original evaluation design

The original impact evaluation design included 14 FDAC sites that accept referrals from 31 Local Authorities. Our plan was to collect FDAC cases from the FDAC sites, and non-FDAC cases from Local Authorities. However, due to disruptions caused by the COVID-19 pandemic (March 2020 – ongoing), Local Authorities expressed resourcing constraints in providing non-FDAC data for this evaluation. As a result, 12 Local Authorities have now agreed to be part of this evaluation and collect and provide data on non-FDAC cases. We

report the list of participating Local Authorities in Table 1. After consultation with WWCS, we decided to include cases that were live between October 2019 and June 2022 and collect data at one point in time and not at four as previously planned.

Due to resourcing constraints faced by the Local Authorities and difficulties providing data to us, we consulted with them to determine for which data fields it would be feasible for them to provide data, we decided to include a reduced set of matching variables than originally planned. This decision was made after consultation with WWCS and after we distributed a survey to Local Authorities, asking about whether particular variables exist in information they have direct access to. A revised list of the matching variables is now reported in Table 3. Originally intended matching variables' table can be found in Appendix C. In response to changes made to matching variables, we also updated the identification strategy and model specification, depending on the level of outcome measures. Since not all Local Authorities that operate FDAC sites agree to provide non-FDAC data (as shown in Table 1), we will use multilevel models to deal with the hierarchical structure.

In addition to the analysis of binary indicator of the use of expert witness, we will include the total number of *different* types of expert witness as an additional outcome to explore the impact of FDAC on the likelihood of expert witnesses being consulted (RQ5).

The final report will include a section on lessons learned, e.g., concerning the reasons for changes made above, such as the availability of data and resources to code this data in Local Authorities.

Intervention and Theory of Change

This section outlines how the intervention is delivered and the Theory of Change (ToC) that was developed with stakeholders during the feasibility stage of this evaluation. Although there is variation in the elements included in FDACs and how they are implemented, the ToC is designed to outline the overarching logic common to the FDAC approach in general. The FDAC logic model and ToC are detailed in Appendices A and B respectively.

Intervention

FDAC provides support to parents to help them overcome their problems to give children the best possible chance of being raised by their own parents. FDAC recognises that very few parents intend to abuse or neglect their children, but that parents fail when they have significant problems. This includes substance and alcohol misuse, domestic abuse, mental health problems and severe poverty.

FDAC is designed to be a 'problem solving' court that adopts a less adversarial approach than typical care-proceedings. It follows the principle of therapeutic jurisprudence, empowering families with a stronger voice in care proceedings.

How is the intervention delivered?

Each FDAC site has a dedicated FDAC judge. The FDAC judge has jurisdiction over both care-proceedings and the FDAC treatment intervention. The FDAC judge oversees fortnightly reviewing hearings with the multidisciplinary team. Lawyers do not attend the fortnightly review hearings. The multidisciplinary team provides treatment and support to parents, monitoring their progress and reporting back to the court at the fortnightly review hearings. These hearings aim to solve the problems faced by the parent through an open therapeutic forum.

The staffing of the multidisciplinary team varies across FDAC sites, though the core structure includes substance misuse specialists, social workers and an overall site manager.

When and where is the intervention delivered?

The intervention is delivered during care-proceedings, which typically last up to 26-weeks. In some circumstances, extensions to care-proceedings are granted.

There are fourteen FDAC sites, operating within Local Authorities. A full list of FDAC sites and the Local Authorities each site covers is detailed in Table 1, along with 12 Local Authorities who agreed to provide non-FDAC data as part of this evaluation.¹

Table 1 FDAC sites and Local Authorities	
FDAC site	Local Authority
Pan-Bedfordshire	Central Bedfordshire*
	Luton
	Bedford
Birmingham and Solihull	Birmingham City*
	Solihull*
Coventry	Coventry*
	Warwickshire*
East Sussex	East Sussex
Gloucestershire	Gloucestershire
Kent	Kent*
Leeds	Leeds City Council*
London	Bromley
	Camden
	Croydon
	Kingston
	Lambeth
	Merton
	Redbridge*
	Richmond
	Sutton
	Wandsworth
Milton Keynes and Buckinghamshire	Milton Keynes
	Buckinghamshire
Newcastle, Gateshead & North Tyneside	Newcastle
	Gateshead
	North Tyneside
Somerset	Somerset
Southampton	Southampton City Council*

¹ Only one Local Authority who does not operate an FDAC site agreed to provide data.

Stockport	Stockport*
Walsall, Sandwell and Dudley	Sandwell
	Dudley
	Walsall*
Not operating FDAC site	Manchester*

* indicates the 12 Local Authorities that have agreed to provide data as part of this evaluation.

Variation and iterations

Although the core FDAC model is the same, there are some differences in the implementation of the intervention across sites. For example, some sites have started providing support in pre-proceedings (London and Kent) or post-proceedings (Gloucestershire). Some sites also use peer-mentoring, where successful parents support parents in care-proceedings. This study focuses on the overall effectiveness of FDAC across sites.

Separate evaluations are being conducted on behalf of the What Works Centre for Social Care which will explore these variations in greater detail:

- Peer-mentoring; evaluated by King's College London.
- Post-proceedings support in the Gloucestershire FDAC; evaluated by the University of Sussex.
- Engagement with FDAC using behavioural insights; evaluated by the Centre for Evidence Implementation.

Impact Evaluation

Research questions

The impact evaluation will seek to answer the following questions:

RQ1 What is the impact of FDAC on the likelihood that children are reunited with their parents at the end of care proceedings relative to non-FDAC care proceedings as usual?

RQ2 What is the impact of FDAC on the likelihood that parents continue to misuse alcohol or drugs by the end of care proceedings relative to non-FDAC care proceedings as usual?

RQ3 What proportion of children reunified at the end of FDAC care proceedings are still placed with their parent(s) three years after final court hearing and how does this compare with the national average?

RQ4 What is the impact of FDAC on the likelihood of final care proceedings hearings being contested relative to non-FDAC care proceedings as usual?

RQ5 What is the impact of FDAC on the likelihood of external expert witnesses being consulted during care-proceedings relative to non-FDAC care proceedings as usual?

RQ6 What is the impact of FDAC on the placement of the child at the end of care proceedings relative to non-FDAC care proceedings as usual?

Participants

Study participants will be drawn from 14 FDAC sites (for the intervention group) and from 12 Local Authorities (for the control group). Participants in the intervention group will be those

going through FDAC court proceedings. Control cases will be selected from non-FDAC care proceedings as usual cases.

Eligibility for study inclusion² is defined as the following:

- **Intervention** – all cases that have been referred to an FDAC that were open between October 2019³ and June 2022 and a final hearing took place by October 2022 will be considered eligible.
- **Control** – any case that meets the basic criteria for an FDAC referral and sits within an area covered by a Local Authority⁴ that has an FDAC but receives non-FDAC care proceedings as usual that were open between October 2019 and June 2022 and a final hearing took place by October 2022 will be considered eligible.

The basic criterion for FDAC referral is that “Parental substance misuse (drugs or alcohol or both) is a key factor of the Local Authority’s concerns about the child(ren) within a care proceedings case”. This will be captured for FDAC cases from a suite of variables, including:

- Current or historical misuse of drugs or alcohol
- Substance misuse type
- Severity of alcohol misuse
- Severity of drug misuse

This data is not systematically collected by Local Authorities for non-FDAC cases. Instead, care proceedings case data is typically recorded in court bundles and case notes. In June 2022, we distributed a survey to Local Authorities to determine which data fields for non-FDAC cases exist in their records, whether Local Authorities have direct access to that information, and whether it would be feasible for them to provide data for those fields. Based on their responses, we adjusted the data collection template and revised the study protocol. Some FDAC sites have developed their own referral inclusion and exclusion criteria though the variation between sites has never been closely documented. For instance, some FDAC sites exclude cases where parental psychosis or litigation capacity may act as a barrier to parental engagement with FDAC or where there is a history of severe physical or sexual abuse of a child. Furthermore, the selection process likely varies by FDAC site in regard to subjective components that are not included within the criteria. There is therefore likely to be a degree of selection bias, with systematic differences between control and treatment participants.

FDAC case data will be retrieved directly from CJI following the development of a data sharing agreement. This data sharing agreement should be sufficient to enable access to all necessary FDAC data for the purposes of the study. Alternatively, however, separate data sharing agreements may have to be drawn for each of the individual FDAC sites. Further data sharing agreements will also be drawn for each of the participating Local Authorities in order to grant access to the necessary control case data.

Design

Table 2 Study design

² Note that this refers to RQ1, 2, 4 and 5. RQ3 draws on data from a prior cohort of FDAC participants (2017 and 2018).

³ Previously this was January 2021 – we expanded the date range to maximise sample size.

⁴ Manchester was the only Local Authority that agreed to provide non-FDAC care proceeding as usual data without an FDAC site in their area.

Study type and number of arms		Quasi-experimental design: Coarsened Exact Matching
Unit of identification		Case, parent, and child
Matching variables		Parent demographics, Domestic Violence, Alcohol or drug misuse, Child demographics, Case characteristics (see Table 3).
Primary outcome	variable	Reunification
	measure (instrument, scale)	Binary indicator derived from the placement of the child at the end of care proceedings.
Secondary outcomes	variables	<ol style="list-style-type: none"> 1) Alcohol and drug misuse cessation; 2) Contested final hearing; and 3) Use of expert witnesses.
	measures (instrument, scale)	<ol style="list-style-type: none"> 1) Binary indicator of whether the parent's alcohol and/or drug misuse has ceased; 2) Binary indicator of whether the final hearing was contested; 3) a) Binary indicator of whether expert witnesses were used during care proceedings; and b) Count of the number of different types of expert witness (based on the binary expert reports/assessments indicators, e.g. independent social work assessment, psychiatric assessment).

A randomised controlled trial was considered for this evaluation but was rejected as the judiciary thought that randomisation of families in care-proceedings could be subject to legal challenge. A feasibility study was conducted as part of this evaluation considering suitable evaluations. The impact evaluation, therefore, uses a quasi-experimental design: Coarsened Exact Matching (CEM).

Identification strategy

The intervention group will consist of cases that have been selected by an FDAC site to receive FDAC care proceedings. A control group will be selected from non-FDAC cases that meet the basic criteria for inclusion in an FDAC but were not selected for FDAC. The basic criteria for inclusion in FDAC are that care proceedings have been issued and that there are concerns about parental alcohol or drug misuse as part of the care proceedings case. The average treatment effect will be estimated by combining a CEM approach, as described by lacus et al. (2009), with regression analysis.

Implementing CEM requires data for characteristics associated with selection into the intervention or the outcome at the start of care proceedings (see Table 3 for a list of all variables used to implement matching). For example, this could include risks to the child (such as parental alcohol or drug misuse or domestic violence in the household). Variables will be “coarsened” into binary or categorical variables (for example, if it is a continuous variable, such as the age of the child, the variable would be re-categorised into age bands). Some variables that are already collected in FDAC sites are currently collected categorically and will not require coarsening.

However, due to resourcing constraints faced by the Local Authorities, some variables collected ordinarily in FDAC cases will require recoding to be comparable to those in non-FDAC cases. For example, parental alcohol and drug misuse at the start of proceedings are assessed using clinical judgement (high, medium and low risk). Substance misuse is recorded differently by Local Authorities for non-FDAC cases, which only comprise types of parental substance misuse rather than clinical judgement of severity. Therefore, to be comparable to non-FDAC cases, severity of parental alcohol and drug misuse at the start of proceedings in FDAC cases will be recoded as general type of parental substance misuse (i.e. Drugs, Alcohol, Drugs and Alcohol, None, and Unknown). The recoded variable will then not require coarsening. Table 3 outlines the variables collected and how they will be coarsened for matching. A sensitivity analysis will test the impact of further collapsing coarsened variables.

The number of children in household and age of the youngest child in household are only available in non-FDAC data. We will derive variables for number of children and age of the youngest child for case and for primary carer, rather than household, using available data at child level. FDAC sites provide ordinal data on severity of drug and alcohol misuse. It was not possible to obtain this data for non-FDAC cases, so instead we will dichotomise the severity ratings in FDAC data so that they align. Table 4 summarise the variables we will derive to harmonise the data between FDAC and non-FDAC groups.

The sample will then be reduced so that every FDAC observation has at least one non-FDAC match on coarsened variables. To maximise the likelihood of including all FDAC observations, we aim to use a larger sample of control cases relative to the intervention group. This approach is typical for matched study designs. Non-FDAC observations that do not match any FDAC observation will be removed from the analysis sample.

The impact estimate is obtained by comparing the outcomes of the intervention group with the outcomes of the matched-control group, while controlling for the full set of uncoarsened covariates. We will use a multilevel logistic regression model, accounting for the hierarchical structure of the data.

The CEM approach stratifies the data on the basis of unique combinations of coarsened variables. For example, every parent in FDAC and non-FDAC with exactly the same combination of coarsened characteristics will end up in the same stratum.

Non-FDAC observations will be weighted as follows (Iacus et al., 2012):

$$\frac{M_{Control}}{M_{Treatment}} \frac{m_{Treatment}^S}{m_{Control}^S}$$

Where $M_{Control}$ and $M_{Treatment}$ denote the total number of matched units in the control and treatment group, respectively; $m_{Control}^S$ and $m_{Treatment}^S$ denote the number of control and treatment units in stratum s , respectively. All FDAC observations get a weight of 1. These weights will be used in the multilevel model.

A CONSORT flow diagram of losses and exclusions will be provided in the evaluation report.⁵ If intervention cases are lost at the matching stage, this would indicate that there were not suitably similar observations in the control group. This would limit the generalisability of the study findings.

The unit of analysis varies across outcomes. The primary outcome (reunification) is defined at the child-level while secondary outcomes are defined at either the parent-level (i.e. parental substance cessation) or case-level (i.e. contested hearing and the use of expert witnesses). Matching will be implemented separately for outcomes at different units of analysis. Child-level outcomes will include covariates about the child (such as age) and parent characteristics (mainly about primary carer's characteristics such as alcohol and drug misuse at baseline). Parent-level outcomes will include case characteristics (i.e. date of issue) and parent-level covariates (e.g. age, gender, substance misuse, and for how many children they are primary carer).⁶ Case-level outcomes will include case characteristics, including date of issue, aggregated parent-level (e.g. primary carer age, gender, ethnicity) and aggregated child-level characteristics (e.g. how many children in the case).

Comparisons will be made using the original uncoarsened covariates, with differences reported as *Hedges' g* effect sizes. If we observe an imbalance with an effect size of greater than 0.05, we will revise the matching specification by adjusting the coarsening of any variables with an imbalance of greater than 0.05. We will reduce the number of coarsening categories (unless the variable in question is already uncoarsened). If this does not resolve the imbalance, we will coarsen the variable further instead.

We will also use the multivariate L1 distance statistic before and after matching to describe the quality of the matching. This statistic measures the overall imbalance with respect to the joint distribution, including all interactions, of the covariates (Blackwell et al., 2010). The smaller L1, the less the imbalance across the covariates, with L1=0 indicating perfect balance and L1=1 complete separation across covariates.

Data sources and availability

The data to be used for matching and the evaluation of outcomes will be collected from several sources:

- The FDAC data collection tool, for FDAC cases
- Local Authority case management systems, typically within children's social care and legal teams
- Case notes, typically held by Local Authorities and courts

Data collected as part of the FDAC data collection tool will be the source of data for intervention cases. Ideally, data for the control group would be collected from the same data source, however Local Authorities do not systematically collect all the information collected by FDAC sites. Local Authorities will be asked to provide comparable data from their own case management systems and case notes based on a template developed by NatGen. As far as is possible, we will aim for comparable data collection. For example, we will collect data from fields used in statutory returns, such as those collected in the Children in Need census. Fields collected by control sites will be more limited relative to intervention sites. For

⁵ Although the CONSORT *Statement* was originally developed to guide the reporting of RCTs, many of its components also apply to other types of quasi-experimental impact studies. A flow diagram template will be downloaded from <http://www.consort-statement.org/>.

⁶

example, drug misuse will be based on data collected by Local Authorities, which may not be as accurate as the testing conducted in FDAC sites (e.g. drug testing using samples of parent's hair).

Our data collection template that will be completed by the Local Authorities will include detailed instructions and guidance for all key fields required to complete this evaluation. There will be cell validation in order to minimise blank cells and ensure data comparability across Local Authorities. We will only be able to use fields collected in both FDAC sites and Local Authorities in the matching.

Key characteristics that we intend to include in the matching are illustrated in Table 3. Each of these characteristics are collected in both the FDAC data collection tool and the control group data collection tool. These variables are important indicators of whether a case is suitable for FDAC (such as type of drug and alcohol misuse). However, it cannot account for subjective criteria that may be used by FDAC sites in determining which cases should be supported by FDAC. Table 3 also outlines how specific variables will be coarsened in matching.

Local Authorities will be asked to complete this template using data from their own case management systems and case notes. Due to resourcing constraints faced by the Local Authorities, and following consultation, we removed some covariates from the planned analysis and data collection template. The original set of covariates can be found in Appendix C .

Table 3. Intended variables for matching intervention and control cases and coarsening strategy for child, parent and case-level outcomes			
Characteristics	Variable	Type	Coarsening Strategy
<i>Child-level outcome: reunification</i>			
Case characteristics	Date of issue	Date	Year and quarter
Parent characteristics	Primary carer – Age	Continuous	Age bands: Less than 18 years old, 18–24, 25–34, 35–44, 45 or older
	Primary carer – Gender	Categorical	No coarsening: Male, Female, Other, Unknown
	Primary carer – Ethnicity	Categorical	No coarsening: White, Asian/Asian British, Black African/Black Caribbean/Black British, Mixed/multiple ethnic groups, other, unknown
	Primary carer for how many children	Continuous	Bands: 0 (if not primary carer), 1, 2, 3 or more
	Primary carer – Age of youngest child cared for	Continuous	Age bands: Less than 12 months old, 1, 2–3, 4–7, 8–11, 12–16
	Primary carer – Past experience of domestic abuse	Categorical	No coarsening: Yes – perpetrator, Yes – Victim, Yes – both, No, Unknown
	Primary carer – Currently experiencing domestic abuse	Categorical	No coarsening: Yes – perpetrator, Yes – Victim, Yes – both, No, Unknown
	Primary carer – Whether misusing at time of referral	Categorical	No coarsening: Yes, No, Unknown

	Primary carer - Substance misuse type	Categorical	No coarsening: Drugs, Alcohol, Drugs and Alcohol, None, Unknown
Parent characteristics aggregated to case level	Number of parents in the case	Categorical	One, two three or more
Child characteristics	Age	Continuous	Age bands: Less than 12 months old, 1, 2-3, 4-7, 8-11, 12-16
	Gender	Categorical	No coarsening: Male, Female, Other, Unknown
	Ethnicity	Categorical	No coarsening: White, Asian/Asian British, Black African/Black Caribbean/Black British, Mixed/multiple ethnic groups, other, unknown
Parent-level outcome: substance misuse cessation			
Case characteristics	Date of issue	Date	Year and quarter
Parent characteristics	Age	Continuous	Age bands: Less than 18 years old, 18-24, 25-34, 35-44, 45 or older
	Gender	Categorical	No coarsening: Male, Female, Other, Unknown
	Ethnicity	Categorical	No coarsening: White, Asian/Asian British, Black African/Black Caribbean/Black British, Mixed/multiple ethnic groups, other, unknown
	Primary carer for how many children	Continuous	Bands: 0 (if not primary carer), 1, 2, 3 or more
	Primary carer – Age of youngest child cared for	Continuous	Age bands: Less than 12 months old, 1, 2-3, 4-7, 8-11, 12-16, N/A (if not primary carer)
	Past experience of domestic abuse	Categorical	No coarsening: Yes – perpetrator, Yes – Victim, Yes – both, No, Unknown
	Currently experiencing domestic abuse	Categorical	No coarsening: Yes – perpetrator, Yes – Victim, Yes – both, No, Unknown
	Whether misusing at time of referral	Categorical	No coarsening: Yes, No, Unknown

	Substance misuse type	Categorical	No coarsening: Drugs, Alcohol, Drugs and Alcohol, None, Unknown
Case-level outcome: contested hearing, the use of expert witness			
Case characteristics	Date of issue	Date	Year and quarter
Parent characteristics aggregated to case level	Number of parents in the case	Continuous	Bands: 1, 2, 3 or more
Parent characteristics	Primary carer – Age	Continuous	Age bands: Less than 18 years old, 18-24, 25-34, 35-44, 45 or older
	Primary carer – Gender	Categorical	No coarsening: Male, Female, Other, Unknown
	Primary carer – Ethnicity	Categorical	No coarsening: White, Asian/Asian British, Black African/Black Caribbean/Black British, Mixed/multiple ethnic groups, other, unknown
	Primary carer – Past experience of domestic abuse	Categorical	No coarsening: Yes – perpetrator, Yes – Victim, Yes – both, No, Unknown
	Primary carer – Currently experiencing domestic abuse	Categorical	No coarsening: Yes – perpetrator, Yes – Victim, Yes – both, No, Unknown
	Primary carer – Whether misusing at time of referral	Categorical	No coarsening: Yes, No, Unknown
	Primary carer – Substance misuse type	Categorical	No coarsening: Drugs, Alcohol, Drugs and Alcohol, None, Unknown
Child characteristics aggregated to case level	Number of children in the case	Continuous	Bands: 1, 2, 3 or more
	Age of youngest child in the case	Continuous	Age bands: Less than 12 months old, 1, 2-3, 4-7, 8-11, 12-16

Table 4. Harmonising FDAC and non-FDAC data			
Derived variable to harmonise available data	FDAC data	Non-FDAC data	How derived
Primary carer for how many children	N/A	Number of children in household	We will derive a new parent-level variable from child-level data, counting how many children each parent cares for as primary carer, for both FDAC and non-FDAC groups.
Primary carer – Age of youngest child cared for	N/A	Age of youngest child in household	We will derive a new parent-level variable from child-level data, selecting the age of the youngest child the parent cares for as primary carer.
Number of children in the case	N/A	Number of children in household	We will derive a new case-level variable that is a count of the number of children in the case.
Age of youngest child in the case	N/A	Age of youngest child in household	We will derive a new case-level variable, calculated from the age variable in the child-level data.
Substance misuse type	Severity of parental alcohol misuse: High, Medium, Low, None, Unknown*	Substance misuse type: Drugs, Alcohol, Drugs and Alcohol, None, Unknown	FDAC data will be recoded to match non-FDAC data as follows: 1) If misusing at time of referral = No, we will code the substance misuse type as None; 2) If misusing at time of referral = Yes, we will check parental alcohol misuse and drug misuse and categorise their substance misuse type as Drugs, Alcohol, Drugs and Alcohol, or Unknown; 3) If misusing at time of referral = Unknown, we will recode the substance misuse type as Unknown
	Severity of parental drug misuse: High, Medium, Low, None, Unknown*		

*This is based on clinical judgements on substance misuse severity where guidance for the FDAC data collection tool is provided in Appendix D.

Minimum detectable effect size calculations

Power calculations were conducted in Stata 17.1 based on the anticipated matched sample size, using formulae from Dong and Maynard (2013). These formulae are presented in Appendix F. The results are presented in Table 5. The total number of non-FDAC cases will exceed the numbers prior to matching. Cases are then weighted so that there are equivalent FDAC and non-FDAC cases, so we assume equal sample sizes at analysis. Our assumptions are:

- Clustering of children within parents, with an ICC of 0.70
- No clustering of children within Local Authorities
- Variance explained by the uncoarsened covariates used in a regression model after matching, with an R-squared of 0.20 at level one and 0.10 at level two. We estimate this to provide a correlation of 0.45 and 0.32 respectively⁷
- That 25% of children in control are reunified with their parents by the end of care-proceedings (informed by Harwin et al., 2018)
- A type one error rate of 0.05
- Power of 0.80 (a type two error rate of 0.20)
- Two tailed significance testing

There is some uncertainty on the expected sample size. Based on Harwin et al. (2018) we anticipate that each FDAC case will have a mean of 1.4 children. Our assumptions use this figure and build on estimates of expected caseloads for the implementation period (January 2021 – June 2022) collected by FDAC sites by CJI in Summer 2020. Based on updated figures concerning expected caseloads, we anticipate a sample of 300 intervention children from 215 cases.

Based on these assumptions, we expect the evaluation will be powered to detect a relative risk ratio of 1.36 (or equivalent to a 9.1 percentage point difference). No power calculations are conducted for secondary analyses, but these will have lower power as the units of analyses are at the parent and case-levels. However, as the intracluster correlation coefficient is relatively large for child-level analyses, the reduction in power will be relatively small.

Table 5 Minimum detectable effect size calculation		
Relative Risk Ratio		1.36
Baseline/Endline correlations	Child	0.45
	Parent	0.32
Intracluster correlations (ICCs)	Parent	0.70
		0.00

⁷ Our sample size calculations include estimates of the proportion of variance explained through the included covariates at each of these levels (R^2). We have converted these into pre- post-test correlations by taking the square root of the R^2 value.

Social Worker Team		0.00
Alpha		0.05
Power		0.80
One-sided or two-sided?		2
Level of intervention clustering		Parent
Average cluster size (parent)		1.40
Sample Size (children)*	Intervention	300
	Control	300
	Total	600
Sample Size (cases)	Intervention	215
	Control	215
	Total	430

* Of which, we assume half are intervention children and half are matched control children

Outcome measures

The primary and secondary outcomes will be sourced from the FDAC data collection tool for intervention cases and Local Authority administrative data for control cases. Local Authorities will already collect data on reunification but may not systematically capture parental alcohol or substance misuse, contested final hearing and use of expert witnesses. This will be included in the data collection template prepared by NatGen.

The primary outcome of interest will be a binary indicator of reunification immediately at the end of care proceedings. We define reunification as the legal order given for the child to either return to live with the parent, or to continue to live with the parent. Reunification is not achieved where the placement of a child at the end of care proceedings is different from the start of proceedings. This includes placement with another parent or family member at the end of care proceedings. For FDAC cases, the judge's ruling on the placement of the child will be recorded in the FDAC tool by FDAC staff. We expect the placement of the child would be recorded by Local Authorities and this will be captured in the data collection template for non-FDAC cases.

The secondary outcomes are parental alcohol and drug misuse cessation, whether the final hearing is contested, and whether expert witnesses were used during care proceedings.

Parental alcohol and drug misuse are currently recorded in the FDAC data tool as two key categorical variables: the severity of parental substance misuse (low, medium, high) and the level of risk to the child from parental substance misuse (low, borderline, harmful). This will be recorded differently in the new tool – using a suite of binary indicators that used to be fed into the clinical judgement that is currently used, due to resource constraints with non-FDAC data. The outcome variable will be a binary indicator of whether the parent is currently misusing drugs and/or alcohol at the end of care proceedings (where one indicates they are currently misusing and zero indicates they are not currently misusing).

As we are interested in the impact of FDAC on the likelihood of final care proceedings hearings being contested relative to non-FDAC care proceedings as usual (RQ4), contested final hearings will be recorded as a binary outcome. The final hearing will be classified as contested regardless of which party contests the hearing.

Similarly, as we are interested in the impact of FDAC on the likelihood of expert witnesses being consulted during FDAC care-proceedings relative to non-FDAC care proceedings as usual (RQ5), the use of expert witnesses will be defined as a binary variable. If the number of witnesses is recorded, then this will be dichotomised. In addition to this binary outcome, we will further conduct an additional analysis with a count outcome variable that indicates the number of *different* types of expert witness being consulted during care-proceedings (based on the binary expert reports/assessments variables, e.g. cognitive functioning assessment, psychiatric assessment, independent social work assessment).

Long term reunification will also be assessed. This will be defined based on a return to court for care proceedings within three years of reunification at the end of care proceedings. To address this research question (RQ3), we will use long term reunification data obtained from Cafcass. This data has detailed accounts of FDAC care proceedings cases from 2017/18. We will also assess the final legal order from the return to court.

Finally, we will also consider the placement of the child. We will conduct an additional analysis (RQ6) with a categorical outcome variable that indicates whether the child is placed with their parents, living with another relative or in LA care, rather than as a strict binary outcome indicating whether cases resulted in reunification or not.

Analysis plan

Primary Analysis

Matching

The primary analysis will estimate the impact of FDAC on reunification at the end of care proceedings based on the placement of the child. CEM will be conducted at the child-level using the characteristics identified in the Identification Strategy section. The matching is conducted at parent and child-level, as the placement of children at the end of care proceedings can vary for different children in a care proceedings case. Therefore, the primary analysis seeks to match children in FDAC care proceedings with similar children in non-FDAC care proceedings as usual.

The matching will be conducted using the user-written package *cem* in Stata 17.1 SE (Blackwell et al., 2010), which implements CEM as described in Iacus et al. (2009). The primary analysis will be conducted on an intention-to-treat basis where the outcome is non-missing. Where covariate data is missing, the *cem* package matches cases that are missing data on the same covariates. If missing outcome data exceeds five percent of the intervention sample, we will consider conducting a sensitivity analysis using multiple imputation (see missing data analysis).

Any intervention cases excluded from the analysis because no match can be found will be transparently reported using a CONSORT flow diagram. If more than five percent of intervention cases are lost because of issues with common support, we will consider adjusting how covariates are coarsened. This would involve collapsing categories of coarsened covariates and altering bin sizes. A summary of matching variables and how they will be coarsened can be found in Table 3.

Analysis

A “doubly robust” estimation of causal effects will be estimated for the matched sample, applying the weights assigned during the matching, including a binary indicator of allocation to FDAC, while also including the uncoarsened matching covariates in the regression model, following Funk et al. (2011). The “doubly robust” estimation reduces the risk that the average treatment effect on the treated (ATT) is biased, provided that either the matching (modelling exposure to the intervention) or the regression model (describing the relationship between the dependent and independent variables) is well specified (Funk et al., 2011). To account for the clustering of children within primary carers, cases and within sites (as shown in Table 1)⁸, we will use a multilevel logistic regression model. The full model is as follows:

$$P(\text{Reunification}_{ipcs}) = \text{logit}^{-1}(\beta_0 + \beta_1 \text{Intervention}_{ipcs} + \beta_2 X_{ipcs} + u_{pcs} + u_{cs} + u_s)$$

Where $P(\text{Reunification}_{ipcs})$ is the probability of reunification. Children (i) are nested within primary carers (p), cases (c), and sites (s). The vector X_{ipcs} denotes the uncoarsened covariates. The random intercepts are represented as u_{pcs} , u_{cs} , u_s , respectively. Should the random intercept adjustments at primary-carer-level and case-level be significantly highly correlated (over .8) with each other, we will drop case-level random intercepts from the model.

The multilevel models described above will be estimated using the *melogit* command in Stata.

Additional Analysis

As a sensitivity analysis, we will fit a multilevel logistic regression model on the matched sample excluding the characteristics used in the matching in the regression model:

$$P(\text{Reunification}_{ipcs}) = \text{logit}^{-1}(\beta_0 + \beta_1 \text{Intervention}_{ipcs} + u_{pcs} + u_{cs} + u_s)$$

Secondly, we will also assess the impact of FDAC on the placement of the child (RQ6), using a categorical outcome variable that indicates 1) reunification with the child’s parents 2) placement with another relative, or 3) LA care. This will be assessed using a multilevel multinomial logistic regression.

$$P(\text{Placement}_{ipcs}) = \text{logit}^{-1}(\beta_0 + \beta_1 \text{Intervention}_{ipcs} + \beta_2 X_{ipcs} + u_{pcs} + u_{cs} + u_s)$$

As with the primary analysis, we include the uncoarsened covariates used in matching. We will present effect sizes as relative risk ratios, with 95% confidence intervals. The risk ratio will be estimated as described above.

Missing data analysis

⁸ Site refers to case locations, as shown in Table 1. Control cases will be coded as the FDAC site that serves their Local Authority (e.g. both Coventry and Warwickshire non-FDAC data will be allocated to Coventry site data). Manchester will be coded independently as there is no FDAC site.

If greater than five percent of cases are missing outcome data on the primary analysis, it is likely that missingness may impact the results of the evaluation and we will therefore consider conducting additional analysis for the primary outcome.

Firstly, we will assess if missing data can be predicted using observed characteristics using a ‘drop out’ model. The dependent variable will be a binary indicator of missing data on the primary outcome. Independent variables will include all (uncoarsened) covariates used in the matching model. Additional categories will be added to ensure that cases with missing data on independent variables are included in this model.

If this model finds statistically significant associations (a p-value of less than 0.05) between observed characteristics and the dependent variable, we will assume that data is missing at random (MAR).⁹ If we assume data is MAR we will conduct a sensitivity analysis using multiple imputation.

Multiple imputation by chained equations (MICE) will be estimated in Stata 17.1 using the *mi* suite of commands. The first 200 observations will not be used (‘burn in’) to ensure that a stable distribution has been reached. In total, 75 datasets will be imputed. The imputed values will be used in the matching model by using the *impvar* option of the user-written *cem* package used for the primary analysis.

Secondary Analysis

The secondary analysis will assess the impact of FDAC on three outcomes:

- Parental alcohol and drug misuse cessation (RQ2)
- If the final hearing was contested (RQ4)
- Whether expert witnesses were used (and the total number of different types of expert witnesses were included). (RQ5)

Matching for these outcomes will be conducted at the parent (or case) level using the covariates outlined in Table 3. A separate matching model is used to assess the impact on these outcomes to the primary analysis. Unlike the primary analysis, the unit of analysis for these outcomes is at parent (or case) level. We therefore want to match similar parents (or cases) rather than children within cases.

The approach will be consistent with the primary analysis, using the same user-written *cem* package in Stata 17.1. The numbers of matches, non-matches, and covariate balance will also be reported consistently with the primary analysis.

Each of these outcomes will be analysed as binary variables with the unit of analysis at parent (or case) level. The sample for the analysis on cessation will include only parents who had substance misuse issues at baseline. If the use of expert witnesses is recorded as a count variable (i.e. the number of witnesses used) we will dichotomise the variable for this analysis. These outcomes will therefore be analysed using a multilevel logistic regression model, using an approach consistent with the binary analysis:

⁹ By definition, it is not possible to assess if there are associations with unobserved characteristics. If there were associations with unobserved characteristics, the data would be described as missing not at random (MNAR). In this case, both the primary analysis and multiple imputation would produce biased estimates. A full description of types of missing data and their consequences are available in the WWCS statistical analysis guidance.

$$(1) P(Cessation_{pcs}) = \text{logit}^{-1}(\beta_0 + \beta_1 \text{Intervention}_{pcs} + \beta_2 X_{pcs} + u_{cs} + u_s)$$

$$(2) P(Contested_{cs}) = \text{logit}^{-1}(\beta_0 + \beta_1 \text{Intervention}_{cs} + \beta_2 X_{cs} + u_s)$$

$$(3) P(Experts_{cs}) = \text{logit}^{-1}(\beta_0 + \beta_1 \text{Intervention}_{cs} + \beta_2 X_{cs} + u_s)$$

The total number of different types of expert witness will otherwise be analysed using a multilevel Poisson regression, with overdispersion adjustment where appropriate. The model notation is as follows:

$$(4) \text{ExpertTypes}_{cs} = \exp(\beta_0 + \beta_1 \text{Intervention}_{cs} + \beta_2 X_{cs} + u_s)$$

Where u_s represents the random effects and are assumed to be normally distributed with 0 mean. The risk ratio will be estimated using the *meglm* command in Stata.

Effect Size Estimation

For binary outcomes in this study, we will report the relative risk ratios (RRR), using the following formula:

$$RRR = \frac{P(\text{Reunified} | \text{FDAC}, X)}{P(\text{Reunified} | \text{non-FDAC}, X)}$$

Where the numerator is the probability of reunification for FDAC cases conditional on covariates (denoted X in the formula), and the denominator is the probability of reunification for non-FDAC cases conditional on the same set of covariates.

We will calculate RRR as follows. First, we will calculate the conditional probabilities from the fitted coefficients of the multilevel logistic regression models by holding the covariates constant at their means.¹⁰ Second, we will then calculate relative risk ratios using the *nlcom* command in Stata, which returns the standard errors and confidence intervals of each ratio.

Exploratory Analysis

Additional analysis will be conducted to assess whether reunification can be sustained over time, as prior evidence (Harwin et al., 2019; Broadhurst et al., 2017) indicated that the greatest risk of returning to court for care proceedings are the first two years after reunification.

A prior cohort (2017 and 2018) of FDAC participants will be identified by FDAC sites. They will then share identifiers with Children Family Court Advisory and Support Service (Cafcass). Cafcass will then identify if children were returned to court for care proceedings in the subsequent three years. The analysis does not compare to a counterfactual as it will be drawing on historic data, and we do not expect comparable data for a control group to be available. This means a counterfactual cannot be constructed with matching. As this analysis will not use a counterfactual, it will not provide a causal claim, but we will compare to a national average.

¹⁰ Non-ordered categorical covariates will be represented as binary variables for each level.

We will also assess whether people who experience racism have different outcomes than those who do not. If the available data allows, we will also separate estimates for white FDAC participants and all other FDAC participants. We understand that different ethnicities may experience different impacts, and that white/non-white may mask underlying differences between different groups. However, we do not anticipate that the available sample sizes would support robust estimates for separate minority ethnicity groups.

Contextual Factors Analysis

This evaluation includes FDAC sites across multiple Local Authorities. Some FDAC sites will have been operating for over a decade, whilst others will only have launched in 2020. There are also differences in delivery models across sites. This will be explored as part of the Implementation and Process Evaluation (IPE).

In addition, COVID-19 has had a significant impact on social care, both within FDAC sites and in Local Authorities more generally. The impact of COVID-19 varies across some sites. Existing sites have not been able to take on new cases or have had staff diverted to other social care work. New sites have had difficulties with recruitment and have had to delay their launch dates. At this stage, the full impacts of COVID-19 on FDAC sites are not known, but further contextual information will be provided in the evaluation report.

This evaluation will not be powered sufficiently to estimate variation in the effect across sites, but variation in implementation will be explored across sites as part of the IPE.

Implementation and process evaluation

The IPE will use a qualitative methodology; interviews in sampled case study areas, to understand the advantages and disadvantages of particular FDAC iterations and variations and gather information on barriers and facilitators to successful implementation and delivery. It will also explore how perceived impacts from the FDAC courts compare with non-FDAC care proceedings as usual. Drawing on the work undertaken for the feasibility study to refine the programme theory for the FDAC model, the IPE will also explore (see Appendix E):

- Views and experiences of implementing FDAC;
- Awareness, understanding and commitment to the FDAC approach;
- Parental engagement, views and experiences of court proceedings;
- Staff and practitioner views and experiences of delivering the model, working across multidisciplinary teams and engaging families; and
- Views on the perceived impact of FDAC on families, the courts, practitioners and wider CJS.

With stakeholders and staff, data collection will focus on commitment, buy-in and understanding of FDAC, views on whether the necessary procedures, processes and resources are in place, the skills and competencies of professionals to effectively deliver FDAC (including for example, selection to receive FDAC), the effectiveness of judicial oversight and partnership working and views on parental engagement, experiences and skills development.

With parents, data collection will include an understanding of and engagement with FDAC, experience of court proceedings and support offered through FDAC, perceptions of change related to skills, competencies and behaviours around for example, accessing support, relationships with children and managing safety and wellbeing, views on fairness and perceptions of impact (including key drivers).

A qualitative approach that provides in-depth information on the implementation, delivery and perceived impact of FDACs will complement the impact evaluation and offer explanations for observed effects – this will help to unpack what works by identifying key facilitators and barriers. A range of dimensions of the implementation will be assessed, including:

- *Intervention fidelity reach and dosage* – exploring how the programme has been implemented and delivered, whether and how it was delivered to the intended population and the extent of support and contact this group received.¹¹
- *Quality of delivery* – data will be gathered from participants on their experiences of delivering and receiving support through FDAC.
- *Variations in FDAC delivery* – analysis of interview data will enable a comparison of implementation and delivery issues across different models, to understand facilitators and barriers, drivers of success and will help to draw out learning.
- *Participant responsiveness* – engagement with the FDAC aims, court proceedings, treatment services and other activities will be explored through data collection with staff and parents themselves.
- *Ability to differentiate the programme from non-FDAC care proceeding-as-usual activities* – where appropriate, participants will be asked to reflect on how implementation and delivery experiences differ from non-FDAC care

¹¹ The FDAC 'model' is delivered differently depending on local needs and infrastructure. 'Fidelity' is therefore better understood as being to a set of core elements than a fully articulated model.

proceeding-as-usual. We will also collect data from non-FDAC care proceeding-as-usual sites to compare views and experiences directly.

Methods

The IPE will take a case study approach in FDAC sites which will comprise of interviews with key stakeholders, staff and beneficiaries, including for example the judiciary, LA leads, support staff (e.g. substance misuse specialists) and parents who come before the court. We will also conduct interviews with key staff in four non-FDAC 'business as usual' courts, to enable us to gain a good understanding of how process differ and key facilitators and barriers to delivering FDAC in different areas.

A brief overview of the rationale for this approach, including some preliminary thoughts on sampling and recruitment is set out below. Depending on the scope of the IPE, it may be valuable to increase interview numbers across case study and non-FDAC sites, which would offer more detailed insight into the implementation and delivery of the pilots.

FDAC case studies

We will carry out six case studies (around 36 interviews in total) across sites. Case study areas will be sampled for diversity across:

- FDAC start dates
- The model of delivery
- The iterations included in the FDAC model
- Volume of FDAC cases / throughput
- We would also aim to get diversity in terms geographical location, local demographics and size of the court.

Case study courts will be selected in close partnership with WWCS and the CJI, drawing on the detailed knowledge that CJI have on set-up and progress on FDACs, especially in light of Covid-19.¹²

This case study approach will allow us to explore similarities and differences across the various models of delivery and will include interviews with key stakeholders, staff and beneficiaries, including for example the judiciary, local authority leads, support staff (e.g. substance misuse specialists) and parents who come before the court. The exact breakdown of interviews across these groups will likely vary across the case studies, depending on the model of delivery, however, we would aim to achieve a good spread across the groups and agree a final sampling strategy with WWCS and CJI when planning fieldwork towards the end of 2020. An early indication of how interviews might be spread across each case study area is included in the table below.

Table 6 Interview quotas across participant groups		
Participant Group	Number of interviews within case study	Total number of interviews across sites

¹² Covid-19 has impacted variously on FDACs, delaying set-up and launch in some areas. We will need to be mindful of this when selecting case study courts for the IPE to ensure as much learning as possible can be gathered from the qualitative data collection.

Judiciary (e.g. judges, magistrates, court clerks etc.)	1	6
FDAC team leads/ Local authority leads (e.g. staff working with parents to deliver FDAC – aim to achieve diversity across front line and management roles)	2	12
Support organisations (e.g. specialist domestic violence, substance misuse providers etc.)	1	6
Parents (aim to achieve diversity across parent demographics, for example, family composition, past experience of court proceedings, level of engagement with FDAC etc.)	2	12

Interviews with non-FDAC sites

Alongside the work carried out with case study FDAC courts, we also propose to conduct interviews with key staff in four non-FDAC care proceedings as usual courts (up to eight interviews in total). This will enable us to gain a good understanding of how process differs in non-FDAC courts, including key facilitators, barriers and perceived impacts.

The selection of the non-FDAC sites would be conducted in close collaboration with WWC to ensure sites share some of the key characteristics with the FDAC case studies (e.g. local demographics / geographical location / size of court) for comparison.

Recruitment

Recruitment and fieldwork activities should be coordinated and clearly communicated to those involved to minimise burden on FDAC sites and partners. We suggest that a main lead (or 'link person') is identified at each case study to support recruitment. This individual will be responsible for liaising with other staff, teams and organisations for recruitment purposes and will be fully briefed by the research team about sampling and recruitment strategies, which can be relayed to others as necessary. The process for identifying and inviting individuals to participate, will likely include the following steps:

- The link person will be sent a concise briefing note explaining how we would like them to help with the evaluation and the process of identifying and recruiting participants. This will be followed up with a phone call to check understanding, identify any potential issues and discuss solutions.
- Individuals who are identified as eligible to take part in the research will be given an information sheet supplied by the research team. All recruitment materials will be clear about: the basis and purpose of the study; who is funding the research; how people can opt-in/out of involvement at no personal cost; confidentiality and caveats to this (for example disclosure), and information about NatCen.
- Those interested in taking part will be asked to either consent to have their contact details passed to the research team or for an interview to be arranged.

As consent is an ongoing process, the research team will revisit the information given at the recruitment stage and gain informed consent before the start of all interviews. Our experience indicates that selection bias can be an issue when recruiting some participant

groups (especially parents in this study). We have addressed this on previous studies by initially oversampling people to allow the research team to select individuals who best meet the sample criteria and recommend using this approach here.

For non-FDAC sites, where we do not have contacts through CJI, we will approach the family courts selected directly to inform them of the purpose of the study and invite them to participate. We will utilise publicly available information to do this and follow up as appropriate.

Data collection

We will draw on our extensive track record of delivering high quality and ethically sound research to inform the approaches we use to introduce the research, encourage participation, and collect data. This will include the development of clear and comprehensive recruitment materials setting out the research requirements and parameters of participation to support informed consent.

Qualitative data collection will be led by experienced members of the research team, who will draw on a range of techniques to collect the highest quality of data, including using active listening and responsive, open questioning to build rapport. Interviews will be supported by a topic guide which will provide an overview of key themes to be discussed with each participant. All recruitment and fieldwork materials will be developed and refined in close collaboration with you to ensure they fully meet the research objectives.

Analysis and reporting

Interviews will last up to 60 minutes and will be carried out via phone, web or face-to-face, depending on participant preference and social distancing requirements at the time of data collection. Interviews will be audio-recorded and transcribed verbatim to facilitate robust analysis.

All recordings will be managed using NatCen's Framework approach. This will involve managing interview data and conducting case- and theme-based analysis. Key topics emerging from the transcripts will first be identified. A thematic framework will then be developed and used to organise the data from each participant. Then the coded data will be reviewed in detail, drawing out the range of experiences or views, identifying similarities and differences, developing and testing hypotheses, and interrogating data to seek to explain patterns and findings.

Based on the systematic analysis of the IPE data, a short report will be drafted and submitted to the WWCS and CJI in the Autumn of 2021. The report will highlight key findings thematically, examining similarities and differences between case study areas and participant groups. A summary of IPE findings will also be included in the final report, due to be submitted in September 2022.

Cost evaluation

Costs will be estimated based on the delivery costs of the intervention. Each FDAC site manager will be provided with pro-forma to complete in Summer 2022 covering costs incurred in the financial year 2020/2021. This will assign costs to different categories, such

as setup costs (e.g. preparation costs, facilities, overheads) and delivery costs (e.g. salary costs, training and support costs, assessment and legal fees).

Sites will be asked to provide this data for a one financial year. The set-up costs will be estimated only for sites that have launched in March 2020 or after. Costs will be estimated from the perspective of the LA on a per-child basis by dividing the total annual cost (i.e. setup and delivery costs) by the number of children who go through FDAC care proceedings.

Ethics & Participation

Ethical clearance was sought from NatCen's ethics committee in June 2020. Ethical approval was granted on 25th June 2020 subject to the condition of agreeing safeguarding procedures with Local Authorities and providing support for NatCen staff working on the evaluation.

Further ethical clearance will be requested from NatCen's ethics committee for the IPE in December 2020, when plans and timings for fieldwork, (including the selection of case study areas) have been agreed. We will also seek ethical approval from the judiciary in order to conduct interviews with judges and magistrates for the IPE. Once case study areas have been decided, applications will be submitted to the relevant Head of Division or the Senior Presiding Judge, as stipulated in the Courts and Tribunals Judiciary guidance.¹³ We are experienced in seeking ethical approval to carry out research with members of the judiciary, which will help us to prepare applications and requests for these approvals.

Ethical clearance was revisited in July 2022, confirming an ethical requirement to notify parents whose case was seen by non-FDAC care proceedings as usual of the use of their data with a requirement to provide an option for them to opt-out. NatCen and WWCS researchers consulted with each other and all parties agreed to send a letter to parents informing them of the use of their data and the reasons for the use of their data within this project giving the parents and their children the right to opt-out of their data being used within a reasonable timeframe. Local Authorities are expected to send out an opt-out letter to data subjects in August 2022, unless exemptions apply.

Registration

The study has been pre-registered on the Open Science Framework on December 4th, 2020. The registration can be found here: <https://osf.io/w7zac>.

Data protection

NatCen will be the data controller for the duration of the impact evaluation and will be responsible for the collection, processing and storage of the data. NatCen has signed data sharing agreements with Local Authorities for sharing non-FDAC care proceedings data, with FDAC sites for sharing data from FDAC cases and with WWCS for sharing personal data.

The legal basis for processing and sharing the data under GDPR Article 6 1(f) is 'legitimate interests', with processing of special categories of personal data for scientific research purposes under GDPR Article 9 2(j). This means that there is a good reason for NatCen to collect and manage this data, and that this data is needed to evaluate and learn about FDACs.

¹³ <https://www.judiciary.uk/publications/judicial-participation-in-research-projects/>

NatCen will be data controller for personal data collected as part of IPE. For all data collection encounters we conduct, we will provide detailed information on what participation entails and we will invite people to participate in the study with their fully, informed consent. All participants will be given a copy of the privacy notice which will provide further information on how we will use the data we collect for the IPE, what their rights are as research participants and how they can withdraw their data from the study if they wish.

Personnel

Table 7 Evaluation team		
Name	Title	Role
Dr Sashka Dimova	Research Director, Evaluation	Principle Investigator and impact evaluation lead
Dr Andi Fugard	Deputy Director, Evaluation	Oversight and QA of impact evaluation. PI and impact evaluation lead during the redesign.
Ellie Roberts	Research Director, Crime and Justice	IPE lead
Professor Judith Harwin	Professor of Socio-Legal Studies	Advisor for impact evaluation and IPE
Dr Bachar Alrouh	Advisor	Advisor for impact evaluation and IPE
Dr Kostas Papaioannou	Senior Researcher (Analyst), Evaluation	Impact evaluation
Sarah Sharrock	Senior Research, Crime and Justice	IPE
Dr Tien-Li Kuo	Researcher, Evaluation	Impact evaluation
Robert Wishart	Formerly Research Director, Evaluation	Principle Investigator and impact evaluation lead
Ben Stocker	Formerly Researcher (Analyst), Evaluation	Impact evaluation

Timeline

Table 8 Timeline		
Dates	Activity	Staff responsible/ leading
December 2020	Evaluation registered on the Open Science Framework	Robert Wishart
March 2021	Data sharing agreements with all Local Authorities and sites	Robert Wishart
November - December 2020	Select case study areas, agree sampling strategy and submit ethical approval to Judiciary	Ellie Roberts
January 2020	Data sharing agreement and research approval from Cafcass	Robert Wishart
January 2020	Fieldwork materials drafted (to include gatekeeper briefings, information sheets, support leaflets, privacy	Ellie Roberts

	notices and web text for different participant groups and topic guides)	
January – February 2021	Liaise with sites to set up fieldwork and begin recruitment	Ellie Roberts
March - July 2021	Conduct qualitative fieldwork	Ellie Roberts
June - August 2021	Data management and analysis	Ellie Roberts
August – September 2021	Report on IPE findings drafted and submitted to WWCS and CJI	Ellie Roberts
Summer 2022	Pro-forma provided to FDAC sites for cost analysis	Andi Fugard
October 2022	Data returned from all FDAC sites and Local Authorities	Andi Fugard and WWCS
November - December 2022	Analysis and reporting	Sashka Dimova, Ellie Roberts
February 2023	Final report submitted	Sashka Dimova, Ellie Roberts

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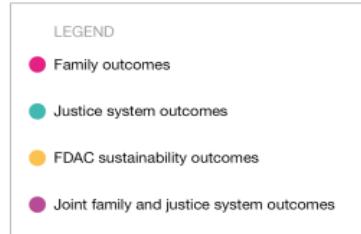
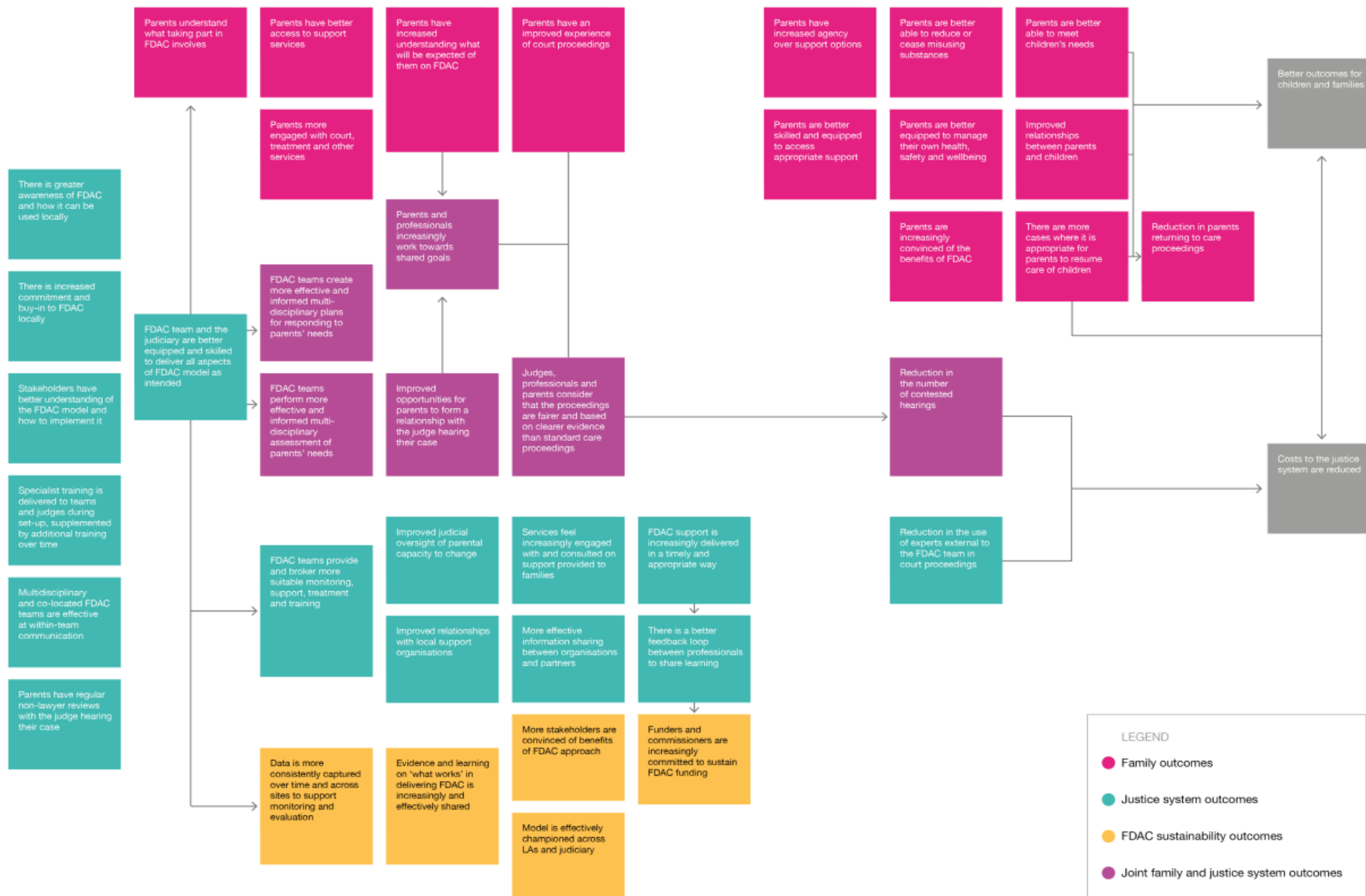
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Appendix A – FDAC Logic Model

INPUTS	ACTIVITIES	OUTPUTS	INTERIM OUTCOMES	Long term outcomes	
<p>FDAC team</p> <p>Budget for the team including:</p> <ul style="list-style-type: none"> Salaries and on costs Non staff costs Training Travel Testing Equipment and accommodation 	<p>Strategic activity</p> <ul style="list-style-type: none"> Engagement with commissioners Engagement with treatment services Engagement with children services Communications materials 	<ul style="list-style-type: none"> Budget and commissioning systems agreed Local leaflets and other materials regularly disseminated Minimum of 2 lawyers and Guardian forums every year FDAC team link for treatment services, quarterly meetings Quarterly Steering groups held Quarterly Operational groups held FDAC Team including Judges, where possible, to attend training with NU every 4 months FDAC team hold in house training days every 6 months, such as VIG or testing 	<p>Professionals Knowledge/awareness/Buy in</p> <ul style="list-style-type: none"> More Key stakeholders have awareness that FDAC is up and running More Key stakeholders understand the model (problem solving approach to care proceedings) More champions for the model in local authority senior management and in the judiciary More stakeholders are convinced of the benefits of the FDAC approach Commissioner are convinced that FDAC should have ongoing funding 		<p>Proportion of children living with the same primary carer with whom they were living before the start of proceedings, at the end of proceedings</p>
<p>Judicial and court time</p> <ul style="list-style-type: none"> Judges time Court staff and administration times Listing office time 	<p>Operational activity</p> <ul style="list-style-type: none"> FDAC Team attendance at court Pre-court meetings Day assessment with each family in 1st week of proceedings. Team organise IPMs and and chair review IPMS, and file minutes Team write initial parenting report, review reports and review parenting assessment report, minutes of meetings and circulate to all parties and court. The FDAC team hold regular key work, using MI and other interventions. The FDAC team chair Children's Needs meeting within first 10 weeks, and file minutes. Recruit, train and supervise parent mentors Organise and ensure team attendance at reflective supervision Drug treatment and testing Child psychiatry 	<ul style="list-style-type: none"> All parents met at court for first hearing by FDAC team All parents meet their allocated FDAC Judge and continue to meet same Judge throughout proceedings All parents have initial assessment by multi-disciplinary team within first 4 weeks FDAC team will formulate each case as a multidisciplinary team. All parents have an Intervention planning meeting within 4 weeks, and attended by FDAC team, parents, children's services, Guardian and treatment providers where possible. Initial Parenting assessment report and intervention plan presented to Court for 2nd Hearing. Once parents signed up to FDAC, parents proceed with regular fortnightly Non-Lawyer reviews Continuous monitoring and support by a therapeutic team, including regular testing, keywork and review reports. All parents will have a review IPM held at 10 weeks and 18 weeks. All parents will have been offered interventions delivered by local services and the FDAC team matched to their needs. All parents have a review parenting assessment report filed with Court for the IRH, which will include final recommendations, and options for post proceedings support. All parents will have had a time-limited highly coordinated therapeutic 'trial for change' by the end of proceedings Parent mentors will be present at Court and all parents will be offered option of support from a parent mentor. Team attend reflective supervision every 8 weeks Team and Judges will have attended initial and follow up training on a regular basis Team and Judges will be mindful of timescales and the National requirements Problem solving court approach is embedded in local court Parties recognise expertise of FDAC team 	<p>Parents' outcomes in FDAC as opposed to normal proceedings</p> <ul style="list-style-type: none"> More engagement of parents in the court processes and with treatment and other services More parents are able to cease misusing substances and sustain their recovery More parents are able to resume care of their children More parents show improvement in their mental health More parents and children are protected from the impact of domestic abuse More improved relationships between parents and children even when children are not going to return home More parents have insight into why they have not been reunited with their children 		<p>Proportion of parents who have ceased substance misuse at end of proceedings</p>
<p>LA and other agency time</p> <ul style="list-style-type: none"> Senior Mgt time for Social worker's time Guardian's time Treatment provider's time Mentors – Budget, recruitment and support 	<p>Training and development</p> <ul style="list-style-type: none"> Attending training Organising in service training days 		<p>Court outcomes</p> <ul style="list-style-type: none"> The team and judges are more skilled at working in line with the problem solving approach within timescales appropriate for children in care proceedings Proceedings when children do not go home will end within the National and Local requirements around 26 weeks More proceedings will end without a final contested hearing More proceedings do not require any other externally funded experts, other than FDAC More proceedings take place without a residential placement for similar type of cases 		<p>Proportion of FDAC than comparison reunification parents with sustained cessation over the follow up period</p>
				<p>A proportion of FDAC than comparison reunifications parents who experienced no disruption to family stability over follow up period</p>	
				<p>Procedural fairness experience</p>	

Appendix B – FDAC Theory of Change



Appendix C – Previous version of intended variables for matching intervention and control cases and coarsening strategy

Table C-1. Intended variables for matching intervention and control cases and coarsening strategy			
Parent characteristics	Variable	Type	Coarsening Strategy
Demographics	Age	Continuous	Age bands: Less than 18 years old, 18-24, 25-34, 35-44, 45 or older
	Ethnicity	Categorical	No coarsening: White, Asian/Asian British, Black African/Black Caribbean/Black British, Mixed/multiple ethnic groups, other, unknown
	Number of children	Continuous	Bands: 1, 2, 3 or more
	Age of youngest child in the household	Continuous	Age bands: Less than 12 months old, 1-2, 2-3, 4-7, 8-11, 12-16
	Accommodation status	Categorical	Coarsened: Owner occupier, Tenant (private or social) Supported housing/hostel/refuge, homeless, other
Domestic abuse	Past experience of domestic abuse	Categorical	No coarsening: Yes – perpetrator, Yes – Victim, Yes – both, No, Unknown
	Currently experiencing domestic abuse	Categorical	No coarsening: Yes – perpetrator, Yes – Victim, Yes – both, No, Unknown
Background	Former looked after child	Binary	No coarsening
	Any previous child(ren) removed	Binary	No coarsening
	Any criminal convictions or cautions	Binary	No coarsening
Substance misuse	Whether misusing at time of referral	Binary	No coarsening
	Substance misuse type	Categorical	No coarsening: Drugs, Alcohol, Drugs and Alcohol, None, Unknown
	Severity of alcohol use (clinical judgment)	Categorical	No coarsening: None, Low, Medium, High, Unknown
	Severity of drug use (clinical judgement)	Categorical	No coarsening: None, Low, Medium, High, Unknown

Mental health	DSM/ICD Diagnosis	Open text	Binary: Yes – any recorded diagnosis, No, Unknown
Child characteristics	Age	Continuous	Age bands: Less than 12 months old, 1-2, 2-3, 4-7, 8-11, 12-16
	Ethnicity	Categorical	No coarsening: White, Asian/Asian British, Black African/Black Caribbean/Black British, Mixed/multiple ethnic groups, other, unknown
	Previously looked after	Binary	No coarsening: Yes, No, Unknown
	Subject to an order	Binary	No coarsening: Yes, No, Unknown
	Ever had a criminal conviction	Binary	No coarsening
	Issue around school attendance	Categorical	Yes, No, Unknown, not school age.
	Education, Health and Care (EHC) Plan	Categorical	Yes, No, Unknown, not school age.
Case characteristics	Date of issue	Date	Year and quarter

Appendix D – FDAC data collection tool: guidance for clinical judgements on substance misuse severity

Guidance for clinical judgements on substance misuse severity		
Severity	Drug misuse	Alcohol misuse
None	Not using any drugs	Not using alcohol
Low	Low level cannabis use Use of prescription drugs (Zopiclone, diazepam, co-codamol)	Non- harmful/non problematic alcohol use, at or below the recommended level (no more than 14 units per week).
Medium	Social/recreational drug use including club drugs and legal highs	Social drinking with history of harmful non physically dependent use Social drinking where there is a history of physically dependent use
High	Intravenous (IV) drug use Chaotic drug use (homelessness, crime, pre-occupation with drug use dominating lifestyle, chaotic relationships, sex work) Poly-substance misuse of illegal drugs (more than one substance) Poly-substance misuse including misuse of prescribed drugs (more than one substance) Poly-substance misuse including misuse of legal highs High level cannabis use, daily multiple use Misusing prescribed drugs (obtaining without a prescription; overuse)	Physically dependent alcohol use

Appendix E – IPE data collection

Table E-1. Outcomes map for implementation and process evaluation		
Outcome	When is it appropriate to measure?	How can it be collected?
FDAC team and the judiciary are better equipped and skilled to deliver all aspects of FDAC model as intended	When FDAC sites are operational	Interviews with FDAC staff and judiciary
More effective and informed multi-disciplinary assessment and development of plans to meet parents' and children's needs	When FDAC sites are operational	Interviews with FDAC staff, children's services and judiciary
Judges, professionals and parents consider that the proceedings are fairer and based on clearer evidence than standard care proceedings	When FDAC sites are operational and parents have finished care proceedings	Interviews with FDAC staff, children's services, judiciary and parents (including those who may have experienced non-FDAC care proceedings as usual)
FDAC team provide and broker more suitable monitoring, support, treatment and training	When FDAC sites are operational	Interviews with FDAC staff, judiciary and parents
Improved judicial oversight of parental capacity to change	When FDAC sites are operational	Interviews with FDAC staff, judiciary and parents
Improved relationships with local support organisations and more effective information sharing between organisations and partners	When FDAC sites are operational	Interviews with FDAC staff
Better feedback loop between professionals to share learning	When FDAC sites are operational	Interviews with FDAC staff
Parents understand what taking part in FDAC involves and are more engaged with court, treatment and other services	When FDAC sites are operational	Interviews with FDAC staff, judiciary and parents
Parents have an improved experience of court proceedings	When FDAC sites are operational	Interviews with parents
Parents have increased agency over support options	When FDAC sites are operational	Interviews with parents

Table E-1. Outcomes map for implementation and process evaluation

Outcome	When is it appropriate to measure?	How can it be collected?
Parents better skilled and equipped to access appropriate support	When FDAC sites are operational	Interviews with FDAC staff, judiciary and parents
Parents increasingly convinced of the benefits of FDAC	When FDAC sites are operational	Interviews with parents
Improved relationships between parents and children	When FDAC sites are operational	Interviews with FDAC staff, judiciary and parents

Appendix F – Power calculation formulae

The formula used to estimate the power of the primary analysis are adapted from Dong and Maynard (2013). The MDES is calculated as follows:

$$MDES = M_{K(J-2)-g_2} \sqrt{\frac{\rho(1-R_1^2)}{P(1-P)JK} + \frac{(1-\rho)(1-R_2^2)}{P(1-P)JKn}}$$

Where:

- ρ is the intracluster correlation;
- R_1^2 and R_2^2 is the variance explained at level one and level two respectively;
- n is the average number of children per family
- J is the average number of parents per Local Authority
- K is the number of Local Authorities

The multiplier, $M_{K(J-2)-g_2}$ is calculated as follows:

$$M_{K(J-2)-g_2} = T_1 + T_2$$

Where T_1 (precision) and T_2 (power) are drawn from the inverse students' t-distribution as:

$$T_1 = \alpha, K(J - 2) - g_2$$

$$T_2 = 2\beta, K(J - 2) - g_2$$

Where g_2 is the number of covariates at level two, alpha is the type one error rate and beta is the type two error rate. We then convert the MDES to an odds ratio:

$$OR = (MDES * \frac{\pi}{\sqrt{3}})^e$$

The relative risk ratio is then estimate using the prevalence of the outcome in control, $Prev_c$:

$$RRR = \frac{OR}{(1 - Prev_c) + (Prev_c * OR)}$$

Trial Evaluation Protocol
Family Drug and Alcohol Courts
Evaluator: NatCen Social Research
Principal investigator: Robert Wishart

Evaluation of Family Drug and Alcohol Courts

Intervention Developer	Family Drug and Alcohol Courts in England
Delivery Organisations	Family Drug and Alcohol Courts in England
Evaluator	NatCen Social Research
Principal Investigator	Robert Wishart
Protocol Author(s)	Robert Wishart, Kostas Papaioannou, Adam Gilbert, Katariina Rantanen
Type of Trial	Quasi-experimental design: Coarsened Exact Matching
Age or Status of Participants	Parents in care proceedings; Children of Parents in care proceedings
Number of Participating Local Authorities	31
Number of Children and Families	600 children (300 intervention; 300 control): 430 families (215 intervention; 215 control)
Primary Outcome(s)	Reunification
Secondary Outcome(s)	Parental alcohol and drug misuse cessation, number of contested hearings, use of expert witnesses, parent-child relationship, parental mental health.
Contextual Factors	Covid-19

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Background and Problem Statement

In 2008 the Government launched its family drug policy strategy, which aimed to prevent intergenerational harm because of parental substance misuse (HM Government, 2008) and to improve outcomes for children affected by parental substance misuse (Harwin and Ryan, 2008). The first UK Family Drug and Alcohol Court (FDAC) was set up in London in 2008 as a three-year pilot funded by central government in the Central London Family Proceedings Court. The London FDAC was followed by sites in Milton Keynes and Buckinghamshire (2014) and East Sussex (2015).

The basic criterion for FDAC referral is that parental substance misuse (drugs or alcohol or both) is a key factor of the Local Authority's concerns about child(ren) within a care proceedings case. FDACs aim to improve outcomes for children and families by providing an alternative way of working with parents involved in care proceedings in relation to alcohol and drug use. The primary aim of FDAC is to ensure that a child can stay with parents or be reunified at the end of care proceedings if it is safe to do so. If reunification is not possible, then the aim is to find an alternative carer for the child swiftly, to give the child the best chance for permanency and stability. FDAC also aims to stop parents from misusing alcohol and drugs, to make the home environment safer, and to reduce the risk of future instances of care-proceedings.

FDACs use a 'problem-solving' court approach to justice, whereby courts use their authority to help address the complex social issues that bring people before them (Harwin and Ryan, 2008; Roberts et al., 2017). FDACs encourage parents to believe recovery and change are possible, along with aiming to provide a realistic understanding of the challenges they face. Specialist, designated judges provide parents with regular supervision and support through fortnightly court reviews. A specialist multidisciplinary team also works closely with the courts and parents to support families to change and overcome their alcohol and drug misuse problems and other difficulties.

Children and Families Act 2014

The Children and Families Act 2014 made several substantive changes to the implementation of care proceedings. The changes most relevant to FDAC relate to the use of experts in care proceedings and the introduction of new limits on the duration of care proceedings. S13 restricted the use of experts as these delayed cases. S14 introduced a 26-week limit on the length of care proceedings, though extensions can be granted in some circumstances.

The National Unit

In April 2015, the Department of Education's (DfE) Children's Social Care Innovation Programme supported the Tavistock and Portman NHS trust and adoption charity Coram to create the FDAC 'National Unit' to scale up the intervention. The National Unit supported nine FDAC sites and closed in September 2018. Further information about the National Unit's implementation is explored in Roberts et al. (2017).

Effectiveness

Early evidence about FDAC was promising. Harwin et al. (2011) found that FDAC parents were more likely to stop misusing alcohol and drugs, and more likely to be reunified with their children relative to a comparison group. This study also provided some evidence that FDAC could provide cost savings by using fewer experts relative to 'business-as-usual' care proceedings.

The 'After FDAC: outcomes 5 years later' study, found that a higher proportion of mothers in FDAC abstained from drugs or alcohol over the five-year follow-up, relative to comparison mothers (Harwin et al., 2016; Harwin et al., 2018). It also found that a significantly higher proportion of FDAC than comparison mothers who had been reunited with their children at the end of proceedings experienced no disruption to family stability at three-year follow-up. Whilst the study compared intervention outcomes to a comparison group, the comparison group was drawn from business-as-usual care proceedings where alcohol or substance misuse was a factor in issuing care proceedings. The study did not construct a counterfactual using other factors, which may have made for a stronger comparison (e.g. by using matching). In addition, this study drew on a relatively small sample size (140 intervention cases; 100 comparison cases).

A study of the London FDAC found that a higher proportion of parents whose case was heard in FDAC had ceased misusing alcohol and drugs by the end of proceedings, and more FDAC than comparison families were reunited with their children. Additionally, proportionately fewer children in FDAC families experienced new neglect or abuse in the first year following reunification (Harwin et al., 2014).

Variation between sites

FDACs also deliver their support differently across sites. All sites deliver support during care proceedings, but some sites (such as Gloucestershire) also offer pre-proceedings or post-proceedings support. Some sites offer peer-mentoring, and overall staffing varies between sites. Additionally, some teams are embedded within Local Authorities (such as Gloucestershire) whilst other multidisciplinary teams are commissioned services delivered by external providers (such as London).

Variation also exists between sites when there is subjective decision making (such as deciding which cases to offer support out of a sample of cases that meet the inclusion criteria).

Rationale for further evaluation of FDAC

Prior evidence suggests that FDAC is a promising intervention for children in families with alcohol or drug misuse. Yet much of the prior evidence about FDAC's effectiveness comes from the London FDAC site. The evidence may also be further strengthened by assessing impact with a stronger counterfactual and with larger sample sizes (to detect smaller effects). Furthermore, these evaluations assessed the effectiveness of FDAC before the reforms introduced by the Children and Families Act 2014, which changed how care proceedings are administered. A separate feasibility study was therefore carried out to establish a research design to evaluate the impact of FDAC on outcomes for children and families across FDAC sites and in light of legislative reform. The design of the current study is the result of this feasibility study.

Intervention and Theory of Change

This section outlines how the intervention is delivered and the Theory of Change (ToC) that was developed with stakeholders during the feasibility stage of this evaluation. Although there is variation in the elements included in FDACs and how they are implemented, the ToC is designed to outline the overarching logic common to the FDAC approach in general. The FDAC logic model and ToC are detailed in Appendices A and B respectively.

Intervention

FDAC provides support to parents to help them overcome their problems to give children the best possible chance of being raised by their own parents. FDAC recognises that very few parents intend to abuse or neglect their children, but that parents fail when they have significant problems. This includes substance and alcohol misuse, domestic abuse, mental health problems and severe poverty.

FDAC is designed to be a ‘problem solving’ court that adopts a less adversarial approach than typical care-proceedings. It follows the principle of therapeutic jurisprudence, empowering families with a stronger voice in care proceedings.

How is the intervention delivered?

Each FDAC site has a dedicated FDAC judge. The FDAC judge has jurisdiction over both care-proceedings and the FDAC treatment intervention. The FDAC judge oversees fortnightly reviewing hearings with the multidisciplinary team. Lawyers do not attend the fortnightly review hearings. The multidisciplinary team provides treatment and support to parents, monitoring their progress and reporting back to the court at the fortnightly review hearings. These hearings aim to solve the problems faced by the parent through an open therapeutic forum.

The staffing of the multidisciplinary team varies across FDAC sites, though the core structure includes substance misuse specialists, social workers and an overall site manager.

When and where is the intervention delivered?

The intervention is delivered during care-proceedings, which typically last up to 26-weeks. In some circumstances, extensions to care-proceedings are granted.

There are fourteen FDAC sites, operating within Local Authorities. A full list of FDAC sites and the Local Authorities each site covers is detailed in Table 1.

Table 1 FDAC sites	
FDAC site	Local Authority covered by FDAC site
Bedfordshire	Central Bedfordshire
	Luton
	Bedford
Birmingham and Solihull	Birmingham City
	Solihull
Coventry	Coventry
	Warwickshire
East Sussex	East Sussex
Gloucestershire	Gloucestershire
Kent	Kent
Leeds	Leeds City
London	Bromley
	Camden
	Croydon
	Kingston

	Lambeth
	Merton
	Richmond
	Sutton
	Wandsworth
Milton Keynes and Buckinghamshire	Milton Keynes
	Buckinghamshire
Newcastle	Newcastle
	Gateshead
	North Tyneside
Somerset	Somerset
Southampton	Southampton City
Stockport	Stockport
Walsall	Sandwell
	Dudley
	Walsall

Variation and iterations

Although the core FDAC model is the same, there are some differences in the implementation of the intervention across sites. For example, some sites have started providing support in pre-proceedings (London and Kent) or post-proceedings (Gloucestershire). Some sites also use peer-mentoring, where successful parents support parents in care-proceedings. This study focuses on the overall effectiveness of FDAC across sites.

Separate evaluations are being conducted on behalf of the What Works Centre for Social Care which will explore these variations in greater detail:

- Peer-mentoring; evaluated by King's College London.
- Post-proceedings support in the Gloucestershire FDAC; evaluated by the University of Sussex.
- Engagement with FDAC using behavioural insights; evaluated by the Centre for Evidence Implementation.

Impact Evaluation

Research questions

The impact evaluation will seek to answer the following questions:

RQ1 What is the impact of FDAC on the likelihood that children are reunited with their parents at the end of care proceedings relative to business-as-usual care proceedings?

RQ2 What is the impact of FDAC on the likelihood that parents continue to misuse alcohol or drugs by the end of care proceedings relative to business-as-usual care proceedings?

RQ3 What proportion of children reunified at the end of FDAC care proceedings are still placed with their parent(s) three years after final court hearing and how does this compare with the national average?

RQ4 What is the impact of FDAC on the likelihood of final care proceedings hearings being contested relative to business-as-usual care proceedings?

RQ5 What is the impact of FDAC on the likelihood of expert witnesses being consulted during care-proceedings relative to business-as-usual care proceedings?

RQ6 What is the impact of FDAC on the placement of the child at the end of care proceedings relative to business-as-usual care proceedings?

Participants

Study participants will be drawn from the thirty-one Local Authorities covered by the fourteen FDAC sites. Participants in the intervention group will be those going through FDAC court proceedings. Control cases will be selected from business-as-usual care proceedings cases.

Eligibility for study inclusion¹ is defined as the following:

- **Intervention** – all cases that have been referred to an FDAC that are live between January 2021 and June 2022 will be considered eligible.
- **Control** – any case that meets the basic criteria for an FDAC referral and sits within an area covered by a Local Authority that has an FDAC but receives business-as-usual care proceedings that are live between January 2021 and June 2022 will be considered eligible.

The basic criterion for FDAC referral is that “Parental substance misuse (drugs or alcohol or both) is a key factor of the Local Authority’s concerns about the child(ren) within a care proceedings case”. This will be captured for FDAC cases from a suite of variables, including:

- Current or historical misuse of drugs or alcohol
- Substance misuse type
- Severity of alcohol misuse
- Severity of drug misuse

This data is not currently systematically collected for non-FDAC cases, but we intend to work with Local Authorities to capture this data for non-FDAC cases based on existing data (for example in case notes).

Additionally, some FDACs have developed their own referral inclusion and exclusion criteria though the variation between sites has never been closely documented. Some FDAC sites exclude cases where parental psychosis or litigation capacity may act as a barrier to parental engagement with FDAC or where there is a history of severe physical or sexual abuse of the child(ren). Furthermore, the selection process likely varies by FDAC site in regard to subjective components that are not included within the criteria. There is therefore likely to be a degree of selection bias, with systematic differences between control and treatment participants.

FDAC case data will be retrieved directly from CJI following the development of a data sharing agreement. This data sharing agreement should be sufficient to enable access to all necessary FDAC data for the purposes of the study. Alternatively, however, separate data sharing agreements may have to be drawn for each of the individual FDAC sites. Further

¹ Note that this refers to RQ1, 2, 4 and 5. RQ3 draws on data from a prior cohort of FDAC participants (2017 and 2018).

data sharing agreements will also be drawn for each of the participating Local Authorities in order to grant access to the necessary control case data.

Design

Table 2 Study design		
Trial type and number of arms		Quasi-experimental design: Coarsened Exact Matching
Unit of identification		Family
Matching variables		Parent demographics, Domestic Violence, Alcohol or drug misuse, Child demographics, Family demographics (see Table 3).
Primary outcome	variable	Reunification
	measure (instrument, scale)	Binary indicator derived from the placement of the child at the end of care proceedings.
Secondary outcome(s)	variable(s)	1) Alcohol and drug misuse cessation; 2) Contested final hearing; and 3) Use of expert witnesses.
	measure(s) (instrument, scale)	1) Instrument to be confirmed; 2) Binary indicator of whether the final hearing was contested; 3) Binary indicator of whether expert witnesses were used during care proceedings.

A randomised controlled trial was considered for this evaluation but was rejected as the judiciary thought that randomisation of families in care-proceedings could be subject to legal challenge. A feasibility study was conducted as part of this evaluation considering suitable evaluations. The impact evaluation, therefore, uses a quasi-experimental design: Coarsened Exact Matching (CEM).

Identification strategy

The intervention group will consist of cases that have been selected by an FDAC site to receive FDAC care proceedings. A counterfactual, or control group, will be selected from business-as-usual care proceedings cases that meet the basic criteria for inclusion in an FDAC but were not selected for FDAC despite being within a Local Authority that has an FDAC. The basic criteria for inclusion in FDAC are that care proceedings have been issued and that there are concerns about parental alcohol or drug misuse as part of the care proceedings case. This method will involve constructing the counterfactual by combining a CEM approach, as described by Iacus et al. (2009), with regression analysis.

Implementing CEM requires covariate data for characteristics associated with selection into the intervention or the outcome at the start of care proceedings. For example, this could include risks to the child (such as parental alcohol or drug misuse or domestic violence in the household). Covariates will be “coarsened” into binary or categorical variables (for example, if it is a continuous variable, such as the age of the child, the variable would be re-categorised into age bands). Some covariates that are already collected in FDAC sites are currently collected categorically and will not require coarsening. For example, parental alcohol and drug misuse at the start of proceedings are assessed using clinical judgment

(high, medium and low risk). Table 3 outlines the variables collected and how they will be coarsened for matching. A sensitivity analysis will coarsen these covariates (by further collapsing coarsened covariates).

The sample will then be reduced to just observations that have at least one observation for both groups (intervention and control) for unique combinations (strata) of coarsened covariates. To maximise the likelihood of including all treatment cases, we aim to use a larger sample of control cases relative to the intervention group. This approach is typical for matched study designs. Control cases in strata without intervention cases will be removed from the sample. Observations will be weighted to ensure that the number of intervention and matched-control observations within strata is the same. The impact is estimated by comparing the outcomes of the intervention group with the outcomes of the matched-control group.

A CONSORT flow diagram of losses and exclusions will be provided in the evaluation report.² If intervention cases are lost at the matching stage, it would indicate that there were not suitably similar observations in the control group. This would limit the generalisability of the study findings.

The unit of analysis varies across outcomes. The primary outcome (reunification) is defined at the child-level, but some secondary outcomes (such as parental alcohol or drug misuse) are defined at the family-level. Matching will be implemented separately for outcomes at different units of analysis. Child-level outcomes will include covariates about the child (such as age), parent (such as alcohol and drug misuse at baseline) and family characteristics (such as the number of children in the family). Parent-level outcomes will include all these covariates except for child-level characteristics, which will be aggregated to family level characteristics.

After cases have been matched, covariate balance will be assessed by comparing the characteristics of the intervention and control groups before and after matching. Comparisons will be made using the covariates in an uncoarsened state, with differences reported as *Hedge's g* effect sizes.

If we observe an imbalance with an effect size of greater than 0.05, we will revise the matching specification by adjusting the coarsening of any variables with an imbalance of greater than 0.05. We will reduce the number of coarsening categories (unless the variable in question is already uncoarsened). If this does not resolve the imbalance, we will coarsen the variable further instead.

Data sources and availability

The data to be used for matching and the evaluation of outcomes will be collected from several sources:

- The FDAC data collection tool, for FDAC cases
- Local Authority case management systems, typically within children's social care and legal teams

² Although the CONSORT *Statement* was originally developed to guide the reporting of RCTs, many of its components also apply to other types of quasi-experimental impact studies. A flow diagram template will be downloaded from <http://www.consort-statement.org/>.

- Case notes, typically held by Local Authorities and courts

Data collected as part of the FDAC data collection tool will be the source of data for intervention cases. Ideally, data for the control group would be collected from the same data source, however Local Authorities do not systematically collect all the information collected by FDAC sites. Local Authorities will be asked to provide comparable data from their own case management systems and case notes based on a template developed by NatCen. As far as is possible, we will aim for comparable data collection. For example, we will collect data from fields used in statutory returns, such as those collected in the CIN census. Fields collected by control sites will be more limited relative to intervention sites. For example, drug misuse will be based on data collected by Local Authorities, which may not be as accurate as the testing conducted in FDAC sites (e.g. drug testing using samples of parent's hair).

Our data collection template that will be completed by the Local Authorities will include detailed instructions and guidance for all key fields required to complete this evaluation. For example, there will be guidance for clinical judgements on substance misuse severity, into three categories: low, medium and high. For each of these categories, a detailed description would explain to LAs how to classify each case. For example, intravenous (IV) drug use would fall into the high drug misuse category, where the use of prescription drugs such as zopiclone, diazepam, co-codamol would fall into the low drug category. Furthermore, there will be cell validation in most fields of the designed data collection template in order to minimise blank cells and ensure data comparability across Local Authorities. We will only be able to use fields collected in both FDAC sites and Local Authorities in the matching.

Key characteristics that we intend to include in the matching are illustrated in Table 3. Each of these characteristics are collected in both the FDAC data collection tool and the control group data collection tool. These variables are important indicators of whether a case is suitable for FDAC (such as type and severity of drug and alcohol misuse). However, it cannot account for subjective criteria that may be used by FDAC sites in determining which cases should be supported by FDAC. Table 3 also outlines how specific variables will be coarsened in matching.

We intend for Local Authorities to complete this data tool using data from their own case management systems and case notes. However, there is a risk that some items (particularly if instruments need to be administered by clinicians) may not be available.

Table 3 Intended variables for matching intervention and control cases and coarsening strategy

Parent characteristics	Variable	Type	Coarsening Strategy
Demographics	Age	Continuous	Age bands: Less than 18 years old, 18-24, 25-34, 35-44, 45 or older
	Ethnicity	Categorical	No coarsening: White, Asian/Asian British, Black African/Black Caribbean/Black British, Mixed/multiple ethnic groups, other, unknown
	Number of children	Continuous	Bands: 1, 2, 3 or more
	Age of youngest child in the household	Continuous	Age bands: Less than 12 months old, 1-2, 2-3, 4-7, 8-11, 12-16
	Accommodation status	Categorical	Coarsened: Owner occupier, Tenant (private or social) Supported housing/hostel/refuge, homeless, other
Domestic abuse	Past experience of domestic abuse	Categorical	No coarsening: Yes – perpetrator, Yes – Victim, Yes – both, No, Unknown
	Currently experiencing domestic abuse	Categorical	No coarsening: Yes – perpetrator, Yes – Victim, Yes – both, No, Unknown
Background	Former looked after child	Binary	No coarsening
	Any previous child(ren) removed	Binary	No coarsening
	Any criminal convictions or cautions	Binary	No coarsening
Substance misuse	Whether misusing at time of referral	Binary	No coarsening
	Substance misuse type	Categorical	No coarsening: Drugs, Alcohol, Drugs and Alcohol, None, Unknown
	Severity of alcohol use (clinical judgment)	Categorical	No coarsening: None, Low, Medium, High, Unknown

	Severity of drug use (clinical judgement)	Categorical	No coarsening: None, Low, Medium, High, Unknown
Mental health	DSM/ICD Diagnosis	Open text	Binary: Yes – any recorded diagnosis, No, Unknown
Child characteristics	Age	Continuous	Age bands: Less than 12 months old, 1-2, 2-3, 4-7, 8-11, 12-16
	Ethnicity	Categorical	No coarsening: White, Asian/Asian British, Black African/Black Caribbean/Black British, Mixed/multiple ethnic groups, other, unknown
	Previously looked after	Binary	No coarsening: Yes, No, Unknown
	Subject to an order	Binary	No coarsening: Yes, No, Unknown
	Ever had a criminal conviction	Binary	No coarsening
	Issue around school attendance	Categorical	Yes, No, Unknown, not school age.
	Education, Health and Care (EHC) Plan	Categorical	Yes, No, Unknown, not school age.
Case characteristics	Date of issue	Date	Year and quarter

Minimum detectable effect size calculations

Power calculations were conducted in Stata 16.1 based on the anticipated matched sample size, using formulae from Dong and Maynard (2013). These formulae are presented in Appendix D. The total number of control cases will exceed the numbers prior to matching. Cases are then weighted so that there are equivalent intervention and control cases, so we assume equal sample sizes at analysis. Our assumptions are:

- Clustering of children within parents, with an ICC of 0.70
- No clustering of children within Local Authorities
- Variance explained by the uncoarsened covariates used in a regression model after matching, with an R-squared of 0.20 at level one and 0.10 at level two. We estimate this to provide a correlation of 0.45 and 0.32 respectively³
- That 25% of children in control are reunified with their parents by the end of care-proceedings (informed by Harwin et al., 2018)
- A type one error rate of 0.05
- Power of 0.80 (a type two error rate of 0.20)
- Two tailed significance testing

There is some uncertainty on the expected sample size. Based on Harwin et al. (2018) we anticipate that each FDAC case will have approximately 1.40 children. Our assumptions use this figure and build on estimates of expected caseloads for the implementation period (January 2021 – June 2022) collected by FDAC sites by CJI in Summer 2020. Based on updated figures concerning expected caseloads, we anticipate a sample of 300 intervention children from 215 families.

Based on these assumptions, we expect the evaluation will be powered to detect a relative risk ratio of 1.36 (or equivalent to a 9.1 percentage point difference). No power calculations are conducted for secondary analyses, but these will have lower power as the unit of analysis is at the family-level. However, as the intracluster correlation coefficient is relatively large, the reduction in power will be relatively small.

Table 4 Minimum detectable effect size calculation		MDES (Proportion of a Standard Deviation)
Relative Risk Ratio		1.36
Baseline/Endline correlations	Child	0.45
	Family	0.32
	Social Worker	0.00
	Family	0.70

³ Our sample size calculations include estimates of the proportion of variance explained through the included covariates at each of these levels (R-squared). We have converted these into pre- post-test correlations by taking the square root of the R-squared value.

Intracluster correlations (ICCs)	Social Worker	0.00
	Team	0.00
Alpha		0.05
Power		0.80
One-sided or two-sided?		2
Level of intervention clustering		Family
Average cluster size		1.40
Sample Size (children)†	Intervention	300
	Control	300
	Total	600
Sample Size (families)	Intervention	215
	Control	215
	Total	430

† Of which, we assume half are intervention children and half are matched control children

Outcome measures

The primary and secondary outcomes will be sourced from the FDAC data collection tool for intervention cases and Local Authority administrative data for control cases. Local Authorities will already collect data on reunification but may not systematically capture parental alcohol or substance misuse, contested final hearing and use of expert witnesses. This will be included in the data collection template prepared by NatGen.

The primary outcome of interest will be a binary indicator of reunification immediately at the end of care proceedings. We define reunification as the legal order given for the child to either return to live with the parent, or to continue to live with the parent. Reunification is not achieved where the placement of a child at the end of care proceedings is different from the start of proceedings. This includes placement with another parent or family member at the end of care proceedings. For FDAC cases, the judge's ruling on the placement of the child will be recorded in the FDAC tool by FDAC staff. We expect the placement of the child would be recorded by Local Authorities and this will be captured in the data collection template for non-FDAC cases.

The secondary outcomes are parental alcohol and drug misuse cessation, whether the final hearing is contested, and whether expert witnesses were used during care proceedings.

Parental alcohol and drug misuse are currently recorded in the FDAC data tool as two key categorical variables: the severity of parental substance misuse (low, medium, high) and the level of risk to the child from parental substance misuse (low, borderline, harmful). We anticipate that this will be recorded differently in the new tool – using a suite of binary indicators that used to be fed into the clinical judgement that is currently used. For example, whether a drug is taken intravenously (IV) or whether cannabis use is occasional or heavier (more than once a day, or large quantities). The final outcome variable will be a binary indicator of whether the parent is currently misusing drugs or alcohol at the end of care proceedings (where one indicates they are currently misusing and zero indicates they are not currently misusing).

As we are interested in the impact of FDAC on the likelihood of final care proceedings hearings being contested relative to business-as-usual care proceedings (RQ4), contested final hearings will be recorded as a binary outcome. The final hearing will be classified as contested regardless of which party contests the hearing.

Similarly, as we are interested in the impact of FDAC on the likelihood of expert witnesses being consulted during FDAC care-proceedings relative to business-as-usual care proceedings (RQ5), the use of expert witnesses will be defined as a binary variable. If the number of witnesses is recorded, then this will be dichotomised.

Long term reunification will also be assessed. This will be defined based on a return to court for care proceedings within three years of reunification at the end of care proceedings. To address this research question (RQ3), we will use long term reunification data obtained from Cafcass. This data has detailed accounts of FDAC care proceedings cases from 2017/18. We will also assess the final legal order from the return to court.

Finally, we will also consider the placement of the child. We will conduct an additional analysis (RQ6) with a categorical outcome variable that indicates whether the child is placed with their parents, living with another relative of in LA care, rather than as a strict binary outcome indicating whether cases resulted in reunification or not.

Analysis plan

Primary Analysis

Matching

The primary analysis will estimate the impact of FDAC on reunification at the end of care proceedings based on the placement of the child. CEM will be conducted at the child-level using the characteristics identified in the Identification Strategy section. The matching is conducted at parent and child-level, as the placement of children at the end of care proceedings can vary for different children in a care proceedings case. Therefore, the primary analysis seeks to match children in FDAC care proceedings with similar children in business-as-usual care proceedings.

The matching will be conducted using the user-written package *cem* in Stata 16.1 SE (Blackwell et al., 2010), which implements CEM as described in Iacus et al. (2009). The primary analysis will be conducted on an intention-to-treat basis where the outcome is non-missing. Where covariate data is missing, the *cem* package matches cases that are missing data on the same covariates. If missing outcome data exceeds five percent of the intervention sample, we will consider conducting a sensitivity analysis using multiple imputation (see missing data analysis).

Any intervention cases excluded from the analysis because of common support (i.e. that there are no cases with the same coarsened characteristics in the control group) will be transparently reported using a CONSORT flow diagram. If more than five percent of intervention cases are lost because of issues with common support, we will consider adjusting how covariates are coarsened. This would involve collapsing categories of coarsened covariates and altering bin sizes. As a first step we would coarsen the following variables further:

- Child age bands: Less than 4, 5-11, 12-16
- Parental accommodation status: Owned, Tenant (private or social), other
- Parent ethnicity: White, Black, Asian or other minority ethnicity, other, unknown
- Child ethnicity: White, Black, Asian or other minority ethnicity, other, unknown

Covariate balance will be assessed before and after matching. The difference between intervention and control group characteristics (as measured by the uncoarsened covariates) will be presented using *Hedge's g* effect sizes, using the *esize* command in Stata 16.1.

Analysis

A “doubly robust” estimation of causal effects will be estimated for the matched sample, applying the weights assigned during the matching, including a binary indicator of allocation to FDAC, while also including the uncoarsened matching covariates in the regression model, following Funk et al. (2011). The “doubly robust” estimation reduces the risk that the ITT is biased, provided that either the matching (modelling exposure to the intervention) or the regression model (describing the relationship between the dependent and independent variables) is well specified (Funk et al., 2011). To account for the clustering of children within sites, the regression model will include fixed effects for each site.⁴ The full model notation is as follows:

$$Reunification_{ijk} = \beta_0 + \beta_1 Intervention_{jk} + \beta_2 Site_k + \beta_3 X_{jk} + e_j$$

Where children (i) are nested within families (j) within sites (k). The vector X_{jk} denotes the characteristics used in the matching as covariates. The error term is represented by e_j . We will use cluster-robust standard errors to account for the clustering of children within parents. As the outcome is binary, the effect size will be estimated as a relative risk ratio, with 95% confidence intervals.

Additional Analysis

We will conduct two sensitivity analyses:

- A logistic regression model will be estimated for the matched sample, applying the weights assigned during the matching, including a binary indicator of allocation to FDAC or business as usual, while dropping the characteristics used in the matching as covariates; and,
- Adjusting the cut-off points when coarsening covariates for the matching

The first sensitivity analysis will be estimated using a similar approach to the primary analysis, but, this time, excluding the characteristics used in the matching in the regression model:

$$Reunification_{ijk} = \beta_0 + \beta_1 Intervention_{jk} + \beta_2 Site_k + e_j$$

The coarsening of covariates is potentially a subjective decision for the trial analyst that could impact upon the matching and the subsequent effect estimate. Therefore, we consider alternative coarsening strategies (e.g. a greater number of age bands or separating different minority ethnicities). Provided the primary analysis is conducted as planned, the alternative specification will further coarsen covariates, as outlined in the “matching” section.

Finally, we will also assess the impact of FDAC on the placement of the child, using a categorical outcome variable that indicates 1) reunification with the child's parents 2) placement with another relative, or 3) LA care. This will be assessed using a multinomial logit regression, with cluster-robust standard errors to account for the clustering of children within families.

⁴ Control cases will be allocated to the FDAC site that serves their Local Authority.

$$Placement_{ijk} = \beta_0 + \beta_1 Intervention_{jk} + \beta_2 Site_k + \beta_3 X_{jk} + e_j$$

As with the primary analysis, we include the uncoarsened covariates used in matching. We will present effect sizes as Relative Risk Ratios, with 95% confidence intervals.

Missing data analysis

If greater than five percent of cases are missing outcome data on the primary analysis, it is likely that missingness may impact the results of the evaluation and we will therefore consider conducting additional analysis for the primary outcome.

Firstly, we will assess if missing data can be predicted using observed characteristics using a 'drop out' model. The dependent variable will be a binary indicator of missing data on the primary outcome. Independent variables will include all (uncoarsened) covariates used in the matching model. Additional categories will be added to ensure that cases with missing data on independent variables are included in this model.

If this model finds statistically significant associations (a p-value of less than 0.05) between observed characteristics and the dependent variable, we will assume that data is missing at random (MAR).⁵ If we assume data is MAR we will conduct a sensitivity analysis using multiple imputation.

Multiple imputation by chained equations (MICE) will be estimated in Stata 16.1 using the *mi* suite of commands. The first 200 observations will not be used ('burn in') to ensure that a stable distribution has been reached. In total, 75 datasets will be imputed. The imputed values will be used in the matching model by using the *impvar* option of the user-written *cem* package used for the primary analysis.

Secondary Analysis

The secondary analysis will assess the impact of FDAC on three outcomes:

- Parental alcohol and drug misuse cessation
- If the final hearing was contested
- Whether expert witnesses were used (and the number of expert witnesses).

Matching for these outcomes will be conducted at the family (or case) level using the covariates outlined in Table 3. A separate matching model is used to assess the impact on these outcomes to the primary analysis. Unlike the primary analysis, the unit of analysis for these outcomes is at family (or case) level. We therefore want to match similar parents (or cases) rather than children within cases.

The approach will be consistent with the primary analysis, using the same user-written *cem* package in Stata 16.1. Common support and covariate balance will also be reported consistently with the primary analysis.

Each of these outcomes will be analysed as binary variables with the unit of analysis at family (or case) level. If the use of expert witnesses is recorded as a count variable (i.e. the number of witnesses used) we will dichotomise the variable for this analysis. These

⁵ By definition, it is not possible to assess if there are associations with unobserved characteristics. If there were associations with unobserved characteristics, the data would be described as missing not at random (MNAR). In this case, both the primary analysis and multiple imputation would produce biased estimates. A full description of types of missing data and their consequences are available in the WWCS statistical analysis guidance.

outcomes will therefore be analysed using a logistic regression model, using an approach consistent with the binary analysis:

$$Cessation_{jk} = \beta_0 + \beta_1 Intervention_{jk} + \beta_2 Site_k + \beta_3 X_{jk} + u_j$$

$$Contested_{jk} = \beta_0 + \beta_1 Intervention_{jk} + \beta_2 Site_k + \beta_3 X_{jk} + u_j$$

$$Experts_{jk} = \beta_0 + \beta_1 Intervention_{jk} + \beta_2 Site_k + \beta_3 X_{jk} + u_j$$

Effect Size Estimation

The outcomes for this study are binary, and will therefore be estimated as relative risk ratios, with the following formula:

$$RRR = \frac{\text{Risk of event in intervention}}{\text{Risk of event in control}} = \frac{\frac{a}{(a+b)}}{\frac{c}{(c+d)}}$$

Where (a) is the probability of failure in the intervention group, (b) is the probability of success in the intervention group, (c) is the probability of failure in the control group and (d) is the probability of success in the control group.

Exploratory Analysis

Additional analysis will be conducted to assess whether reunification can be sustained over time, as prior evidence (Harwin et al., 2019; Broadhurst et al., 2017) indicated that the greatest risk of returning to court for care proceedings are the first two years after reunification.

A prior cohort (2017 and 2018) of FDAC participants will be identified by FDAC sites. They will then share identifiers with Children Family Court Advisory and Support Service (Cafcass). Cafcass will then identify if children were returned to court for care proceedings in the subsequent three years. The analysis does not compare to a counterfactual as it will be drawing on historic data, and we do not expect comparable data for a control group to be available. This means a counterfactual cannot be constructed with matching. As this analysis will not use a counterfactual, it will not provide a causal claim, but we will compare to a national average.

We are also interested in assessing whether people who experience racism have different outcomes than those who do not. If the available data allows, we will also separate estimates for White FDAC participants and all other FDAC participants. We understand that different ethnicities may experience different impacts, and that White/non-White may mask underlying differences between different groups. However, we do not anticipate that the available sample sizes would support robust estimates for separate minority ethnicity groups.

Contextual Factors Analysis

This evaluation includes FDAC sites across multiple Local Authorities. Some FDAC sites will have been operating for over a decade, whilst others will only have launched in 2020. There are also differences in delivery models across sites. This will be explored as part of the Implementation and Process Evaluation (IPE).

In addition, Covid-19 has had a significant impact on social care, both within FDAC sites and in Local Authorities more generally. The impact of Covid-19 varies across some sites. Existing sites have not been able to take on new cases or have had staff diverted to other social care work. New sites have had difficulties with recruitment and have had to delay their launch dates. At this stage, the full impacts of Covid-19 on FDAC sites are not known, but further contextual information will be provided in the evaluation report.

This evaluation will not be powered sufficiently to estimate variation in the effect across sites, but variation in implementation will be explored across sites as part of the IPE.

Implementation and process evaluation

The IPE will use a qualitative methodology; interviews in sampled case study areas, to understand the advantages and disadvantages of particular FDAC iterations and variations and gather information on barriers and facilitators to successful implementation and delivery. It will also explore how perceived impacts from the FDAC courts compare with business-as-usual proceedings. Drawing on the work undertaken for the feasibility study to refine the programme theory for the FDAC model, the IPE will also explore (see Appendix C):

- Views and experiences of implementing FDAC;
- Awareness, understanding and commitment to the FDAC approach;
- Parental engagement, views and experiences of court proceedings;
- Staff and practitioner views and experiences of delivering the model, working across multidisciplinary teams and engaging families; and
- Views on the perceived impact of FDAC on families, the courts, practitioners and wider CJS.

With stakeholders and staff, data collection will focus on commitment, buy-in and understanding of FDAC, views on whether the necessary procedures, processes and resources are in place, the skills and competencies of professionals to effectively deliver FDAC (including for example, selection to receive FDAC), the effectiveness of judicial oversight and partnership working and views on parental engagement, experiences and skills development.

With parents, data collection will include an understanding of and engagement with FDAC, experience of court proceedings and support offered through FDAC, perceptions of change related to skills, competencies and behaviours around for example, accessing support, relationships with children and managing safety and wellbeing, views on fairness and perceptions of impact (including key drivers).

A qualitative approach that provides in-depth information on the implementation, delivery and perceived impact of FDACs will complement the impact evaluation and offer explanations for observed effects – this will help to unpack what works by identifying key facilitators and barriers. A range of dimensions of the implementation will be assessed, including:

- *Intervention fidelity reach and dosage* – exploring how the programme has been implemented and delivered, whether and how it was delivered to the intended population and the extent of support and contact this group received.⁶
- *Quality of delivery* – data will be gathered from participants on their experiences of delivering and receiving support through FDAC.
- *Variations in FDAC delivery* – analysis of interview data will enable a comparison of implementation and delivery issues across different models, to understand facilitators and barriers, drivers of success and will help to draw out learning.
- *Participant responsiveness* – engagement with the FDAC aims, court proceedings, treatment services and other activities will be explored through data collection with staff and parents themselves.
- *Ability to differentiate the programme from business-as-usual activities* – where appropriate, participants will be asked to reflect on how implementation and delivery

⁶ The FDAC ‘model’ is delivered differently depending on local needs and infrastructure. ‘Fidelity’ is therefore better understood as being to a set of core elements than a fully articulated model.

experiences differ from business-as-usual. We will also collect data from business-as-usual sites to compare views and experiences directly.

Methods

The IPE will take a case study approach in FDAC sites which will comprise of interviews with key stakeholders, staff and beneficiaries, including for example the judiciary, LA leads, support staff (e.g. substance misuse specialists) and parents who come before the court. We will also conduct interviews with key staff in four non-FDAC 'business as usual' courts, to enable us to gain a good understanding of how process differ and key facilitators and barriers to delivering FDAC in different areas.

A brief overview of the rationale for this approach, including some preliminary thoughts on sampling and recruitment is set out below. Depending on the scope of the IPE, it may be valuable to increase interview numbers across case study and non-FDAC sites, which would offer more detailed insight into the implementation and delivery of the pilots.

FDAC case studies

We will carry out six case studies (around 36 interviews in total) across sites. Case study areas will be sampled for diversity across:

- FDAC start dates
- The model of delivery
- The iterations included in the FDAC model
- Volume of FDAC cases / throughput
- We would also aim to get diversity in terms geographical location, local demographics and size of the court.

Case study courts will be selected in close partnership with WWCS and the CJI, drawing on the detailed knowledge that CJI have on set-up and progress on FDACs, especially in light of Covid-19.⁷

This case study approach will allow us to explore similarities and differences across the various models of delivery and will include interviews with key stakeholders, staff and beneficiaries, including for example the judiciary, local authority leads, support staff (e.g. substance misuse specialists) and parents who come before the court. The exact breakdown of interviews across these groups will likely vary across the case studies, depending on the model of delivery, however, we would aim to achieve a good spread across the groups and agree a final sampling strategy with WWCS and CJI when planning fieldwork towards the end of 2020. An early indication of how interviews might be spread across each case study area is included in the table below.

Table 5 Interview quotas across participant groups		
Participant Group	Number of interviews within case study	Total number of interviews across sites

⁷ Covid-19 has impacted variously on FDACs, delaying set-up and launch in some areas. We will need to be mindful of this when selecting case study courts for the IPE to ensure as much learning as possible can be gathered from the qualitative data collection.

Judiciary (e.g. judges, magistrates, court clerks etc.)	1	6
FDAC team leads/ Local authority leads (e.g. staff working with parents to deliver FDAC – aim to achieve diversity across front line and management roles)	2	12
Support organisations (e.g. specialist domestic violence, substance misuse providers etc.)	1	6
Parents (aim to achieve diversity across parent demographics, for example, family composition, past experience of court proceedings, level of engagement with FDAC etc.)	2	12

Interviews with non-FDAC sites

Alongside the work carried out with case study FDAC courts, we also propose to conduct interviews with key staff in four non-FDAC ‘business as usual’ courts (up to eight interviews in total). This will enable us to gain a good understanding of how process differs in non-FDAC courts, including key facilitators, barriers and perceived impacts.

The selection of the non-FDAC sites would be conducted in close collaboration with WWC to ensure sites share some of the key characteristics with the FDAC case studies (e.g. local demographics / geographical location / size of court) for comparison.

Recruitment

Recruitment and fieldwork activities should be coordinated and clearly communicated to those involved to minimise burden on FDAC sites and partners. We suggest that a main lead (or ‘link person’) is identified at each case study to support recruitment. This individual will be responsible for liaising with other staff, teams and organisations for recruitment purposes and will be fully briefed by the research team about sampling and recruitment strategies, which can be relayed to others as necessary. The process for identifying and inviting individuals to participate, will likely include the following steps:

- The link person will be sent a concise briefing note explaining how we would like them to help with the evaluation and the process of identifying and recruiting participants. This will be followed up with a phone call to check understanding, identify any potential issues and discuss solutions.
- Individuals who are identified as eligible to take part in the research will be given an information sheet supplied by the research team. All recruitment materials will be clear about: the basis and purpose of the study; who is funding the research; how people can opt-in/out of involvement at no personal cost; confidentiality and caveats to this (for example disclosure), and information about NatGen.
- Those interested in taking part will be asked to either consent to have their contact details passed to the research team or for an interview to be arranged.

As consent is an ongoing process, the research team will revisit the information given at the recruitment stage and gain informed consent before the start of all interviews. Our experience indicates that selection bias can be an issue when recruiting some participant

groups (especially parents in this study). We have addressed this on previous studies by initially oversampling people to allow the research team to select individuals who best meet the sample criteria and recommend using this approach here.

For non-FDAC sites, where we do not have contacts through CJI, we will approach the family courts selected directly to inform them of the purpose of the study and invite them to participate. We will utilise publicly available information to do this and follow up as appropriate.

Data collection

We will draw on our extensive track record of delivering high quality and ethically sound research to inform the approaches we use to introduce the research, encourage participation, and collect data. This will include the development of clear and comprehensive recruitment materials setting out the research requirements and parameters of participation to support informed consent.

Qualitative data collection will be led by experienced members of the research team, who will draw on a range of techniques to collect the highest quality of data, including using active listening and responsive, open questioning to build rapport. Interviews will be supported by a topic guide which will provide an overview of key themes to be discussed with each participant. All recruitment and fieldwork materials will be developed and refined in close collaboration with you to ensure they fully meet the research objectives.

Analysis and reporting

Interviews will last up to 60 minutes and will be carried out via phone, web or face-to-face, depending on participant preference and social distancing requirements at the time of data collection. Interviews will be audio-recorded and transcribed verbatim to facilitate robust analysis.

All recordings will be managed using NatCen's Framework approach. This will involve managing interview data and conducting case- and theme-based analysis. Key topics emerging from the transcripts will first be identified. A thematic framework will then be developed and used to organise the data from each participant. Then the coded data will be reviewed in detail, drawing out the range of experiences or views, identifying similarities and differences, developing and testing hypotheses, and interrogating data to seek to explain patterns and findings.

Based on the systematic analysis of the IPE data, a short report will be drafted and submitted to the WWCS and CJI in the Autumn of 2021. The report will highlight key findings thematically, examining similarities and differences between case study areas and participant groups. A summary of IPE findings will also be included in the final report, due to be submitted in September 2022.

Cost evaluation

Costs will be estimated based on the delivery costs of the intervention. Each FDAC site manager will be provided with pro-forma to complete in Autumn 2021 covering costs incurred in the last twelve months. This will assign costs to different categories, such as staff costs, fixed costs (e.g. purchase of equipment), marginal costs (such as rent) and set-up

costs (where appropriate for newer sites, which may include staff recruitment and training costs).

Sites will be asked to provide this data for a one-year period. The set-up costs will be estimated only for sites that have launched since March 2020. Set-up costs will be averaged over three years to provide a more realistic estimate of the annual running costs of FDAC. This is because interventions tend to have higher costs in the first year of delivery that would otherwise provide an unrealistic estimate of what the ongoing costs for the intervention would be. Costs will be estimated from the perspective of the LA on a per-child basis by dividing the total annual cost by the number of children who go through FDAC care proceedings.

Ethics & Participation

Ethical clearance was sought from NatCen's ethics committee in June 2020. Ethical approval was granted on 25th June 2020 subject to the condition of agreeing safeguarding procedures with Local Authorities and providing support for NatCen staff working on the evaluation.

Further ethical clearance will be requested from NatCen's ethics committee for the IPE in December 2020, when plans and timings for fieldwork, (including the selection of case study areas) have been agreed. We will also seek ethical approval from the judiciary in order to conduct interviews with judges and magistrates for the IPE. Once case study areas have been decided, applications will be submitted to the relevant Head of Division or the Senior Presiding Judge, as stipulated in the Courts and Tribunals Judiciary guidance.⁸ We are experienced in seeking ethical approval to carry out research with members of the judiciary, which will help us to prepare applications and requests for these approvals.

Registration

The study has been pre-registered on the Open Science Framework on December 4th, 2020. The registration can be found here: <https://osf.io/w7zac>.

Data protection

NatCen will be both a data controller and a data processor on this evaluation.

Local Authorities and FDAC sites will be data controllers for the data they provide for the impact evaluation, whilst NatCen will be a data processor. It is the responsibility of the data controller to decide on the legal basis for data sharing. At this stage we anticipate that the legal basis for data sharing is "legitimate interests" with special category personal processed for scientific research purposes.⁹

NatCen will be data controller for personal data collected as part of IPE. For all data collection encounters we conduct, we will provide detailed information on what participation entails and we will invite people to participate in the study with their fully, informed consent. All participants will be given a copy of the privacy notice which will provide further information on how we will use the data we collect for the IPE, what their rights are a research participants and how they can withdraw their data from the study if they wish.

⁸ <https://www.judiciary.uk/publications/judicial-participation-in-research-projects/>

⁹ General Data Protection Regulation (GDPR) Article 6(1) and Article 9(2[j]).

Personnel

Table 6 Evaluation team		
Name	Title	Role
Robert Wishart	Research Director, Evaluation	Principle Investigator and impact evaluation lead
Ellie Roberts	Research Director, Crime and Justice	IPE lead
Professor Judith Harwin	Professor of Socio-Legal Studies	Advisor for impact evaluation and IPE
Dr Bachar Alrouh	Advisor	Advisor for impact evaluation and IPE
Dr Kostas Papaioannou	Senior Researcher (Analyst), Evaluation	Impact evaluation
Sarah Sharrock	Senior Research, Crime and Justice	IPE
Ben Stocker	Researcher (Analyst), Evaluation	Impact evaluation

Timeline

Table 7 Timeline		
Dates	Activity	Staff responsible/leading
December 2020	Evaluation registered on the Open Science Framework	Robert Wishart
March 2021	Data sharing agreements with all Local Authorities and sites	Robert Wishart
November - December 2020	Select case study areas, agree sampling strategy and submit ethical approval to Judiciary	Ellie Roberts
January 2020	Data sharing agreement and research approval from Cafcass	Robert Wishart
January 2020	Fieldwork materials drafted (to include gatekeeper briefings, information sheets, support leaflets, privacy notices and web text for different participant groups and topic guides)	Ellie Roberts
January – February 2021	Liaise with sites to set up fieldwork and begin recruitment	Ellie Roberts
March - July 2021	Conduct qualitative fieldwork	Ellie Roberts
June - August 2021	Data management and analysis	Ellie Roberts
August – September 2021	Report on IPE findings drafted and submitted to WWCS and CJI	Ellie Roberts
Spring 2021	Pro-forma provided to FDAC sites for cost analysis	Robert Wishart

June 2022	Data returned from all sites	Robert Wishart
June - August 2022	Analysis and reporting	Robert Wishart, Ellie Roberts
November 2022	Final report submitted	Robert Wishart, Ellie Roberts

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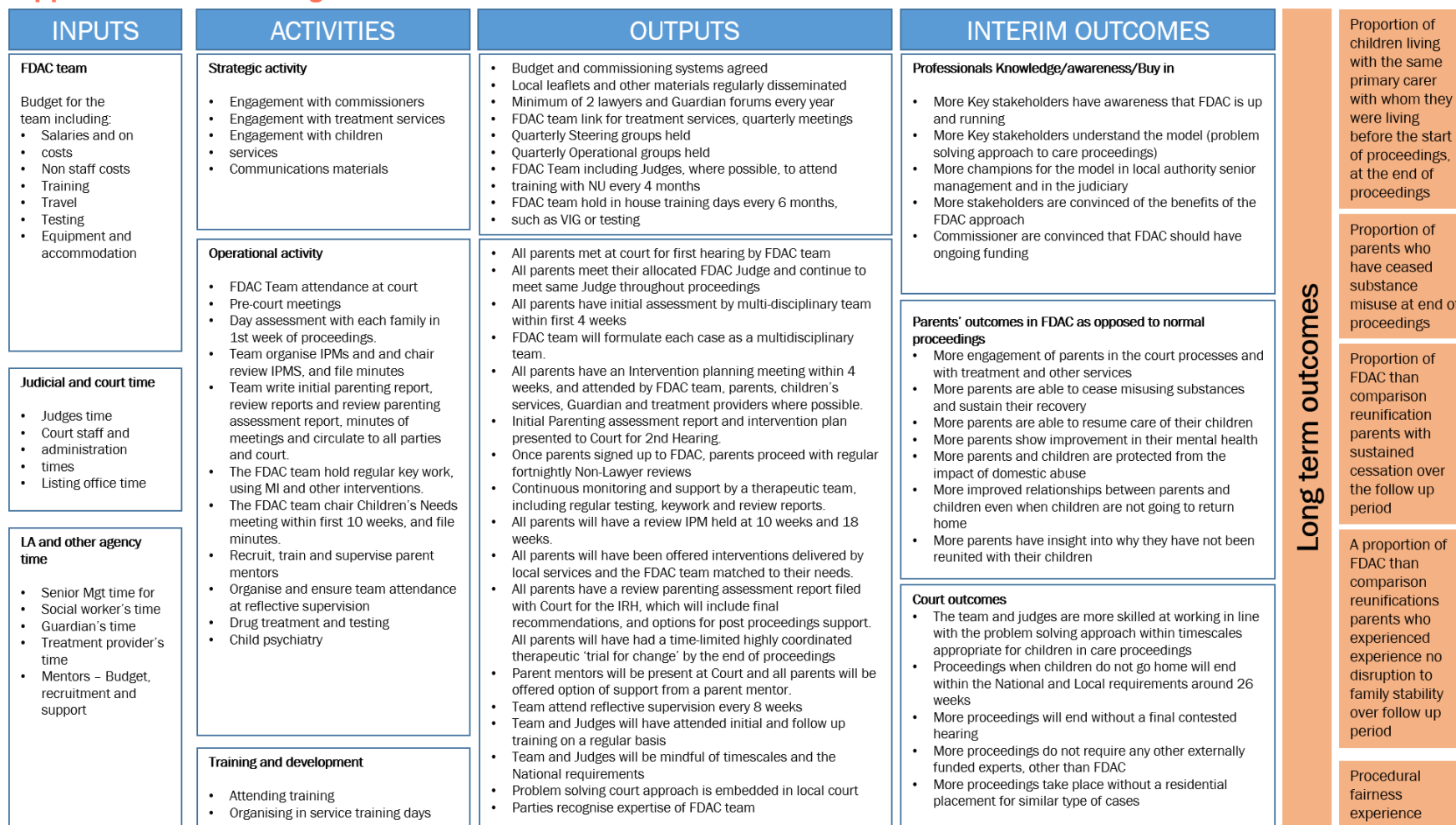
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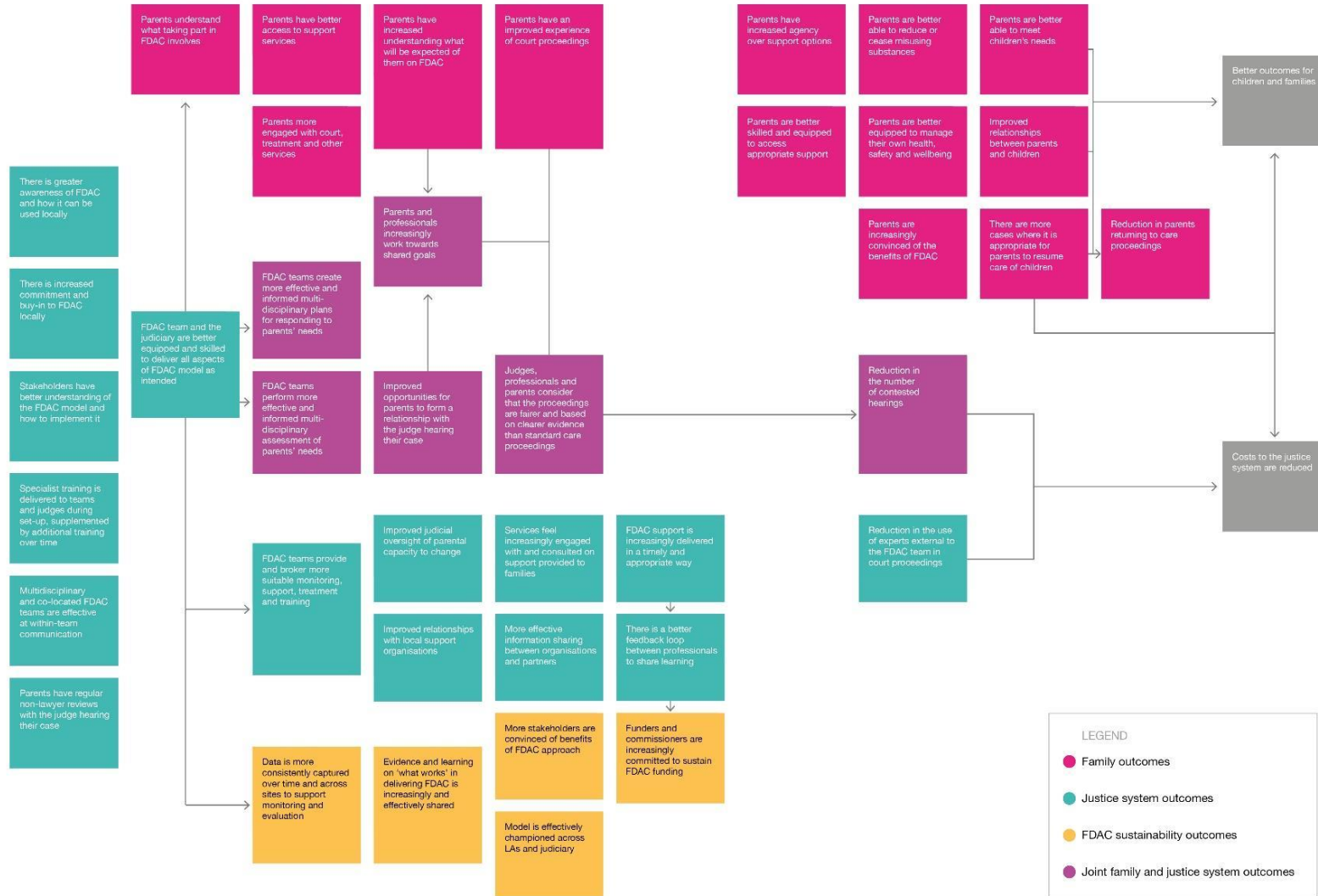
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Appendix A – FDAC Logic Model



Appendix B – FDAC Theory of Change



Appendix C – IPE data collection

Table 8 Outcomes map for implementation and process evaluation		
Outcome	When is it appropriate to measure?	How can it be collected?
FDAC team and the judiciary are better equipped and skilled to deliver all aspects of FDAC model as intended	When FDAC sites are operational	Interviews with FDAC staff and judiciary
More effective and informed multi-disciplinary assessment and development of plans to meet parents' and children's needs	When FDAC sites are operational	Interviews with FDAC staff, children's services and judiciary
Judges, professionals and parents consider that the proceedings are fairer and based on clearer evidence than standard care proceedings	When FDAC sites are operational and parents have finished care proceedings	Interviews with FDAC staff, children's services, judiciary and parents (including those who may have experienced 'business-as-usual' care proceedings)
FDAC team provide and broker more suitable monitoring, support, treatment and training	When FDAC sites are operational	Interviews with FDAC staff, judiciary and parents
Improved judicial oversight of parental capacity to change	When FDAC sites are operational	Interviews with FDAC staff, judiciary and parents
Improved relationships with local support organisations and more effective information sharing between organisations and partners	When FDAC sites are operational	Interviews with FDAC staff
Better feedback loop between professionals to share learning	When FDAC sites are operational	Interviews with FDAC staff
Parents understand what taking part in FDAC involves and are more engaged with court, treatment and other services	When FDAC sites are operational	Interviews with FDAC staff, judiciary and parents
Parents have an improved experience of court proceedings	When FDAC sites are operational	Interviews with parents

Table 8 Outcomes map for implementation and process evaluation

Parents have increased agency over support options	When FDAC sites are operational	Interviews with parents
Parents better skilled and equipped to access appropriate support	When FDAC sites are operational	Interviews with FDAC staff, judiciary and parents
Parents increasingly convinced of the benefits of FDAC	When FDAC sites are operational	Interviews with parents
Improved relationships between parents and children	When FDAC sites are operational	Interviews with FDAC staff, judiciary and parents

Appendix D – Power calculation formulae

The formula used to estimate the power of the primary analysis are adapted from Dong and Maynard (2013). The MDES is calculated as follows:

$$MDES = M_{K(J-2)-g_2} \sqrt{\frac{\rho(1 - R_2^2)}{P(1 - P)JK} + \frac{(1 - \rho)(1 - R_2^2)}{P(1 - P)JKn}}$$

Where:

- ρ is the intracluster correlation;
- R_1^2 and R_2^2 is the variance explained at level one and level two respectively;
- n is the average number of children per family
- J is the average number of families per Local Authority
- K is the number of Local Authorities

The multiplier, $M_{K(J-2)-g_2}$ is calculated as follows:

$$M_{K(J-2)-g_2} = T_1 + T_2$$

Where T_1 (precision) and T_2 (power) are drawn from the inverse students' t-distribution as:

$$\begin{aligned} T_1 &= \alpha, K(J - 2) - g_2 \\ T_2 &= 2\beta, K(J - 2) - g_2 \end{aligned}$$

Where g_2 is the number of covariates at level two, alpha is the type one error rate and beta is the type two error rate. We then convert the MDES to an odds ratio:

$$OR = (MDES * \frac{\pi}{\sqrt{3}})^e$$

The relative risk ratio is then estimate using the prevalence of the outcome in control, $Prev_c$:

$$RRR = \frac{OR}{(1 - Prev_c) + (Prev_c * OR)}$$